AMERICAN SCIENTIST

JUNE, 1965

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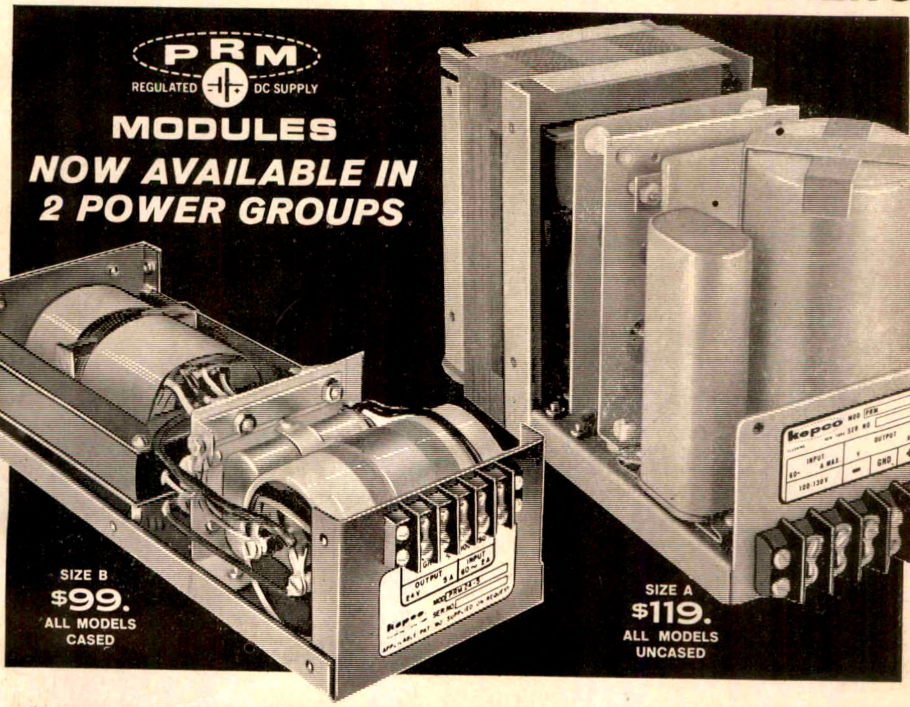

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PRM 36–5	36	0–5	0.8	1.3	0.4
PRM 48–4	48	0–4	1.0	1.8	0.3
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PRM 6–15	6.3	0–15	0.5	0.7	0.4
PRM 12–10	12	0–10	0.6	1.0	0.4
PRM 18–6.7	18	0–6.7	0.8	1.3	0.3
PRM 24–5	24	0–5	1.0	1.7	0.3
PRM 28–4.3	28	0–4.3	1.2	2.0	0.3
PRM 36–3.3	36	0–3.3	1.5	2.4	0.3
PRM 48–2.5	48	0–2.5	1.8	3.1	0.3
PRM 60–2	60	0–2	2.3	3.8	0.3
PRM 120–1	120	0–1	4.3	7.3	0.3

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AMERICAN SCIENTIST

VOLUME 53 JUNE 1965 NUMBER 2

PUBLISHED IN THE INTEREST OF SCIENTIFIC RESEARCH

CONTRIBUTORS

O. A. BATTISTA, *Colloidal Macromolecular Phenomena* 151

Born in Ontario, Canada, and an Honors Graduate in Chemistry of McGill University, Montreal, Dr. Battista's research activities have been successively with Courtaulds (Canada) Limited, the Pulp and Paper Research Institute of Canada, American Viscose and, most recently, with FMC Corporation in Princeton, N. J. He is Manager of Interdisciplinary Research in the Central Research Laboratory of the Corporation. The author of numerous books and articles for national magazines, a holder of domestic and foreign patents, his research in cellulose chemistry led to Avicel microcrystalline cellulose, a new product in commercial production by FMC Corporation at Newark, Del. Address: FMC Corporation, Princeton, N. J. 08540.

A REPORT BY AN AAAS COMMITTEE ON SCIENCE IN THE PROMOTION OF HUMAN WELFARE, *The Integrity of Science* 174

Responsibility for statements of fact and expressions of opinion contained in this report rests with the committee that prepared it. The AAAS Board of Directors, in accordance with Association policy and without passing judgment on the views expressed, has approved its publication as a contribution to the discussion of an important issue. AMERICAN SCIENTIST provides the medium for publication of the Report under the same conditions as are set forth by the AAAS Board of Directors.

Members of the Committee during the preparation of the report are: Barry Commoner, Washington University (Chairman); Robert B. Brode, University of California, Berkeley; T. C. Byerly, United States Department of Agriculture; Ansley J. Coale, Princeton University; John T. Edsall, Harvard University; Lawrence K. Frank, 18 Goden St., Belmont, Mass.; Margaret Mead, American Museum of Natural History; Walter Modell, Cornell University Medical College; William T. Kabisch, AAAS staff representative; and Gorman L. Mattison and Sheldon Novick, administrative assistants to the Committee.

Preprints and reprints may be obtained from Public Understanding Department, American Association for the Advancement of Science, 1515 Massachusetts Ave. N. W., Washington, D.C.

AMERICAN SCIENTIST is published quarterly (March, June, September, December) by the Society of the Sigma Xi. *Office of Publication:* Mack Printing Co., 20th and Northampton Sts., Easton, Pa.
Second-class mail privileges authorized at Easton, Pa. Additional entry at the New Haven, Conn., post office, December 9, 1943. This publication is authorized to be mailed at the special rates of postage prescribed by Section 132.122.

GARRETT C. CLOUGH, *Lemmings and Population Problems* 199

A visiting research fellow in the Department of Conservation of Cornell University for 1964–65, a Ph.D. in Zoology in the University of Wisconsin in 1962, here records his observation of lemmings and voles during a season in Norway, sponsored by the U.S. Public Health Service and the Norwegian State Game Research Institute. A graduate of Union College in 1953, his experience includes teaching and research assistantships in the University of Wisconsin and an assistant professorship in Dalhousie University, Halifax, Nova Scotia. As of June, 1965, Dr. Clough will take up an appointment as Assistant Professor of Zoology, University of Rhode Island, Kingston, R. I.

NORMAN H. CROMWELL, *Chemical Carcinogens, Carcinogenesis and Carcinostasis* 213

Regents' Professor and Chairman of the Department of Chemistry in the University of Nebraska, Lincoln, Nebr., 68508, a Sigma Xi-RESA National Lecturer for 1963–64, Dr. Cromwell has been interested in medicinal chemistry, especially carcinogenesis and cancer therapy, since World War II. He serves as a consultant to the pharmaceutical industry, the American Cancer Society and, since 1952, to the U.S. Public Health Service. A B.S. in chemical engineering, with honors, at Rose Polytechnic Institute, in 1935, he took his Ph.D. in chemistry at the University of Minnesota in 1939 and has been with the University of Nebraska since 1939, becoming Regents' Professor in 1960.

DAVID R. PILBEAM, AND ELWYN L. SIMONS *Some Problems of Hominid Classification* 237

Mr. Pilbeam received his A.B. degree in Natural Science and Anthropology from Cambridge University with First Class Honors with Distinction. For the last two years he has been at Yale University where he is at present a Sterling Fellow participating in the primate research program directed by Professor Simons with whom he co-authors this study. Now Associate Professor of Geology and Curator of Vertebrate Paleontology at Yale University's Peabody Museum, Dr. Simons holds degrees from Rice and Princeton Universities, and University College, Oxford. He has published widely on the evolution of vertebrates from pantodonts to primates, based on studies conducted on three continents.

C. M. SLIEPCEVICH, *Liquefied Natural Gas—A New Source of Energy*–Part I: *Ship Transportation* 260

A transfer student from Montana State College to the University of Michigan in 1939, a B.S. in 1941 and a M.S. in Chemical Engineering in 1942, Dr. Sliepcevich obtained his Ph.D. in 1948 after a period of years devoted to classified research during the war. In 1951, he became an Associate Professor of Chemical and Metallurgical Engineering at the University of Michigan, and, in 1955, became Professor and Chairman of Chemical Engineering at the University of Oklahoma at Norman, Okla. Since 1958 he has been Chairman of the School of General Engineering. In this Sigma Xi-RESA National Lecture, 1961–62 series, he describes equipment and process design for liquefaction and transporation of cryogenic fluids, notably liquefied natural gas.

SU-SHU HUANG, *Perspectives No. I—Life in Space and Humanity on the Earth, An Essay Review* 288

On leave of absence from Northwestern University for the academic year 1964–65, Professor Huang is serving jointly as Professor of Astrophysics at The Catholic University of America and as a research astrophysicist at Goddard Space Flight Center. In this essay review, he returns to the problem of *Occurrence of Life in the Universe* on which he wrote in AMERICAN SCIENTIST in 1959. His present research interests include the interrelationships among binary systems, rotating stars, and planetary systems. Address: Until September, 1965—Division of Space Science and Applied Physics, The Catholic University of America, Washington 17, D.C. Subsequently, Northwestern University, Evanston, Ill.

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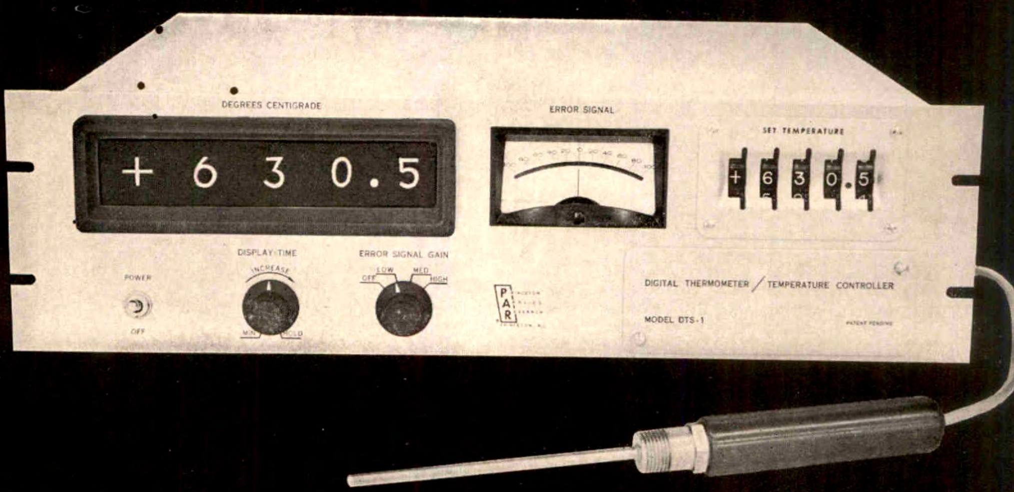

S. GLUCKSBERG, *Perspectives No. II—The Problem of Human Thought, An Essay Review* 299

In this second essay review, Dr. Glucksberg, an Assistant Professor in the Department of Psychology at Princeton University, and a Consultant to AMERICAN SCIENTIST, surveys three very different books in which the thinking process is central to each. A B.S. from City College of N.Y. and Ph.D. from New York University in 1960, he came to Princeton after a tour of duty as Captain in the U.S. Army, where he directed a research section of the Human Engineering Laboratories at Aberdeen Proving Ground. His current researches are concerned with memory, thinking, and the development of communication processes in children, this latter research in collaboration with Dr. R. M. Krauss of Bell Telephone Laboratories.

CHARLES E. PEPPER, *A New Way to Teach Science in Secondary Schools* 163A

A former *New York Times* man, Charles E. Pepper earned his A.B. at Antioch College and his A.M. at Columbia University. He is at present employed in the Public Information Department of Princeton University. He was formerly editor and publisher of *The Crittenden Press*, a weekly, in Marion, Ky. His present article is reprinted, by permission, from *University: A Princeton Quarterly*, Spring, 1965 © by Princeton University and records an experiment in science teaching by Wayne Nelson at Princeton High School and in 59 other centers in eight regions of the country. The course "Time, Space, and Matter" is being tested under the supervision of the Geology Faculty of Princeton University with our Associate Editor, Dr. Sheldon Judson, as chairman of the project's advisory committee.

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Society of the Sigma Xi and the Scientific Research Society of America, RESA

EDITORIAL COMMENT

AMERICAN SCIENTIST extends its congratulations to the Immediate Past President of The Society of the Sigma Xi, Dr. Frederick D. Rossini, Dean of the College of Science and Associate Dean of the Graduate School in the University of Notre Dame, on the recent award to him of the Laetare Medal bestowed annually by his university on distinguished Americans. Only very occasionally has this honor been given to a member of the University faculty so that the selection of Dr. Rossini for this year's award constitutes an outstanding tribute to one who has served science, government, and education so notably over more than thirty years. We recall the opening remark of our new President, Dr. Farrington Daniels, at the Convention of the Society in Montreal, "I express the deep appreciation of the whole Society of the Sigma Xi for the outstanding leadership which he has given to us during the past two years."

The Board of Editors is pleased to announce the appointment of Dr. Sam Glucksberg, Assistant Professor in the Department of Psychology, Princeton University, as a Consultant in succession to Professor Richard O. Rouse, Jr., Chairman of the Department of Psychology at Williams College, Williamstown, Mass. To Professor Rouse we extend our grateful thanks for the imagination and industry which he displayed, advising as to suitable reviewers of books received in the area of psychology and securing for us important articles in the field. Dr. Glucksberg has already been serving energetically in assigning new books to reviewers, in advising the editorial board on articles submitted for publication, and also as a contributor to this issue of AMERICAN SCIENTIST in the second essay-review, in PERSPECTIVES, of three books dealing with the problem of human thought. The presence of Dr. Glucksberg in Princeton facilitates the work involved in assigning new books to reviewers. His appointment as a Consultant gives the Board of Editors, drawn from other sciences, the benefit of his experience in psychology.

The Editor-in-Chief, at the March meeting of the Board of Governors of RESA, offered to the Board a page in each issue of AMERICAN SCIENTIST, to record, for the membership of the Scientific Research Society of America, any items of immediate interest or concern. We print, in a later page of this issue, the first RESA Page, provided by the Director, Dr. Donald B. Prentice.

We expect to continue, in each issue, the Executive Secretary's Page recording items of interest to the Sigma Xi membership. The Chapter-at-Large plans to keep its membership informed and up-to-date on election and promotion of individuals to membership by the Chapter-at-Large and on matters of particular interest to the membership of this, now the largest chapter in Sigma Xi.

In the September issue of AMERICAN SCIENTIST we expect to present the National Lecturers for the Fall Series of 1965–1966, with brief biographies and portraits of the lecturers. Abstracts of the lectures will also be supplied. In the December issue a similar report on the Spring Series of Lectures will be available. This new program was authorized by the Executive Committee of Sigma Xi at its Spring Meeting, 1964, the action taken being recorded in the June 1964 issue of AMERICAN SCIENTIST.

EXECUTIVE SECRETARY'S PAGE

The development of a computer system at National Headquarters is proceeding on schedule. It is expected that, on or about September 15, membership lists prepared by the computer will be furnished each chapter and club. These listings will consist of the membership record and present address for each Member and Associate Member who has indicated his or her desire to be active with a particular chapter or club for the 1965–1966 academic year.

It is planned to have the December issue of AMERICAN SCIENTIST present to the membership a complete description of the new system for the maintenance of membership. Prior to that presentation, however, it is necessary to explain two features of the new method which will require the immediate assistance and cooperation of the membership.

The Post Office has directed the Society to Zip Code, without delay, its address lists for mailing AMERICAN SCIENTIST. This is under process and it is hoped to have the September issue of AMERICAN SCIENTIST mailed in accordance with Zip Codes. If your address label for that issue is incomplete or incorrect it would be appreciated if the actual label with correction could be returned to National Headquarters. In order to be compatible with the new system, names and addresses must be limited to a fixed number of characters—letters or numbers. Please note new form of Notice of Address Change, page 158A.

A second feature essential to the new operation is the assignment of permanent member numbers. Much confusion has resulted in the past from members having the same names. In fact, in one case there are five members with the identical name. Not only is the number necessary to identify, it will be used to address the computer and thus is essential to the entire operation. It will appear on all address labels for AMERICAN SCIENTIST and will be imprinted on the annual membership card furnished.

It is planned to use an eight digit number for each Member or Associate Member. The first two will indicate the year the individual was elected to the Society for the first time (either as an Associate Member or as a Member). The next three digits will specify the chapter which elected him and the last three will merely enumerate.

Since the number is based on membership data, it is imperative that missing information be obtained without delay. Requests are now being mailed to individuals and to chapters for help in those cases where the records at National Headquarters are questionable or incomplete.—T.T.H.

ANNOUNCEMENT

As Chairman of the Committee on Membership-at-Large, it is my pleasure to anounce that under the provisions of Article VII of the Constitution, the Chapter-at-Large has *Promoted* to full membership in the Society:

COLVERT E. CUSHING, JR.
ROBERT H. MOUNT
SUSAN WILLIAMS ROCKFORD
ROY S. YAMAHIRO
JOSEPH STALLARD WATERHOUSE

Also, the Chapter-at-Large, with the approval of the National Executive Committee, has *Elected* to full membership in the Society:

JEAN BATCHELOR ALLISON
WILLIAM E. KELLER
ALFRED MICHAEL POMMER
GERRIT W. H. SCHEPERS

JAMES H. MARKS, Chairman
Committee on Membership-at-Large

CHANGE OF ADDRESS

If your name or address has changed from that on file at headquarters at Sigma Xi or RESA, please return the form below to:

The Society of the SIGMA Xi
Office of the Executive Secretary
51 Prospect Street
New Haven, Connecticut 06511

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- ☐ . . . . Chapter
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Last Issue Received . . . .

GRANTS-IN-AID OF RESEARCH

Over the past twelve years the Society has been able to increase the magnitude of its support of the Grants-in-Aid of Research Program by almost 1000%, yet the requests in number and amounts of assistance needed have been growing at an even greater rate.

The Executive Committee, in consideration of the voluntary contributions received in 1964, the earnings on its endowed funds, and the necessary expenses for the Society's operations in 1965—allocated $75,000 for Grants-in-Aid for this year.

At the first meeting of the Grants-in-Aid of Research Committee for 1965, $37,375 was awarded to 84 recipients. At the second meeting in March, $38,334 was awarded to 105 recipients. It was necessary to deny completely 26 requests for assistance at this last meeting, while 20 were so denied at the first meeting.

In response to urgent appeals from the Committee on Grants-in-Aid of Research, the National Officers and Members of the Executive Committee are exploring ways and means to render additional financial support to this most important activity of Sigma Xi.

Without this commitment of the additional funds, no further meetings on awards for 1965 have been scheduled. Letters have been written to more than 150 individuals whose applications were pending for this meeting, advising them of the situation.

It is hoped that with the conversion of the National Headquarters to the computer system, which will make possible the National billing, voted by this past year's Convention, additional support of the program may be received from the entire membership comparable to that which is now given by the Chapter-at-Large.

HARLOW SHAPLEY
Chairman
Committee on
Grants-in-Aid
of Research

A LETTER OF INTEREST

Dear Professor Holme:

In June of 1963, the Society of the Sigma Xi was kind enough to award me a grant of $450 for our research project dealing with the synthesis of inorganic binary compounds using exploding wire techniques. This research has resulted in a paper in *Exploding Wires*, Volume III. edited by Chace and Moore, Plenum Press, 1964.

Again I wish to express my deep appreciation to the Society of the Sigma Xi for the financial assistance which allowed us to initiate this research project. You may be interested to know that the Petroleum Research Fund of the American Chemical Society is now supporting this project to the extent of $14,000 for two years. We believe that the initial grant from Sigma Xi made this possible.

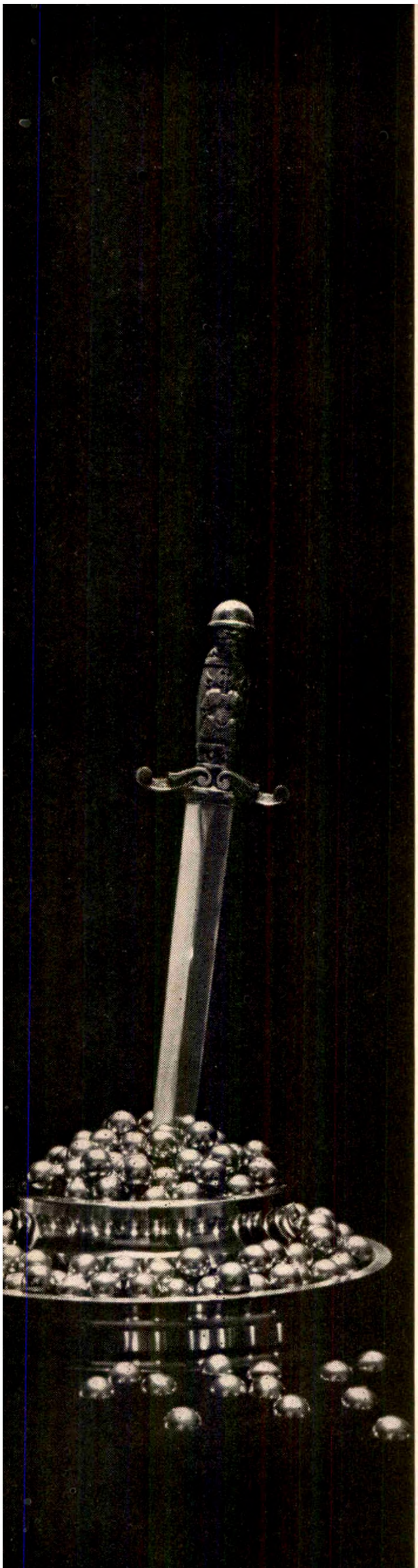
Sincerely yours,
MICHAEL J. JONCICH, HEAD
Department of Chemistry
Northern Illinois University
DEKALB, ILLINOIS 60115

RESA GOES OVERSEAS

The Scientific Research Society of America (RESA) was sponsored by Sigma Xi in 1947–1948 to operate in industrial and governmental research laboratories as Sigma Xi operates in degree-granting institutions. RESA's early years were financed by generous contributions from Dr. William Procter, and this support was made perpetual by a bequest of $100,000 in Doctor Procter's will.

The charter branch in RESA was installed in the spring of 1949 at the Esso Research Laboratories at Linden, N. J., when the Esso Sigma Xi Club voted to affiliate with the new Society. The Board of Governors of RESA, two-thirds of whom must be Sigma Xi members including the officers of the sponsoring society, decided not to use pressure sales methods to promote the new organization but to let growth depend on natural interest among the several research laboratories in the country. Notices of the availability of RESA were sent to the directors of all laboratories with professional personnel of fifty or more. Petitions for branches have been received at a fairly steady rate for sixteen years, and to date ninety-six groups have been approved. Total membership is over 12,000 with about 1500 members-at-large (not in branches or clubs). Doctor Procter's bequest, with some small additions from operating balances, has now grown to $240,000.

Several years ago a request was received from a Japanese scientist, who had done his graduate study in the United States, for a branch in Japan. Because of the distance from this country, the great amount of excellent research being carried on by the Japanese, and the large population of the nation, RESA's Board felt that the establishment of a society similar to RESA in Japan would be a better plan and offered our assistance and advice. Recently, a request was received from a group of scientists, many of them members of Sigma Xi, in San José, Costa Rica, for the establishment of a branch. Circumstances in this Central American country were entirely different from those in Japan, and the RESA Board unanimously and cordially approved the petition. Within a few weeks, therefore, RESA will become an international research society.—D.B.P.



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Marstressing is one of the key features of New Departure—Hyatt Division's new NDur line of bearings which boasts life at least three times the former rating when run on standard New Departure fatigue life tests.

Back of *Marstressing* are decades of GMR research in metal physics and residual stress.

The principle behind *Marstressing* is remarkably simple; but simple answers have a way of occurring most frequently where careful, persistent research has prepared the way.

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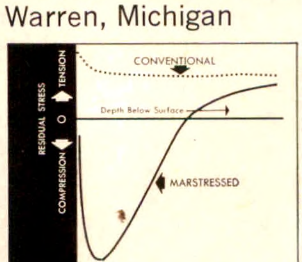

Comparison of subsurface stress patterns in conventionally hardened and *Marstressed* parts.

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AMERICAN SCIENTIST, *53*, 1965

A NEW WAY TO TEACH SCIENCE IN SECONDARY SCHOOLS

By CHARLES E. PEPPER*

Voices rose in dispute all over the room as small groups of high school freshmen wrestled with the concept of time.

"Time is time, that's all!" a girl at one table told her group.

"You're contradicting yourself," came from another table. "You're talking about a time before there was time."

"If there were no events to measure time, there'd just be no time," someone said.

One girl listened to an argument and wrote in her notebook: "If everything stopped all of a sudden, would time stand still? (This is philosophy—does not have meaning to *us*)."

This encounter between youth and the abstract took place as part of a revolutionary physical science course being developed by the Secondary School Science Project under Princeton University's Department of Geology. The class, taught by Wayne Nelson at Princeton High School, had counterparts in 59 other schools in eight different regions of the country where the course—called "Time, Space, and Matter"—is being tested.

It is unusual in several respects: (1) It has no textbook; students write their own. (2) Students discover the material of the course themselves instead of hearing it in a lecture or reading it in a textbook. (3) It isn't aimed at a specific age level. A student gets as much out of it as he can. It has been tried successfully as early as the seventh grade, and its developers believe it could be used with profit by college freshmen in some cases. (4) It is framed so that students can learn even if the teacher is not well trained in science and is not particularly talented.

The heart of the course is a student record book. As one explanation puts it, "The student is the principal investigator and what he does is the course. The notes of his observations and interpretations, which he records in a student record book, become his text." The student also receives a laboratory kit, a book-sized box full of simple equipment such as an equal-arm balance that doubles as a ruler, a magnifying glass, and a thermometer without a scale. From time to time he makes use of illustrated "Student Investigation Booklets" that furnish material for observations that he couldn't conveniently gather otherwise. One booklet, *Exploring a Slice of the Earth*, provides 28 large photographs of the Grand Canyon along with a map that can be used for an armchair field trip in quest of data on what made the canyon and how long it took.

Included also are several paperbound books for supplementary reading, along with one on the moon by Galileo and another on Project Apollo adapted from a book edited by Walter Sullivan of *The New York Times*. And there are special materials for the teacher: course description books, folios giving basic scientific background on different aspects of the course and practical hints by teachers who have used the course, classroom films, and even phonograph records of lectures by scientists and of classes in progress.

The essence of "Time, Space, and Matter" is discovery, but not accidental, hit-or-miss discovery. It is carefully

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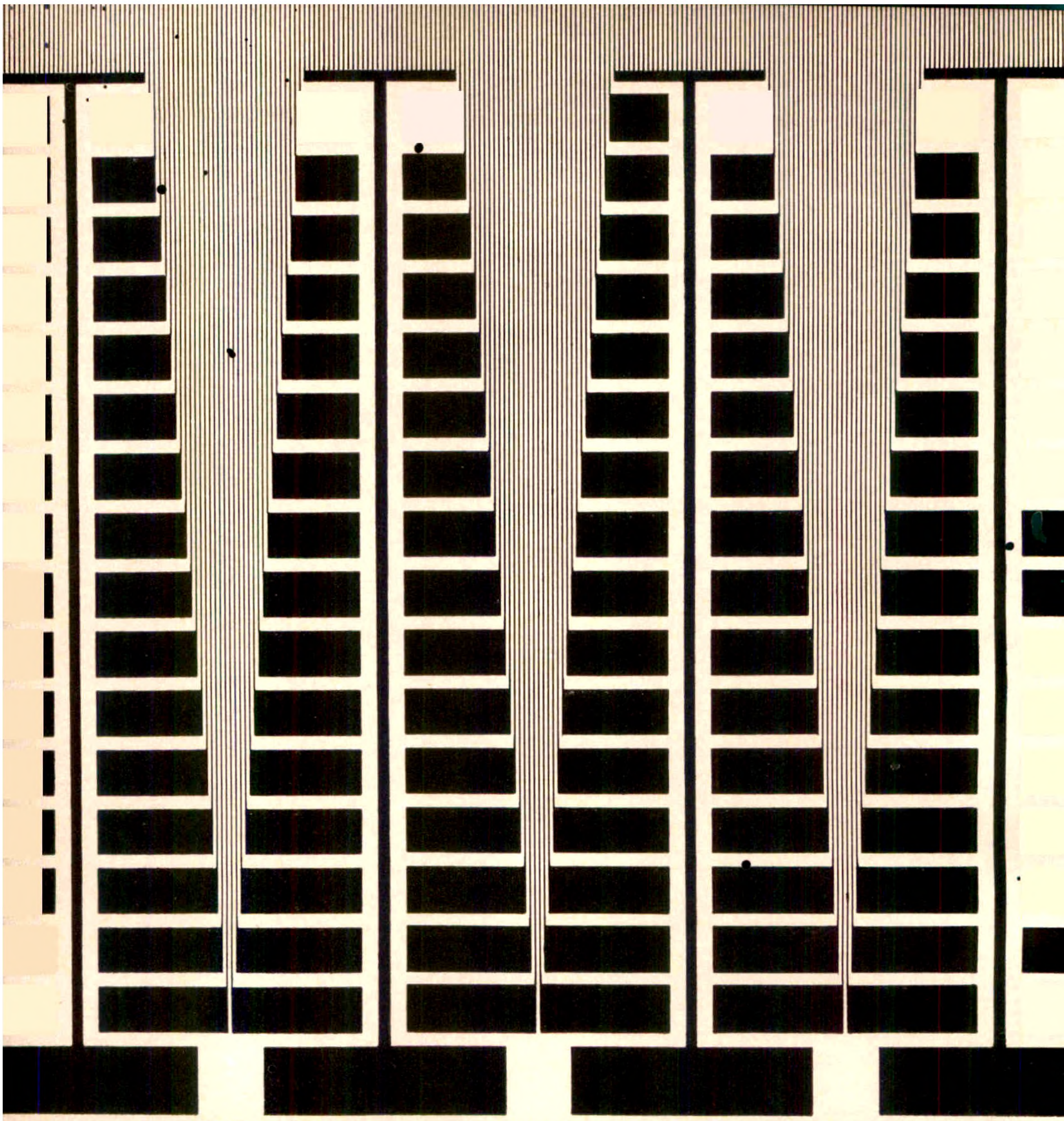


programmed through a series of investigations which the student carries out like a scientist, systematically recording observations and drawing conclusions. The investigations are arranged in a sort of spiral order, returning to a problem that previously seemed insoluble after the student has developed techniques for dealing with it.

There are three cycles of investigations, each logically arising from the one before. One, two, or all three series can occupy an academic year, depending on the time available and the students' ability. Each series leads to a climax that would be a satisfactory ending point for the year's work.

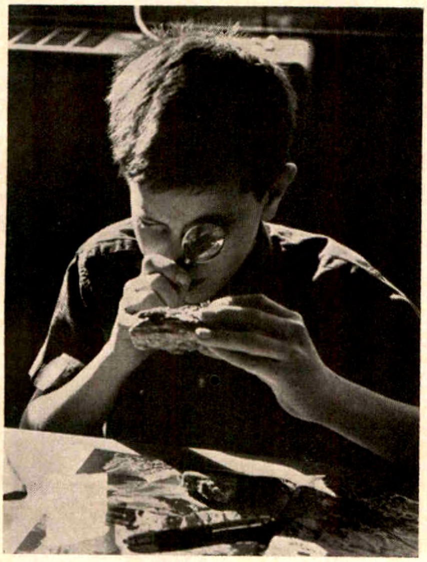


Fig. 1. Student examines the Grand Canyon, or a small chunk of it.

The first cycle is called, with a bow to Lucretius, "On the Nature of Things." It begins with a question: *How can one begin a study of the physical world?* With so many things around—tables and chairs, trees and rocks, even the moon—students find it hard to decide where to start. At length someone proposes starting with, say, the moon—a single thing that can be observed all at once.

The students look at the moon—unaided, or if possible with a small telescope—and record what they see. A booklet provides a series of photographs, including views of the moon as it looked to Galileo through his telescope, and the Ranger 7 photographs bringing it as close as 3000 ft. But even these pictures don't make it clear whether the surface is hard or soft, powdery or crusty, much less what it is made of, and supplementary reading informs them that experts don't know much more about it than Galileo did 350 years ago.

From the moon, the class turns to a slice of the earth—the Grand Canyon. Along with the illustrated booklet the students look at rocks found at the top and bottom of the canyon. The material stimulates them to ask: *How big is the canyon? How did it get that way? How old is it?* They can speculate, but once again they find they lack the tools for reaching reasoned answers. So they turn to a simpler starting place: an experience dramatizing the difference between observation and interpretation. The students are shown two clear glasses filled with what looks like water. But an ice cube floats in one liquid and sinks in the other. Student record books show that some children don't notice the paradox and that others record it but apparently aren't bothered by it. Later, the students discover by feeling, smelling, and so on that the liquid in which the ice sank was alcohol, not water. They learn not to rely on their sense of sight alone and to distinguish between observation and interpretation.

Next they are asked to record as many observations as they can about a glass of water in which an ice cube is floating. Most surprise themselves with the number they produce. But if they observe that the water gets colder as the cube melts, they are unable to tell how much colder.

Here the unmarked thermometers come in. The students discover that they need a common reference point and a standard, and thus have their first contact with ideas of instrumentation and measurement. They also become aware that the thermometer supplements the sense of touch and

that, in general, instruments only extend or add precision to the senses.

At this point they are shown pictures of ice floating on water at the North Pole, taken from a nuclear-powered submarine. They quickly see the relevance of their observations of an ice cube in water—a microcosm—to the world at large—the macrocosm. Other pictures, the account by Walter Lord of the sinking of the Titanic in *A Night to Remember*, and an adaptation of Thomas Huxley's *On a Piece of Chalk* provide more material in this vein.

Now the class returns to the moon. Each student pretends that he is the first to land there. He is given a plastic tray divided into compartments filled with different materials representing samples of the moon's surface. He must collect samples and estimate the relative abundance of each material in his own "region of the moon." In addition, since the weight-lifting ability of the "Apollo space-craft" is strictly limited, he must, on his return trip to earth, leave behind some piece of equipment equal in mass to that of all his samples. This brings his simple equal-arm balance into play.

From this experience students learn that the greatest precision is not always the proper scientific goal. If a man on the moon spent all his limited time trying to determine with great accuracy the relative abundance of one specimen he would not get much information. He must find the *proper* degree of precision.

Later the students compare the mass of two "moon samples" of unequal heft, actually the minerals galena and calcite, using pennies, paper clips, and pins as counterweights. They begin to think in terms of some standard mass and are introduced to the kilogram. Each student makes his own 10-gram mass by filling a little plastic pill bottle with enough lead shot to balance an official-looking weight. But faced with the limitations of a single counterweight they begin experimenting with balancing the arm at other places than the exact middle and discover another principle: *the greater the mass, the smaller the distance.* This enables them to use their single standard to weigh a wide range of objects. They also determine the volume of their specimens by a water displacement method.

Returning to the macrocosm, the class assumes its data on galena and calcite to represent an approximate average of the density of the whole earth and combines it with information on the volume of the earth obtained from the orbital flight of Colonel John Glenn. The assumption seems to be borne out by the facts since the class obtains a value of 10^{28} grams—close to, or "of the same order as," the value given in scientific references.

Now the students take up another aspect of the world around them: change. Objects move about; leaves grow, turn and fall from the trees; new houses grow old. Everything in the world seems to be changing in one way or another. Investigating the agents of change on the earth's surface, the children create a miniature canyon by pouring water over a flat cake of a sand-clay mixture. The canyon has vertical sides and the amount of material removed about equals the amount deposited in a fan-shaped delta at the canyon's mouth.

But is there anything that doesn't change? To look into this question the class turns to a consideration of time, a vital element of the problem. The Princeton High School class described earlier was at this point in the course; it had about found that time seems to be measurable only in terms of regularly occurring events.

Back on the problem of change, the students find that materials such as quartz and calcite seem much more durable than ice or rock salt. But a test of these materials in an abrasion mill—a motor-driven container that tumbles rocks about in water at about the speed of a typical stream—shows that rocks, too, lose mass, but because they lose tiny particles by abrasion rather than because they melt or dissolve.

Now for the Grand Canyon, which stumped them at the beginning. Their topographic maps show that the Col-

orado River is central to the canyon system. Photographs indicate that the river carries pebbles. The experience with the abrasion mill points to pebbles in the river as the agency that created the canyon and gives them a typical rate of mass loss. The work in making a miniature canyon leads them to conclude that, if the effects of tributaries and other processes are disregarded, the canyon would have vertical walls and would be about as wide as the Colorado itself. This makes it fairly easy to calculate the volume of rock removed to make the canyon. The next step is to estimate the total mass of rock the river removes from the area each day and figure how long it would have taken at this rate to cut the canyon. They find that the answer is on the order of millions of years. Despite sources of error they have reached a figure that agrees with the estimates of geologists using other means and learn that the age cannot be calculated more accurately than to an order of magnitude.

The next series of investigations is called "Seeking Regularity in Matter." The students discover crystals growing in solutions of salt and sugar, then come to find that crystals are more common than they had supposed. They grow crystals of different sizes by varying the temperature and the rate of temperature change and find that each pure substance crystallizes in its own way. They also arrive at an idea that the matter that is crystallizing might be made of tiny particles arranging themselves in an orderly fashion. Thus they head toward an atomic theory of matter.

When the class tries to obtain a gaseous form of sugar by heating it, and instead obtains two completely different substances—carbon and water—it concludes that some pure substances are composed of other, simpler substances, and that there may be "particles within particles."

"Interpreting a World of Change," the final series of investigations, first presents the students with a "rock pile"—a collection of 23 rocks and minerals that make up 99 per cent of the earth's crust—and challenges them to organize it into some meaningful system. Applying what they have learned they separate pure substances, or minerals, from the impure rocks and arrange the rocks into the classes geologists know as igneous, sedimentary, and metamorphic. Later they find they are faced with conflicting evidence as to the mean density of the earth, leading them to make reasonable speculations about the existence of an earth core and its size and density. At the end they reach a conclusion about the minimum age of the earth by considering rates of erosion, accumulation of sediments, and the ever-recurring rock cycle.

Although the course seems fairly well set in the outline described here, Dr. George J. Pallrand, the project's executive director, said it was still being developed on the basis of "feedback" from the children, teachers, and administrators in the schools testing the course.

"One of the main criticisms of curriculum projects like this is that the courses take unusually able teachers," he said. "This is particularly true of mathematics, where you may have read many teachers are actually frightened of the new courses and textbooks that deal with the subject in a way they just don't understand. Aside from that, many people think that the discovery approach only works when conducted by an exceptional teacher. It is, certainly, different from the kind of course most teachers have taught.

"We feel that on the whole the course will work well in the hands of an ordinary teacher or even a poor one, although of course a great teacher can always do a better job. But the course stimulates the teacher as well as the student. We've had several reports from the schools about this.

"Up to now all the teachers who have used our materials have come to Princeton during the summer for a two-week orientation session. It's obviously impossible to continue doing this if the course is to be commercially feasible and self-sustaining. So now we are working on more auxiliary

teacher materials to go with the ones we already have. We plan to put them together in a package—we call it a 'do-it-yourself institute.' We hope this will equip even the teachers with extremely weak backgrounds in science to lead a class well.

"If all goes well, material for about a year's classroom work will be available for general use in the spring of 1966. At that stage the University will turn it over, with proper safe-

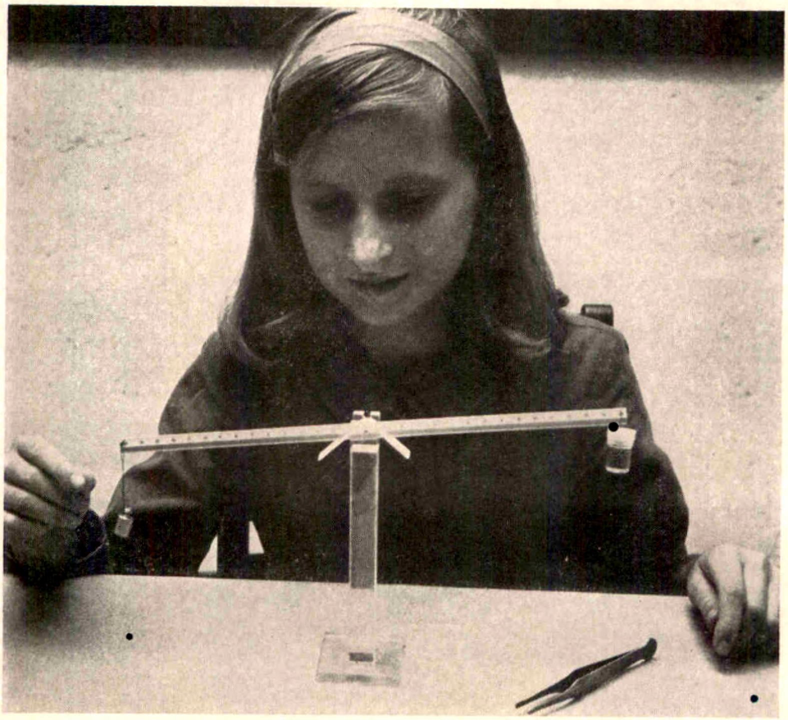


Fig. 2. Weighing is fun on a scale made from a ruler.

guards, to a private enterprise for production and promotion. Anything in the way of royalties will probably return to the federal government."

Distributing such different items as books and rocks to even the four thousand children now involved in the program presents quite a set of problems. The aim is a set of materials that is durable, simple, and inexpensive, quite different itself from the recent emphasis on flashy and costly teaching aids like models of planetary systems. Another member of the project's staff, James F. Murphy, deals with this aspect.

"Aside from the problem of designing the instruments, we have the problem of quantities," Mr. Murphy said. "On one hand, manufacturers aren't interested in making, say, the balance in quantities of just a few thousand. They want to talk about a million or so. The University shop shave been extremely helpful in making prototypes of these instruments for us. On the other hand, few dealers in minerals, even the largest, are big enough to supply what we want from stock, and so they need an unusual amount of lead time.

"We expect that the initial cost of all materials will run less than five dollars per pupil. The materials can be used several years and the schools will probably be able to offset some of the cost with federal funds."

The response at schools testing the course so far has been highly favorable, and word is spreading through

academic circles. The project reports that it gets an average of 15 requests for information a day. Dr. Pallrand said the project has had to turn down quite a number of requests from schools for the course because it already had as many schools as it could supply with materials and use for evaluation.

Examples of the response can be found almost at random from the replies of school administrators who recently were asked to comment on a new brochure describing the course. " 'Time, Space, and Matter' is a giant step forward in providing help for the traditional science teacher," one superintendent declared. "So far, we are very enthusiastic about the work done and feel that this is a real breakthrough in teaching children the spirit and methods of inquiry," wrote a California principal. "I found that the students are displaying an avidity for learning second to none that I have ever encountered," a New York principal said.

Princeton's Secondary School Science Project, directed by Frederick L. Ferris Jr., is one of 60 projects in elementary and secondary school science and mathematics supported by the National Science Foundation now or in the recent past. Some are only seminars of scientists and educators to discuss specific problems, others are ambitious programs to design coordinated curriculums for a broad range of ages, say the first nine grades. Together they form the bulk of the current ferment in American education.

A major cause of this ferment is the Physical Sciences Study Committee, which was formed in 1956 as a result of a general dissatisfaction among university physicists with the way physics was taught in secondary schools. The committee began with informal discussions stimulated primarily by Dr. Jerrold R. Zacharias of the Massachusetts Institute of Technology, evolved into an informal working group of interested physicists in the Boston area, and began formal operations on a national scale in November, 1956, under a grant from the N.S.F. to M.I.T. A year later the Russians put the first Sputnik in orbit, causing many other Americans to re-examine their schools.

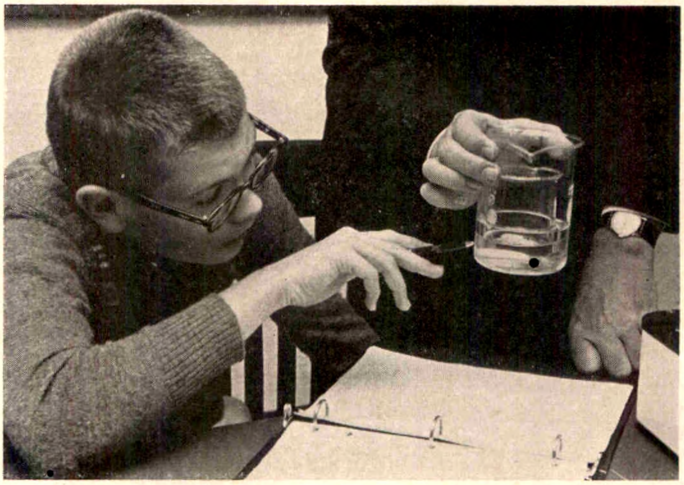
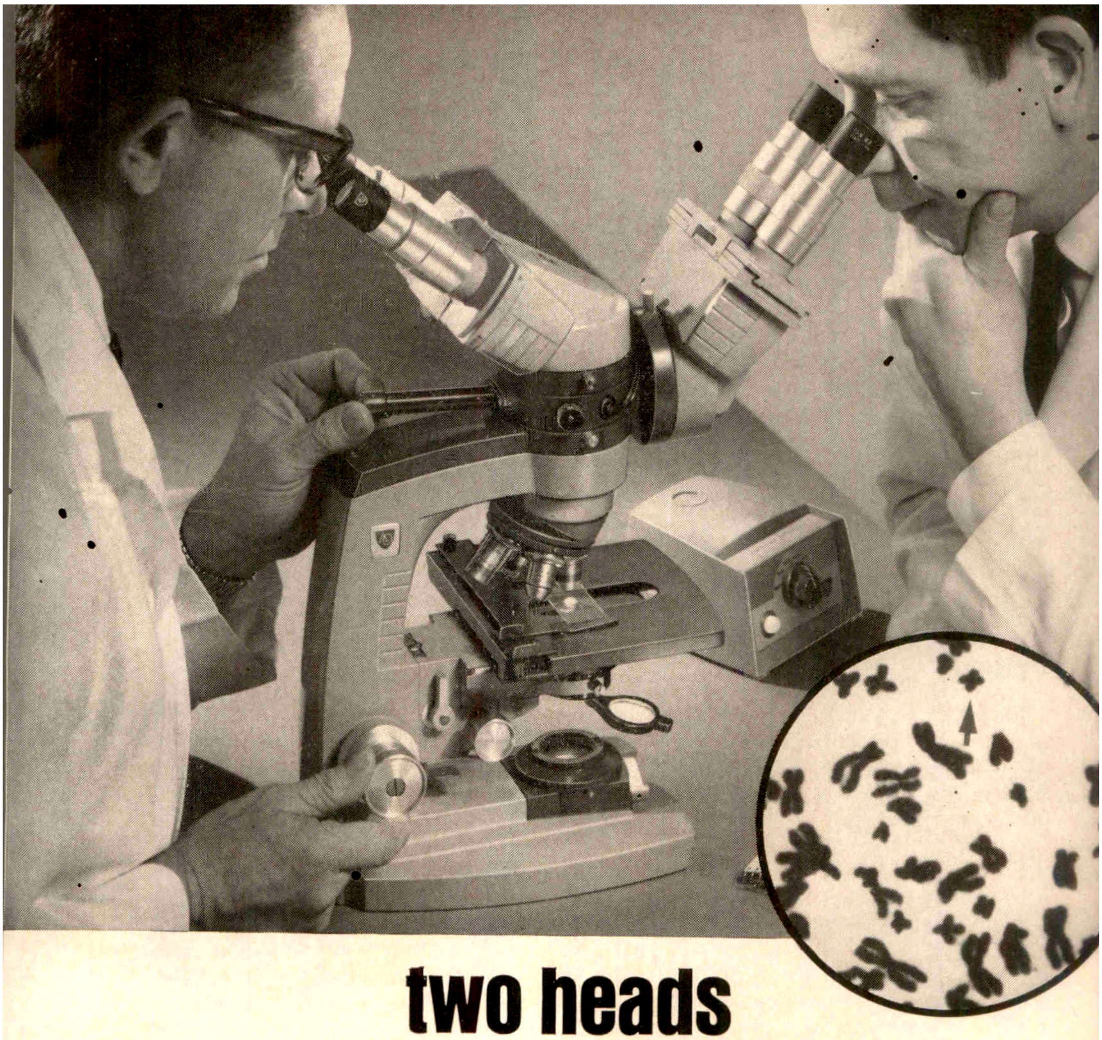


Fig. 3. Obviously more here than meets the eye!

The P.S.S.C.'s first fruits, a complete one-year physics course sharply different from anything available before, entered the schools in 1960. It was a tremendous success, and this success encouraged other scientists—biologists, chemists, anthropologists—to tackle similar programs in their fields, with the help of the N.S.F. Dr.



Zacharias himself is a member of the Princeton project's advisory committee and is reported to have a high regard for "Time, Space, and Matter."

Mr. Ferris, a Princeton graduate in geology in 1941, who once taught at Lawrenceville School, became associated with the P.S.S.C. at an early stage. In the summer of 1957 he received a leave of absence from the Educational Testing Service and took part in the group's first writing conference. Later, in helping the committee develop tests, films and books, he conceived the idea for a new kind of general science course.

"There seemed to be dissatisfaction with the general science courses available," he said, "so my idea was to provide one centered on geology but drawing on physics, chemistry, astronomy, and math as well. Jerome S. Bruner of Harvard, an expert on the process of learning, became interested along with others, and we worked up something and tried it out in three or four schools. At this point the Geology Department here became interested in sponsoring it, and we received our first N.S.F. grant in May 1963."

The project's advisory committee is chaired by Dr. Sheldon Judson, a member of the Geology Department faculty, and also includes Princeton's Dr. Hubert N. Alyea, Dr. Harry H. Hess, Dr. Heinrich D. Holland, and Dr. Aaron Lemonick. Dr. Hess, as chairman of the Geology Department, is responsible for the project's major decisions.

Further information about any aspect of this program should be obtained directly from: Jr. High School Science Project, Green Hall, Princeton University, Princeton, N. J. 08540.

LETTERS TO THE EDITOR

GENTLEMEN:

I have read with interest Dr. Maxwell's article—Will There be Enough Water?

He reports that a householder may pay thirty-five to forty cents per thousand gallons for water delivered to his home. This is very low when compared to 84 cents per thousand which I now pay. Then he writes that converting sea water to fresh water is possible at a high cost. This is now being done at 50 cents per 1000 gallons which again is low compared to my cost of 84 cents per 1000 gallons.

Could you assure me that Dr. Maxwell knew about these facts when he wrote his article?

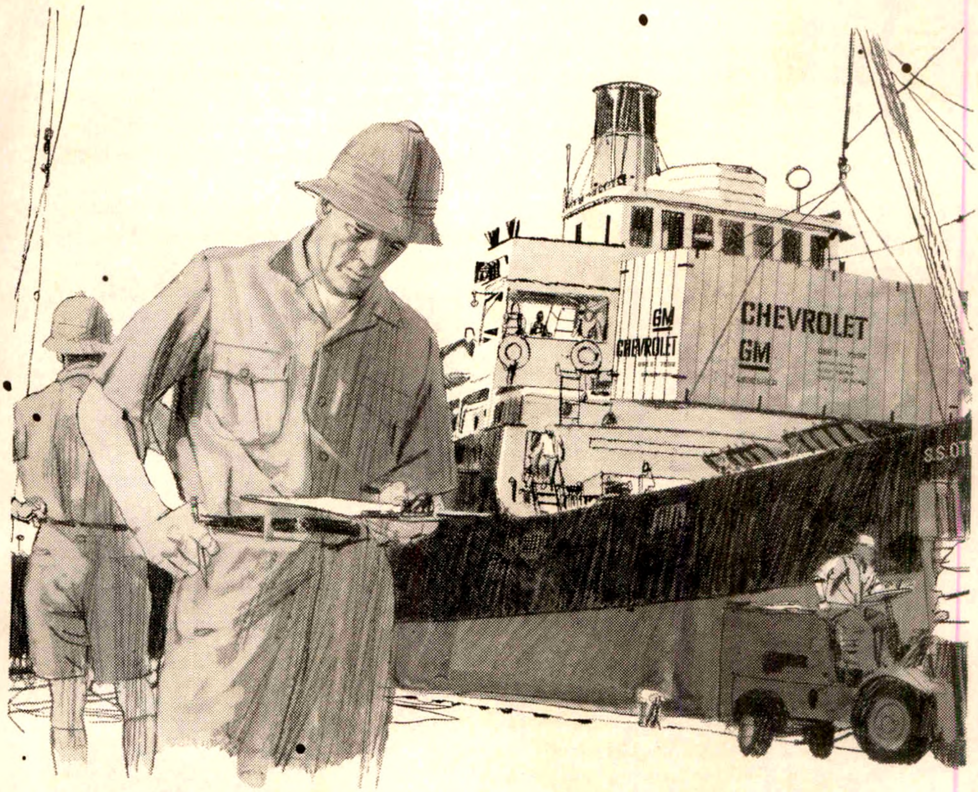
Yours very truly,
H. I. HOOT
301 Lafayette Ave.
Swarthmore, Pa. 19081

DEAR MR. HOOT:

One of the most frustrating aspects of the study of water is the difficulty of obtaining reliable cost data. The average price to householders quoted a couple of years ago was thirteen to twenty cents per thousand gallons. Last January, Mr. Raymond J. Faust, Executive Secretary of the American Water Works Association, wrote me that their organization conducts a survey of water utilities at five-year intervals. To the most recent survey, 1960, 1500 utilities replied, indicating that delivered water costs are in the neighborhood of 35 cents to 40 cents per thousand gallons. This is a bit higher than most of us pay in Princeton, but I know there are many areas where costs are considerably greater.

The actual cost of water depends primarily on two factors; the cost of treatment (filtering, chlorination, etc) and the cost of distribution to consumers. Treatment costs vary from essentially zero for a city with a pure water supply, to some tens of cents for cities plagued with poor water requiring extensive treatment. I fear that costs will continue to climb as our supplies are further contaminated.

Published figures on the cost of desalting sea water also tend to be confusing. Virtually all newspaper accounts refer to pilot plants underwritten by the Office of Saline Water; the costs are operating costs only, and for water at the plant site. Plant construction, salt water waste disposal, and delivery costs are not usually included. Even so, I have not seen any actual cost figures as low as 50 cents per thousand. I would appre-



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ciate it if you could give me the source of this figure.

Thank you for taking the time to write. Letters such as yours are extremely valuable in keeping us abreast of current practices in this most interesting but complex subject.

Cordially yours,

JOHN C. MAXWELL
Chairman, Dept. of
Geological Engineering
Princeton University
Princeton, N. J. 08540

A Reply to Sir Willis Jackson

DEAR SIR:

Discussing the partnership between science and engineering, Sir Willis Jackson [1,2] points to its social implications. He closes his comments: "I wish we had a clearer concept of the kind of community we would like to see emerge with the aid of science and engineering."

I believe that Sir Willis, himself, provides a starting point for finding an answer to his question when he says: "We can no longer be good specialists unless we are also good non-specialists."

Such a role is consistent with Socrates' definition of an educated man. It is not at variance with Professor E. Hutchinson's "my deep love of science *and* my concern for human values" [3]. Aldous Huxley commented "If the problems of humanity could be thought about and acted upon within a frame of reference that has survival for the species, the well-being of individuals, and the actualization of men's desirable potentialities as its coordinates, these peripheral organizations (UNESCO, the food and agricultural organization,...) would become central. The subordinate politics of survival, happiness, and personal fulfillment would take the place now occupied by the politics of power, ideology, nationalistic idolatry, and unrelieved misery" [4].

The members of the scientific community must be willing to act as non-specialists, cooperate, and take a part in the moulding of our future rather than to remain withdrawn from human affairs on the one hand, or to pretend to the throne on the other.

If they can affirm their ability to act according to their beliefs, theories, and conjectures while holding to the concept: I am not my brother's keeper, but my brother's brother, then they can take courage from the Italian Renaissance and the Renaissance of Physics at the turn of this century.

We then need not be frozen to immobility by fear of our future, the possible dehumanization of man, or by the possible closing of our individual life spans as the Hollow Men "...not with a bang but a whimper" [5].

J. A. KNUDSON
545 Kingsley Avenue
Palo Alto, California

REFERENCES

1. Imperial College of Science and Technology, London.
2. "Poles and Zeros, *Proceedings of the Institute for Electrical and Electronic Engineers*, February 1964.
3. Professor Eric Hutchinson, "Technology is not enough," *Stanford Review*, March–April 1963.
4. Aldous Huxley, "The politics of ecology," Occasional Paper, Center for the Study of Democratic Institutions, Box 4068, Santa Barbara, California, 1963.
5. T. S. ELIOT, *Collected Poems, 1909–1935*, Harcourt, Brace and Company.

DEAR SIR:

Thank you for having my book, "Collision Phenomena in Ionized Gases," reviewed in the March 1965 issue of AMERICAN SCIENTIST. Naturally, I am interested in learning what the readers of my book think of it, and the comments of the reviewers are of particular interest and importance to me. The evaluation of my book by Dr. Malamud (page 68A) is phrased ambiguously, in my opinion. Would it be possible to obtain a clarification? In the last paragraph of his review, he states that "As a compendium of information for those working in the field, I cannot recommend the book too highly." I prefer to interpret this statement favorably, but does he mean that he cannot recommend the book *or* does he mean that he cannot recommend the book highly enough? I would appreciate any additional comment which you or Dr.

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Malamud might like to make on this point.

Sincerely,

EARL W. MCDANIEL
Professor of Physics
School of Physics
Georgia Institute of Technology
Atlanta 13, Georgia

DEAR DR. MCDANIEL:

Your letter of April 3 to Dr. Taylor of AMERICAN SCIENTIST was forwarded to me. I thank you for writing to point out the ambiguity in my review of your book, which I assure you was unintentional.

Your favorable interpretation of the phrase "I cannot recommend the book too highly" is the correct one. If I had not been so busy in the attempt to pay the book this deserved compliment in graceful language, I might have realized the other possible meaning of the expression I chose. But this is only the reason for making this error; there is no excuse.

I hope that the editors of AMERICAN SCIENTIST, to whom I am sending a copy of this letter, will be able to publish a clarification.

Sincerely,

HERBERT MALAMUD
Sperry Gyroscope Company
Division of Sperry Rand Corporation
Great Neck, New York 10020

Re: Chinese Characters

DEAR SIR:

The AMERICAN SCIENTIST has the honor of making the first grammatical error in my new language. On page 396A December 1964, the villain and heroine have been interchanged!

Yours sincerely,

A. E. R. WESTMAN
Director of Research
Ontario Research Foundation
43 Queen's Park Crescent East
Toronto 5, Canada

DEAR SIR:

On page 396A of your December 1964 issue, you presented some very novel and amusing interpretations of organic formulae. I am enclosing another example of the kind, which appeared in *Science* recently (29 January, 1965, page 508).

Determining a suitable "translation" for this ideogram was not easy. Having discarded several possibilities relating to stick-up men, Army doctors with hypodermics at the ready, and the like, I finally propose the most succinct: "En garde!"

Very sincerely yours,

CHARLES H. CHANDLER
9404 North Church Drive
Parma Heights, Ohio 44130

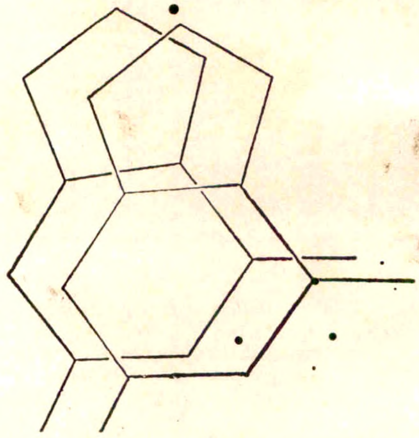

Fig. 1. Projection of a molecule of 8-azaguanine onto the plane of the molecule stacked below it in the crystal of 8-axaguanine monohydrate. The plane of projection is approximately the (102) crystallographic plane.

BOOKS RECEIVED FOR REVIEW

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific hardbound and paperbound books received for review in our Princeton Editorial Offices. Titles are for 1965 publication unless otherwise noted.*

From **Academic Press:**

Advances in Radiation Biology, Vol. I, edited by L. G. AUGENSTEIN, *et al.;* 285 pages; $11, 1964.

Concepts in Quantum Mechanics by F. A. KAEMPFFER; 358 pages; $9.75, 1964.

Fluctuation Phenomena in Solids, edited by R. E. BURGESS; 389 pages; $14, 1964.

Interpretation of Metallographic Structures by W. ROSTOKER & J. R. DVORAK; 226 pages; $10, 1964.

Lectures on The Many-Body Problem, Vol. II, edited by E. R. CAIANIELLO; 294 pages; $10.50, 1964.

The Role of Chromosomes in Development, edited by M. LOCKE; 290 pages; $11, 23rd Symposium of The Society for the Study of Development & Growth.

Physical Properties of Magnetically Ordered Crystals by E. A. TUROV; 222 pages; $10, 1964.

Physiological Mammalogy, Vol. II: Mammalian Reactions to Stressful Environments, edited by W. MAYER & R. VAN GELDER; 326 pages; $11.50, 1964.

Optical Properties of Minerals, A Determinative Table by H. WINCHELL; 91 pages; $5, 1964.

Advances in Quantum Chemistry, Vol. I, edited by P-O LOWDIN; 385 pages; $14, 1964.

Differential & Riemannian Geometry by D. LAUGWITZ; 238 pages; $8.50.

Methods in Carbohydrate Chemistry, Vol. V: General Polysaccharides, edited by R. L. WHISTLER; 463 pages; $16.50.

International Review of Forestry Research, Vol. I, edited by J. A. ROMBERGER & P. MIKOLA; 404 pages; $13, 1964.

Survey of Progress in Chemistry, Vol. II, edited by A. F. SCOTT; 345 pages; $7.95.

Fish as Food, Vol. 3; Processing—Part I, edited by G. BORGSTROM; 489 pages; $17.50.

Advances in Space Science & Technology, Vol. 6, edited by F. I. ORDWAY, III; 444 pages; $15, 1964.

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Frontispiece and FIG. 2, page 155. Microcrystals of cotton cellulose, 25,000×.

AMERICAN SCIENTIST, *53*, 1965

AMERICAN SCIENTIST

SUMMER · JUNE 1965

COLLOIDAL MACROMOLECULAR PHENOMENA

By O. A. BATTISTA

THE SCIENCE and technology of polymers during the past few decades have been centered largely around the phenomena and the products that result from precipitating or "freezing" into fixed matrices molecular dispersions of polymers to give important commercial structural shapes and objects comprising fibers, films, and plastics.

Actually, the evolution of the existing multibillion dollar polymer industries took place in two distinct steps; the results of both paths of growth continue to be important commercially. Firstly, a natural polymer, cellulose, was derivatized to make its constituent molecules soluble in a relatively inexpensive solvent. Such molecular dispersions were reconstituted into useful fibers and films; for example, cellulose nitrate dissolved in acetone was fashioned into the first "artificial silk," a hazardous yet fashionable fiber at the time. This was followed by dissolving cellulose xanthate in sodium hydroxide, and cellulose acetate in an acetone-water solvent, to give viscose rayons and acetate fibers, respectively, both of which remain very much in abundance and commercially prosperous. The second step occurred with the synthesis of man-made polyamide polymers by Wallace H. Carothers and his associates at DuPont, starting with the nylons in 1939 [20].

The thermoplastic synthetic nylon polymers, like their thermoplastic successors, reach molecular dispersions wherein individual molecules are mobile and capable of interacting in a physical sense. The polymer is carefully fused by heat in an inert atmosphere and the resulting melt is squeezed and shaped through orifices to give fibers, films, or shaped plastic objects. As the extruded melt is cooled—and usually stretched—its constituent long chain molecules aggregate and become locked in a three-dimensional web or matrix; the geometry or architecture of this matrix varies significantly with each thermoplastic polymer species. The final properties of such products are very much

FIG. 1A. Microcrystals of wood cellulose, 25,000×.

dependent upon: (1) the homogeneity or nonhomogeneity of the packing of the macromolecules during the precipitation, (2) "freezing," or after-treatment steps which a polymer system may be subjected to, and (3) other factors such as the chemical composition and the rigidity of the individual molecular chains involved.

Literally thousands of publications are available for characterizing "molecular" dispersions of the constituent long chain *molecules* in polymer products. Equally extensive effort has gone into studies aimed at understanding and measuring how individual macromolecules aggregate into clusters and arrange themselves into useful polymer

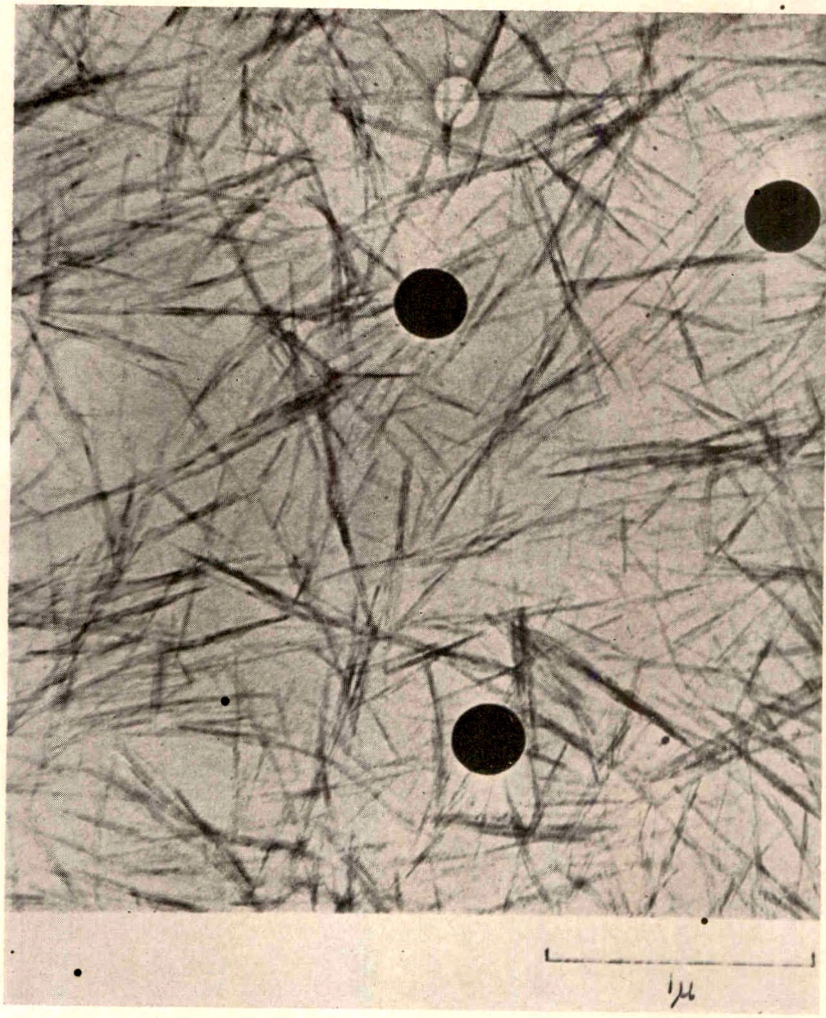


FIG. 1B. "Microcrystals" of colloidal alumina, 25,000×.

products or objects, both natural and synthetic.

This article concerns itself with "colloidal macromolecular phenomena," phenomena associated with discrete, highly crystalline aggregates of hundreds or thousands of long chain molecules, particles in general having a high length to diameter ratio, and being approximately 1 micron in their largest dimension, and at least 25 Å, more generally 50 Å or more in their smallest dimension. In effect, our definition of colloidal macromolecular particles encompasses aggregates of molecules which are joined by strong lateral bonding forces, in such a manner as to form truly discrete colloidal particles having dimensions

of the order of magnitude of polymer micelles, and molecular weights in the many millions. It is of special importance, of course, that the individual long chain molecules in each discrete particle are essentially locked in a "microcrystalline" matrix in an extended or linear state. In essence, one must recognize that long chain linear molecules "frozen" in a microcrystalline particle will occupy an effective volume quite different from the same molecules free to move about in dilute molecular dispersions.

There are at least two commercial products now available which depend largely upon colloidal macromolecular phenomena for their utility in a host of novel applications: (1) Baymal® colloidal alumina [10–15] and (2) Avicel® microcrystalline cellulose [3–9, 17, 18]. There is reason to believe that these two products are the forerunners of many new products to come, new products which will be fashioned from *both* synthetic as well as natural high polymers, from *organic* as well as *inorganic* high polymers.

The remarkable similarities in properties between these two colloidal macromolecular products—despite their complete dissimilarities in chemical composition, drive home the focal point underlying the interest in and promise of colloidal macromolecular phenomena.

Our first similarity is in the dimensions of the basic, discrete micellar-type colloidal macromolecular particles. Figure 1A *and* Figure 1B show individual colloidal microcrystalline particles of cellulose and alumina, respectively.

The similarity in size and shape of these macromolecular colloidal particles is striking. What is even more striking, however, is that, despite the great disparity in chemical composition, despite the fact that one particle (Figure 1A) is 100% organic and carbohydrate in nature and the other particle (Figure 1B) is inorganic in nature, colloidal dispersions, creams, and gels of these materials possess similar rheological, physical, and functional properties. This is the nexus of the novelty and of the practical importance of reducing well-defined aggregations of high polymeric materials to truly colloidal dimensions, namely circular or rectangular rods or platelets of the order of 50 Å × 50 Å × >1000 Å.

The foregoing comments are all the more intriguing when we realize that the microcrystals of cellulose were isolated from a natural product, purified wood cellulose, with essentially the same geometrical dimensions as nature made them. The "microcrystals" of alumina or fibrils of boehmite, on the other hand, are synthetic particles prepared by the controlled heating of a basic aluminum salt solution. These colloidal particles are physically analogous to aggregates of linear, high molecular weight polymers.

The size and shape of the natural cellulose microcrystals are more or

FIG. 3. Microcrystals of cellulose recovered from rayon tire cord, 25,000×.

less fixed by nature. But wide variations in the dimension of such particles are possible by the appropriate choice of the natural raw material. Then, too, by using regenerated celluloses as the source of the microcrystals, an even wider selection of microcrystal sizes is possible [1, 2]. Table 1 illustrates this range [2]. Figures 2 and 3 are electron micrographs illustrating the wide spread in the lengths of microcrystals possible; microcrystals of cotton cellulose are compared with microcrystals from viscose rayon at the same magnification.

It has been well established that the level-off degree of polymerization of cellulose ($\overline{\text{D.P.}}$) is a reasonably accurate yardstick for measuring the length of the recovered microcrystals [2].

In the case of microcrystals of boehmite, or colloidal alumina on the other hand, because they are produced synthetically from a solution phase, the particle size and specific surface area of these macromolecular particles can be varied and accurately controlled. With this system, there is an opportunity to control several variables that are important in determining the final size and shape of the colloidal boehmite particles —variables such as the anion type, anion concentration, alumina concentration, and the time-temperature heating curve.

TABLE 1

VARIABILITY IN MICROCRYSTAL LENGTH IN NATURAL AND REGENERATED CELLULOSES

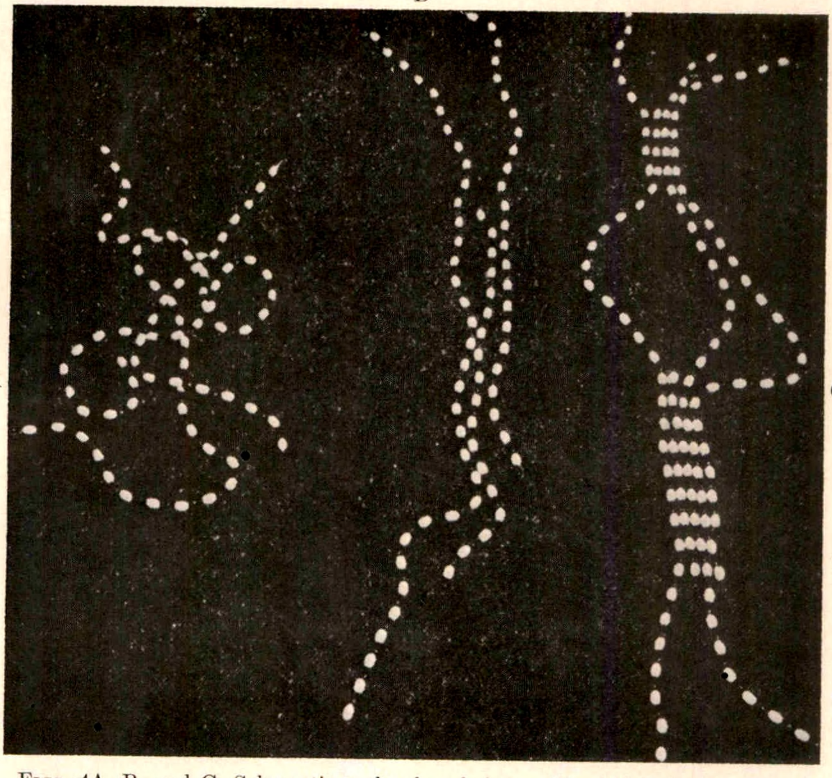
(Note: Level-off Degree of Polymerization $\overline{\text{D.P.}}$ is Directly Related to Crystalline Length) [2]

Form of Cellulose	*Average Basic $\overline{D.P.}$ Range* [a]
Natural fiber	
Ramie, hemp,	350–300
Cotton, purified	250–200
Unbleached sulfite wood pulps	400–250
Bleached sulfite pulps	280–200
Bleached sulfate wood pulps	190–140
Mercerized cellulose (18% NaOH at 20°C., 2 hr.)	90– 70
Vibratory-milled wood cellulose	100– 80
Regenerated fibers	
Fortisan, fiber G	60– 40
Textile yarns	50– 30
Tire yarns	30– 15
Cellulose acetates	
Combined HOAC	
62.0%	200–300
54.5%	Not measurable
29.0%	46
0.5%	35

Crystalline alumina can be regarded as "polymerized" alumina. By analogy with organic systems or the polymerization of the silica system, small units of aluminum—oxygen compounds present can be joined together to form relatively large molecular aggregates and micelles. In such alumina polymers, the ultimate units are joined by chemical bonds rather than by weak physical forces.

One facet of the hypothesis predicting novel functional properties of discrete colloidal macromolecular aggregates in the true colloidal particle size range (approximately >50 Å × >50 Å × >1000 Å) is that particles of such dimensions must be present in a discontinuous matrix of a polymeric object to begin with; if such particles are to be synthesized, then control must be exercised to permit the growth of the

microcrystals until they fall reasonably within the desired colloidal size range. In other words, the aggregation of polymer molecules to form discrete particles such as those shown schematically in Figure 4C is an essential prerequisite for both natural or synthetic polymers if novel colloidal macromolecular particles are to be recovered from their respective matrices in economical yields. We might put this in alternate



FIGS. 4A, B, and C. Schematic molecular chain models. (A) Polymer molecules in solution. (B) Pseudomorphology of polymer molecules in elastomers and (C) discontinuous crystalline morphology of polymer chains in fibers or films.

language and say that it would be highly unlikely that novel colloidal particles could be isolated in reasonable yields from an elastomeric polymer object lacking definitive crystallinity as schematically represented by Figures 4A and 4B. Rather, the formation of or use of fibers or films from a polymer of high enough molecular weight are prerequisites for the subsequent procurement of the desired discrete colloidal particles.

Almost all natural cellulose fibers are known to possess a particle matrix in which the macromolecules approach the configuration sketched in Figure 4C. A typical cellulose fiber, highly purified wood pulp or

fibrous cellulose recovered from spruce or hemlock trees, is an ideal example around which to develop this presentation of colloidal macromolecular phenomena.

If such wood pulp is beaten in water (the key step of all papermaking processes with cellulose fibers), very long strands or fibrils of cellulose

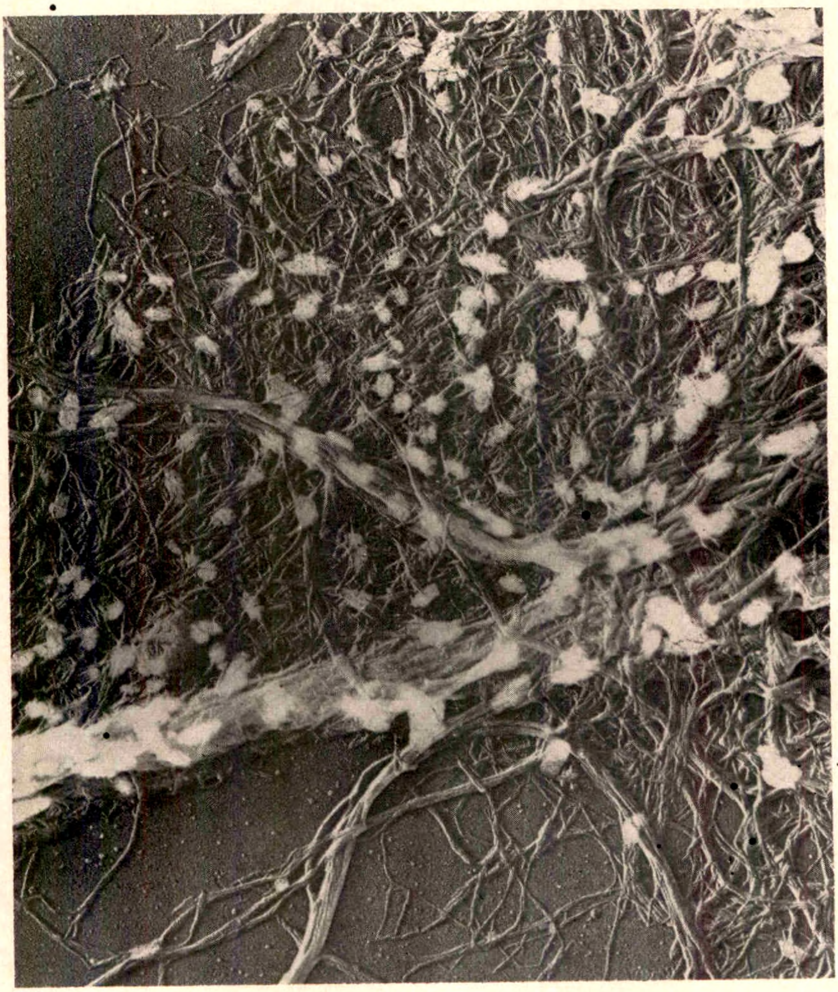


FIG. 5. Microfibrils of beaten wood pulp, 7,500×.

are formed. Figure 5 is an electron micrograph at 7,500× of such a sample of beaten wood pulp.

Within each long strand or fibril of cellulose shown in Figure 5 lie numerous microcrystals interconnected with each other by cellulose molecules, cellulose molecules which hinge these microcrystals tightly together. Schematically, Figure 6 serves only to illustrate how the same

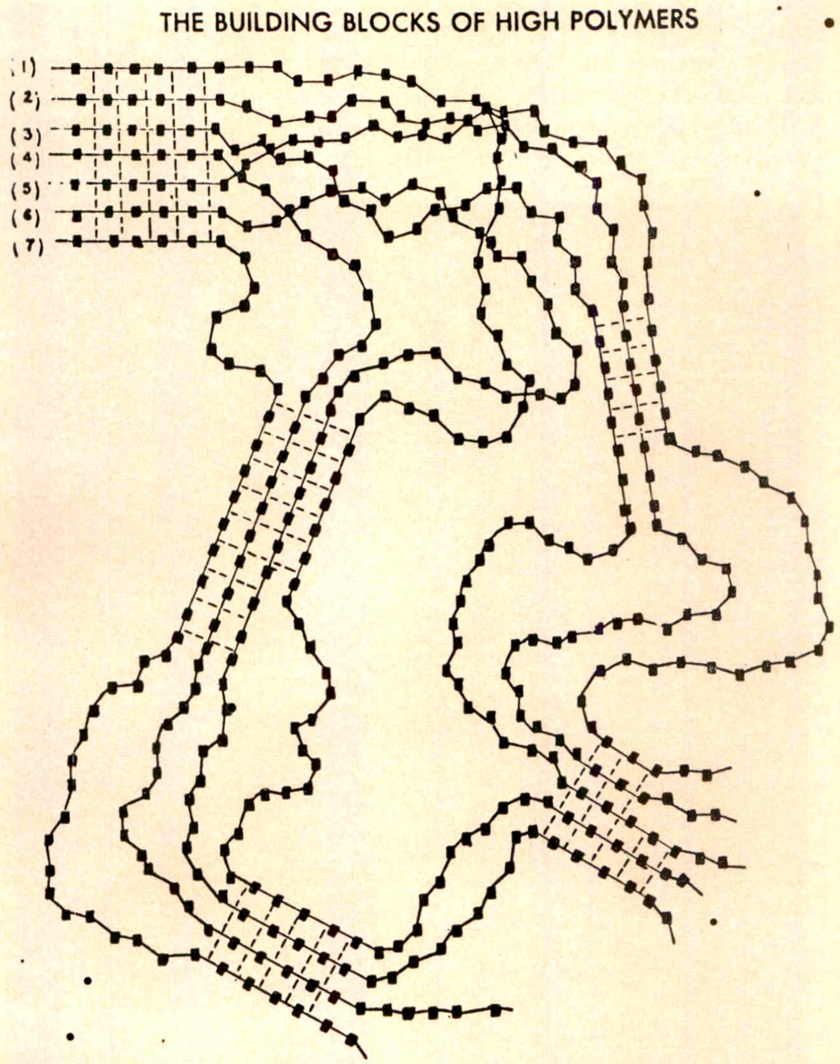


FIG. 6. Schematic illustration of polymer chains forming "hinges" between polymer microcrystals.

molecules of a polymer pass in sequence from one microcrystal (or discrete colloidal region) through several others. It is this type of "hinge morphology" which plays a predominant role in the usefulness of both natural and synthetic fibers and films. Without the "molecular hinges," without discontinuities or holes, cellulose fibers, for example, would be very brittle—and no doubt much less useful. A more realistic model, stemming from the alignment of the microcrystals in the fiber direction shown in Figures 8 and 8A has been proposed in which the cellulose chains are folded within each microcrystal [5].

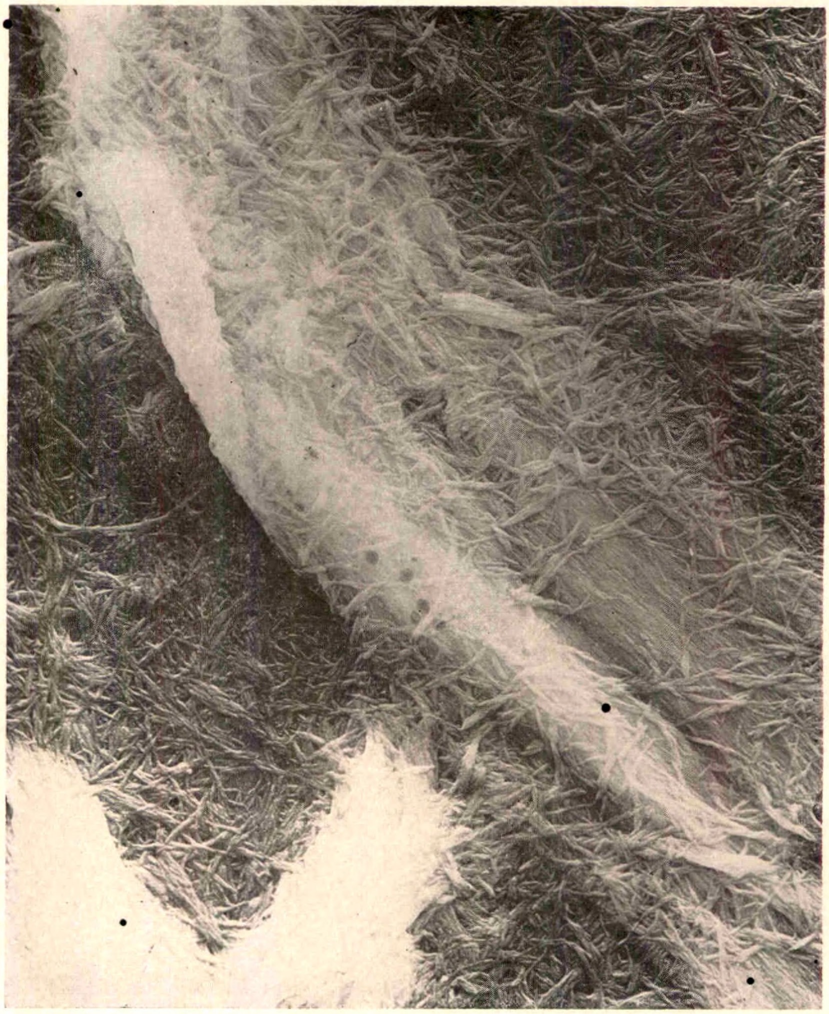


FIG. 7. Microcrystals peeling away from a fiber fragment of $\overline{\text{D.P.}}$ cellulose, 25,000×.

The microcrystals in cellulose fibrils are, therefore, hinged together by very strong forces because the molecules which weave in and out of them are connected by 1,4,β, glycosidic linkages, *true primary linkages*. Mechanical beating of cellulose fibers in water serves to expose more and more of the long fibrils shown in Figure 5, but such energy does little or nothing to cause an unhinging of the constituent microcrystals or their liberation from within their original particle matrix. It is almost impossible to introduce enough energy mechanically to break the millions of strong primary chemical linkages in the cellulose molecules within the hinge areas which tie the microcrystals together in a three dimensional network.

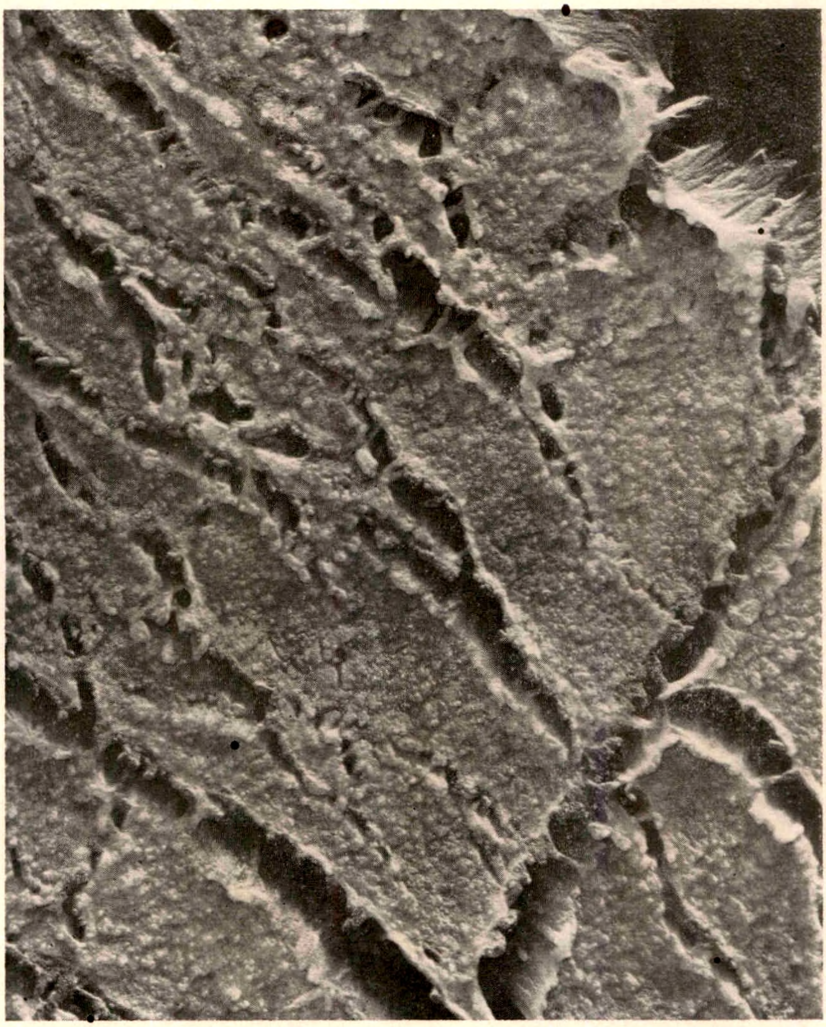


FIG. 8. Cross-section of a fiber-fragment of level-off D.P. cellulose, 50,000×.

Chemical energy almost always is more efficient—and cheaper—than mechanical energy. The exposed 1,4,β, glycosidic bonds in the hinge regions of the cellulose microcrystals are broken quite easily by the hydronium ion, H_3O^+, through the process of hydrolysis or the addition of water. The hydronium ion easily fits into the hinge regions of the cellulose fine structure, but is too large to attack the same linkages confined within the microcrystals.

In Figure 7, we see the striking manifestation of the power of chemical energy. A fibril of cellulose that has been acid treated (2.50 N HCl at 105°C ± 1°C for 15 minutes) is magnified (25,000×). This treatment

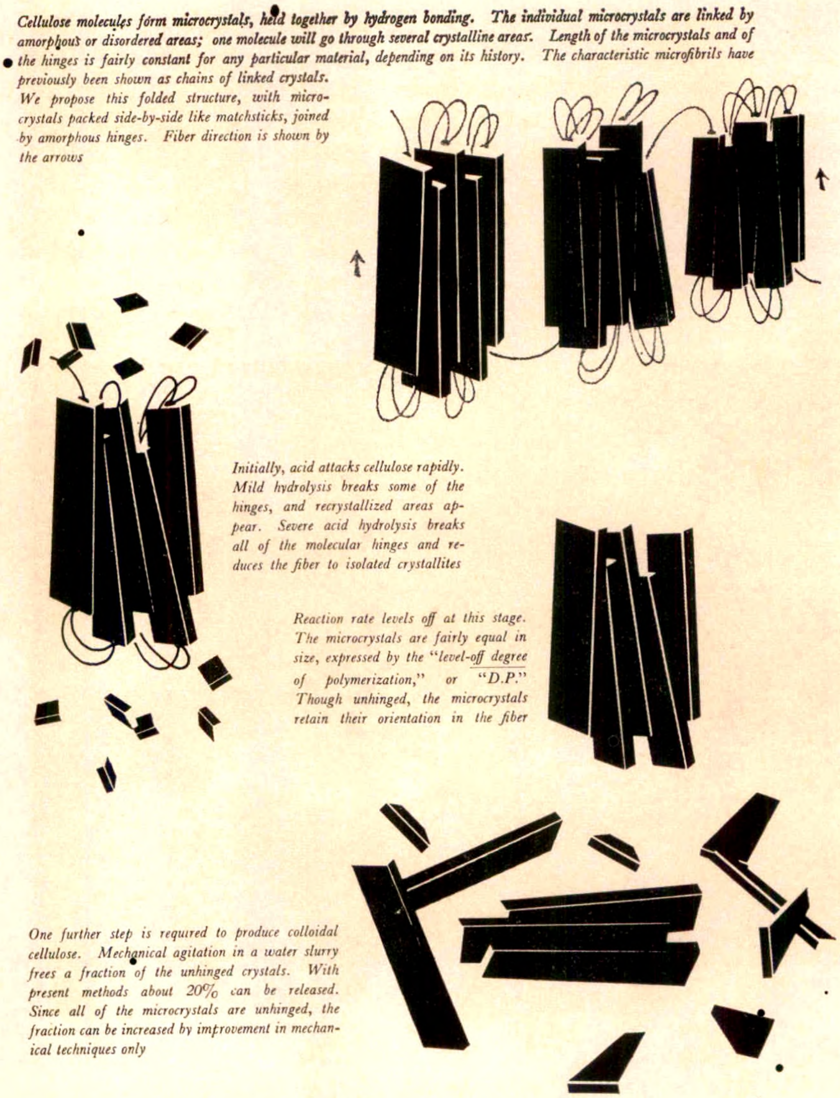


FIG. 8A. Structural model of cellulose microcrystals and formation of microcrystalline cellulose gels (5).

served to unhinge chemically all available primary linkages in the fibrous cellulose and reduce the cellulose to its Level-off Degree of Polymerization ($\overline{\text{D.P.}}$) [1], a state at which further severe acid treatment no longer breaks additional primary bonds at a detectable rate. When $\overline{\text{D.P.}}$ cellulose is beaten in water at about 5% solids in a mixer such as a Waring Blender or Osterizer, the fascinating phenomenon shown in Figure 7 occurs. The now-unhinged, discrete microcrystals of cellulose

begin to peel away from their natural positions. In Figure 7, it is important to note how, in the interior of the fibril, the microcrystals are lined up like well-packed matchsticks because they have not yet been dislodged from their original matrix. Gels will not form until a sufficient number of the microcrystals are dislodged from their natural, tightly-packed positions and are free to act individually.

A particularly unique microphotograph prepared by Frederick F. Morehead [21] is shown in Figure 8. Here we see a cross-section (original magnification 100,000×) of a single cellulose fibril fragment of $\overline{\text{D.P.}}$ cellulose. The packing of the unhinged microcrystals laterally is shown

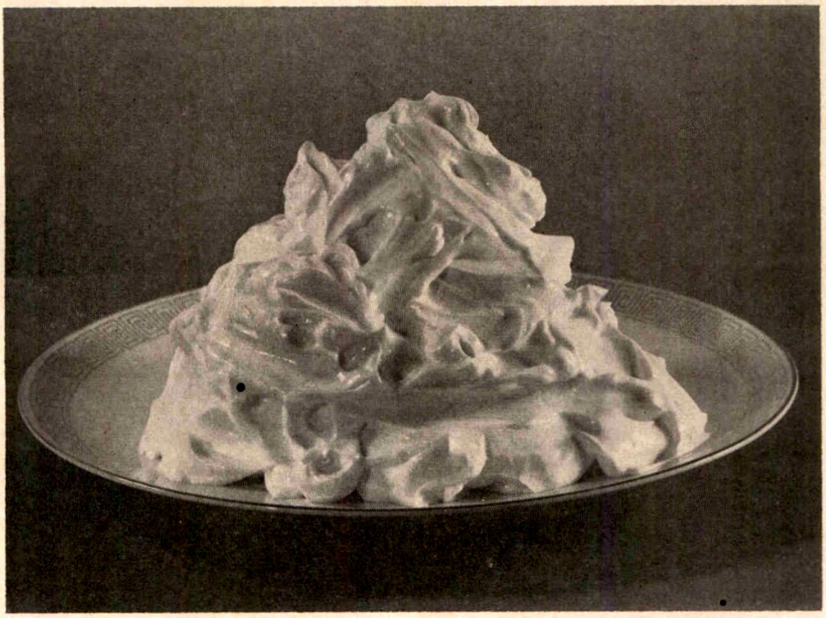


FIG. 9. Microcrystalline cellulose gel (14% solids).

clearly, the cross section being transverse to the normal fibril axis. It is the peeling away of the individual unhinged microcrystals *in sufficient numbers and at a high enough total solids content* that converts the microcrystalline cellulose into a novel state of colloidal macromolecular matter.

As the unhinged microcrystals (approximately 50 Å × 100 Å × 3000 Å) are dislodged from the cellulose fibrils and migrate into the surrounding water in the blender, the impact of colloidal macromolecular phenomena becomes vividly apparent. The two phase water-fiber system gradually goes to an homogenized milk consistency, then to a heavy cream consistency, and finally to a lard-like, smooth, opaque, intensely white spreadable gel such as that shown in Figure 9. This 100 per cent

aqueous dispersion comprising a small percentage of unhinged microcrystals of cellulose admixed within much larger clusters of microcrystals having varying degree of aggregation, comprises a novel form of cellulose. Despite the fact that it consists only of pure cellulose dispersed in water, the product now has the functional and physical properties of a fat. Microcrystalline cellulose both in dry form and as a gel is now a commercial product of growing importance and is finding an increasing number of uses [3–9, 17, 18].

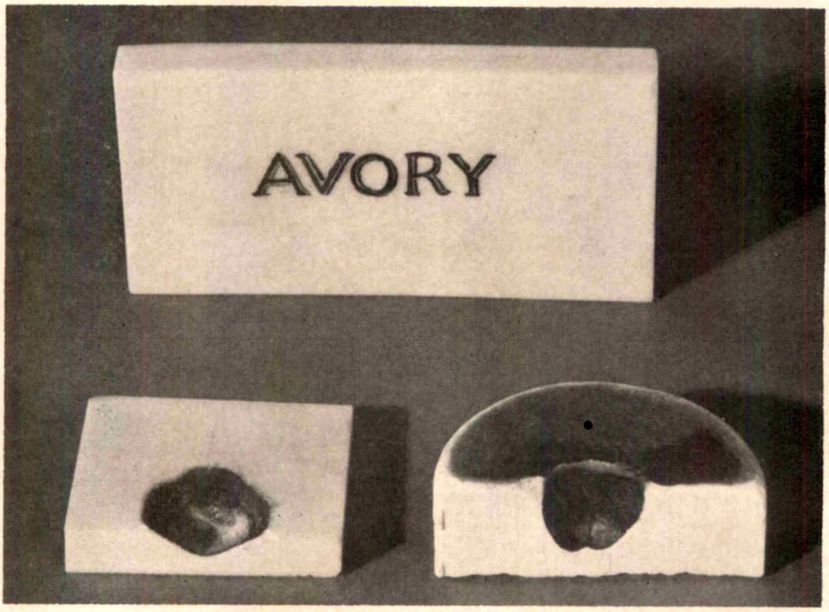


FIG. 10. "Avory" ® microcrystalline cellulose—a new structural form of cellulose.

As already noted, a minimum number of individual microcrystals must be present in the dispersion to give it stability [3, 7]. Removal of the smallest colloidal particles (the microcrystals) from a stable dispersion by a process such as fractionation immediately leads to a complete breakdown of the stable dispersion and the re-appearance of a two-phase system [20].

J. J. Hermans, Jr. has studied the novel rheological properties of microcrystalline cellulose gels, and their sensitivity to electrolytes [16, 19]. The resistance of these gels to an initial shear stress (i.e., their possession of a yield value) is a functional property with valuable commercial application, particularly for the production of stable suspensions of particulate matter [19].

Once a sufficient number of microcrystals has been freed to provide

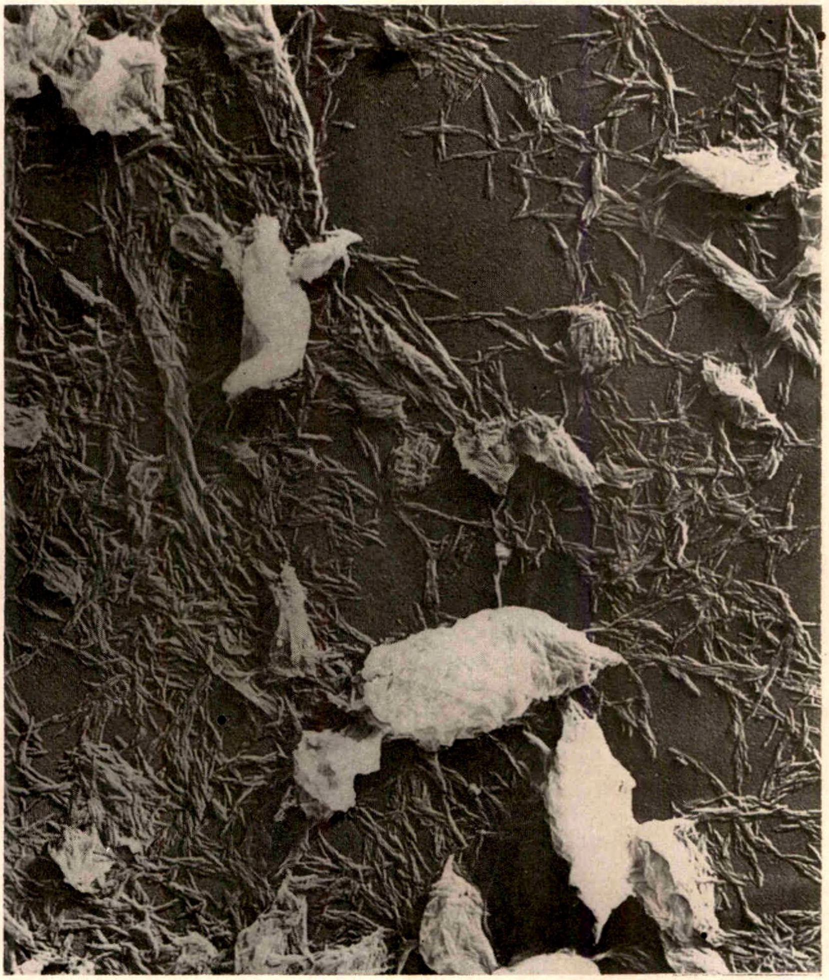


FIG. 11. Reaggregation of microcrystals of cellulose into discrete "porous" particles. "Holes" range from 10–100 Å, 25,000×.

the colloidal substrate that makes a stable suspension of large microcrystalline aggregates possible, additional novel states of matter are formed.

For example, by drying such stable gels at atmospheric pressure, an extremely dense (1.520 *vs.* a theoretical density of a single microcrystal of 1.566) and massive cellulose object is obtained. It is shown in Figure 10. The same forces as those used in making paper come into play to provide this novel structural form of cellulose. The main difference, however, is that in making the dense form of "paper" called "Avory" shown in Figure 10, millions of previously unavailable hydrogen bonds are now able to become engaged because of the new OH-rich surfaces of

FIG. 12. Topochemically derivatized (low D.S.) microcrystals—sodium carboxymethyl cellulose, 25,000×.

the liberated and now surface-exposed microcrystals (see Figure 7). On the other hand, in conventional papermaking, only exposed OH groups on the surface of the fibrils of Figure 5 come into play at those points where the fibrils cross over to give paper its tensile strength. It is obvious from the electron micrographs that the mechanism whereby "Avory" comes into being involves several orders of magnitude of additional hydroxyl groups than are involved in conventional papermaking.

As each unhinged microcrystal is freed from its natural interlocked position, shown clearly in the uniformly packed section of the fiber

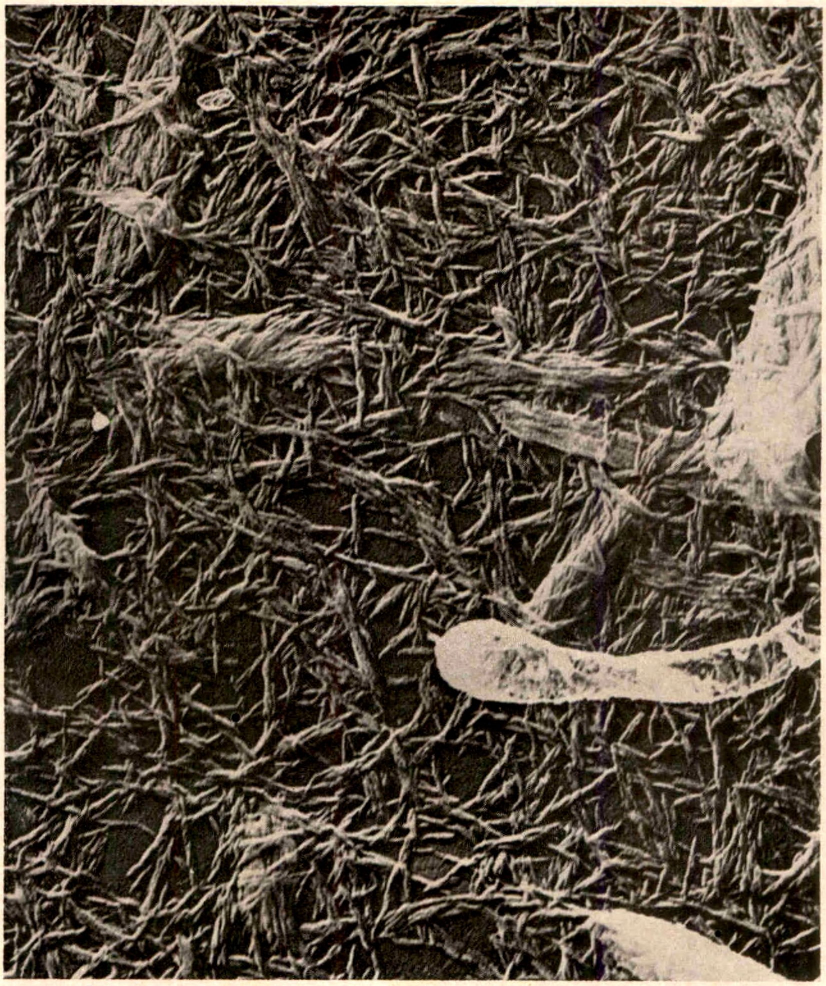


FIG. 13. Topochemically derivatized microcrystals of cellulose containing hydroxypropyl groups on the surfaces, 25,000×.

fragment of $\overline{\text{D.P.}}$ cellulose in Figure 7 (bottom right hand section), additional OH groups on the surface of each microcrystal become available in very large numbers for subsequent hydrogen bonding. The resulting effect has been observed in a practical manner. The density of "Avory" and its ablative properties to the oxyacetylene torch, increase as the number of unhinged microcrystals is increased in the gel prior to drying the colloidal aqueous dispersion at room temperature and atmospheric pressure. Figure 10 shows two pieces of compacted microcrystalline cellulose with different densities, and the corresponding effect of the acetylene torch. The piece on the right consisted of dry micro-

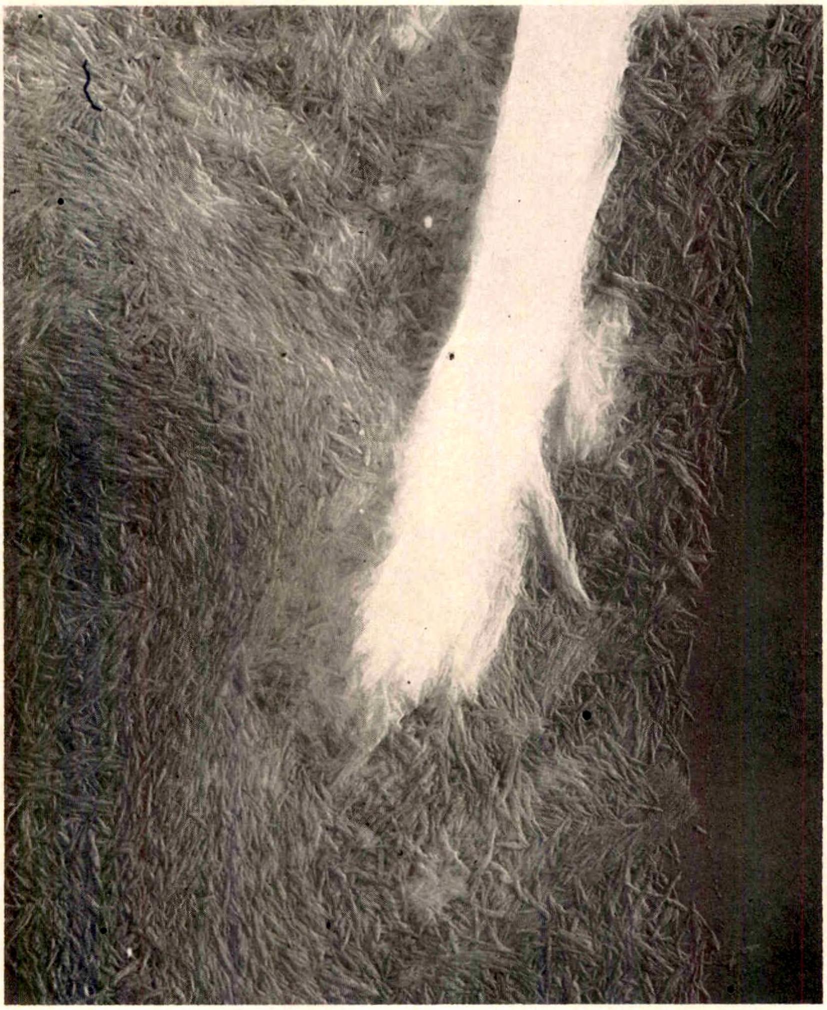


FIG. 14. Surface oxidized (chlorous acid) microcrystals of cellulose, 25,000×.

crystalline cellulose compacted in a press at 15,000 psi to give a tablet with a density of 1.36. The piece on the left is the structural product obtained by drying down a 14% microcrystalline cellulose gel and it has a density of 1.52. Ablation of the right specimen took place in 30 seconds, whereas the left specimen resisted the oxyacetylene torch for 70 seconds.

Figure 11 illustrates the mechanism whereby dry colloidal particles of cellulose containing numerous "holes" varying from 10 Å–100 Å in diameter may be produced. Literally, this is a new porous form of highly crystalline cellulose capable of absorbing oils, greases, catalysts, etc. When a dilute slurry of a suspension of individual microcrystals and

large aggregates of unhinged microcrystals is spray-dried under proper conditions, the free microcrystals reagglomerate, not as nature fashioned them in space originally (Figure 7), but in a man-made cluster, not unlike the manner in which wooden matchsticks aggregate when piled on a table top.

Still other fascinating opportunities present themselves when chemistry is wedded to these novel colloidal macromolecular particles. For example, reaction of the microcrystalline cellulose proceeds with particular ease and speed. Derivatives can be formed which are also colloidal. These are entirely new materials with very different properties and potential applications.

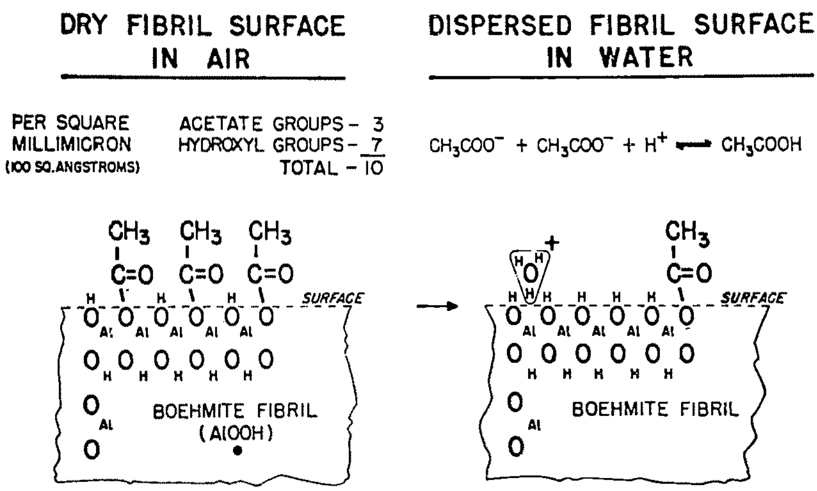


FIG. 15. Chemical modification of the surface of "microcrystals" of colloidal alumina.

At high degrees of substitution (D.S.), derivatives of microcrystalline cellulose are substantially the same material as produced from conventional cellulose. At low D.S., where the colloidal nature is maintained with surface substitution, the derivatives form colloidal dispersions. Dispersions of at least 20% solids in water can be produced. These may have the appearance of greases, ointments, or lotions, depending on the materials present.

Figure 12 shows what happens when sodium carboxymethyl groups are carefully added in very small amounts (average D.S. 0.30) onto the surfaces of unhinged microcrystals of cellulose. Now, with the surface charge altered, less energy is needed to separate the aggregated but unhinged microcrystals more efficiently into discrete and individual microcrystals. As may be seen in Figure 12 (which should be compared with Figure 7) the individual microcrystals are slightly smaller in size (due to

FIG. 16. Aggregation of "microcrystals" of colloidal alumina.

limited swelling and degradation which occurred as a result of the topochemical etherification step). More importantly, perhaps, is that now there is a predominance of individually separated microcrystals, and a great reduction in the size and number of larger agglomerates. The result is a colloidal gel with properties substantially different from that shown in Figure 9. The gel obtained from a dispersion of the topochemically modified cellulose microcrystals shown in Figure 12 is translucent, possessing a higher viscosity at equivalent concentration, and different rheological properties than the gel shown in Figure 9. It resembles a soft wax in many respects and is readily spreadable as an invisible coating on the skin.

FIG. 17. Formation of relatively organized clusters of colloidal alumina.

Topochemical modification of the surfaces of colloidal macromolecular particles, such as microcrystals of cellulose, obviously leads to a host of new products, each exhibiting widely different physical, rheological and functional properties. Figure 13 is an electron micrograph of topochemically altered microcrystals of cellulose in which a few hydroxypropyl groups have been added to the surfaces of the cellulose microcrystals. Now the gel properties resemble more closely those of the product illustrated in Figure 12 in which carboxymethyl ether groups were added. The product illustrated by Figure 13 at about 5 per cent solids is a smooth, "salve-like petrolatum," translucent but non-melting,

readily washable from the skin yet non-greasy. A somewhat different appearance of the size and shape of the microcrystals is obtained by mild $HClO_3$ oxidation of their surfaces. (Figure 14). Once again, the visible and the rheological properties of gels made from these particles are different from gels made by dispersing the topochemically modified microcrystals shown in Figures 12 and 13, respectively.

Other derivatives that have been prepared include methyl-, ethyl-, and hydroxypropylcelluloses. Nitrated $\overline{\text{D.P.}}$ cellulose derivatives have potential applications in solid rocket propellants; they can be made into a very dense particulate form with particles in the 1- to 3-micron range. Properties of all of these materials can be controlled over a wide range by varying, (1) the degree of substitution, (2) the heterogeneity of this substitution, and (3) the nature of the topochemically substituted group.

The surfaces of Baymal® colloidal alumina particles may also be altered to give rise to a range of novel colloidal macromolecular phenomena. U.S. Patent 2,944,914 by John Bugosh [14], for example, describes various esterification reactions on the surface of alumina monohydrate particles. Even organophilic surface-esterified particles having at least one dimension in the colloidal macromolecular particle size range are described [14]. The physical properties of such topochemical particles may be altered over a wide range depending on the chemical nature of the add-ons. Of course, the properties of Baymal® colloidal alumina are very much dependent on a substantial number of acetate groups on the surfaces of the boehmite fibrils [10–15] (See Figure 15.). Once again, however, this serves to demonstrate the far-reaching potential of chemically altering the surface only of a non-cellulosic colloidal macromolecular particle. Furthermore, as may be seen in Figures 16 and 17, there is a remarkable parallel between the manner in which "microcrystals" of colloidal boehmite form aggregates as the concentration in dispersion increases and the aggregation of unhinged microcrystals of cellulose.

We have described the transformation of two polymeric and crystalline materials—one natural, the other synthetic—into discrete colloidal macromolecular aggregates, and the alterations in physical properties which this state of sub-division gives to the parent materials. The unhinging and recovery of microcrystalline macromolecular aggregates with dimensions of approximately 50 Å × 50 Å × 5000 Å from natural polymeric materials or synthetic polymeric materials, or their control ed "synthesis" from solutions of crystallizable long chain molecules, proffer exciting and challenging new frontiers of polymer chemistry. The exploration and utilization of the novel and interesting resulting properties possessed by "free" polymer microcrystals in the true colloidal range will surely occupy an ever-increasing amount of attention on the part of high polymer chemists during the next decade or two.

Acknowledgment

The author acknowledges with appreciation the permission of the management of FMC Corporation to publish this invited paper, as well as the cumulative contributions of many associates and coworkers during the past decade that have helped so much to make this presentation possible.

A special note of thanks is extended to Frederick F. Morehead for the excellent electron micrographs he obtained throughout our microcrystalline cellulose studies, some of which have been presented in this paper.

Appreciation also is expressed to Dr. John Bugosh and the E. I. Du Pont de Nemours and Company, Inc., for their co-operation in connection with the use of information and photographs relating to Baymal® colloidal alumina.

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AMERICAN SCIENTIST, 53, 1965

THE INTEGRITY OF SCIENCE

A Report by the AAAS Committee on Science in the Promotion of Human Welfare*

* Responsibility for statements of fact and expressions of opinion contained in this report rests with the committee that prepared it. The AAAS Board of Directors, in accordance with Association policy and without passing judgment on the views expressed, has approved its publication as a contribution to the discussion of an important issue. AMERICAN SCIENTIST provides the medium for publication of the Report under the same conditions as are set forth by the AAAS Board of Directors.

Members of the Committee during the preparation of the report are: Barry Commoner, Washington University (Chairman); Robert B. Brode, University of California, Berkeley; T. C. Byerly, United States Department of Agriculture; Ansley J. Coale, Princeton University; John T. Edsall, Harvard University; Lawrence K. Frank, 18 Goden St., Belmont, Mass.; Margaret Mead, American Museum of Natural History; Walter Modell, Cornell University Medical College; William T. Kabisch, AAAS staff representative; and Gorman L. Mattison and Sheldon Novick, administrative assistants to the Committee.

I. The Problem

SCIENCE has systematically created a powerful and rapidly growing body of knowledge. From this basic knowledge have come the spectacular feats of modern technology: space vehicles, nuclear explosives and power plants, new substances and electronic machines, and a significant increase in human longevity. This record of growth and achievement creates a widespread impression that science is a strong, well-established human enterprise. Confidence that science can continue to fulfill human needs is a distinctive characteristic of modern society.

But the ultimate source of the strength of science will not be found in its impressive products or in its powerful instruments. It will be found in the minds of the scientists, and in the system of discourse which scientists have developed in order to describe what they know and to perfect their understanding of what they have learned. It is these internal factors—the methods, procedures, and processes which scientists use to discover and to discuss the properties of the natural world—which have given science its great success.

We shall refer to these processes and to the organization of science on which they depend as the *integrity of science.* The term is a useful one, for it connotes the importance of a unified internal structure to the success of science, as well as its guiding imperative—the search for objective knowledge. On the integrity of science depends our understanding of the enormous powers which science has placed at the disposal of society. On this understanding and therefore, ultimately, on the integrity of science, depend the welfare and safety of mankind.

The continued strength of science cannot be taken for granted. Although science has its own history and tradition, it is not wholly independent or self-sustained. Scientists are human beings, and science is a part of culture. What is the influence of changing social conditions, particularly the growing importance of science to society and the rapid approach of scientists to positions of power, on the search for new knowledge and on the system of scientific discourse? Can the very success of science and its closer interaction with the rest of our culture lay it open to the influence of new and possibly alien points of view which derive from other sectors of society: military, business, or political?

This is not a one-sided problem. As the success of science becomes more evident, its trappings and its personalities begin to make their appearance outside the laboratory: at international conference tables, in legislative chambers, in advertising appeals, and in the hue and cry of partisan politics. Are these appropriate uses of the methods of science? Can political life benefit from the viewpoint of science? Can the integrity of science safely withstand the invocation of science in political issues?

The importance of these questions and the absence of any immediate answers to them are the reasons for this report. In it we shall consider the effects of certain features of our culture on the integrity of science. These are problems which affect science wherever it exists, but in ways which vary with local circumstance. We are here concerned with the issues that arise in connection with the recent development of science in the United States.

II. Experiments in Space: "Starfish" and "West Ford"

Symbolic of the immense growth and power of modern science is the exploration of space. With the development of powerful rockets for military purposes, it has become possible in the last decade to send vehicles and human passengers beyond the earth's atmosphere. With the concurrent development of highly efficient sensing devices and methods of communication, these vehicles have also served as important means of gathering scientific information. Elaborate basic research, a vastly expensive technology, and strong political and military motivation unite in nearly every venture into space.

One of the earliest discoveries of space research was the existence of belts of atomic particles surrounding the earth and trapped by its magnetic field in arcs between the north and south poles. Hardly had this "magnetosphere" been noticed and named for its discoverer, Van Allen, than *experiments*, rather than mere observations, were underway there [1].

On April 30, 1962, the U.S. Government announced its intention to conduct nuclear explosions at high altitudes in order to ascertain the effects of artificially-injected electrons on the natural belts of the magneto-

sphere. The immediate motivation was military interest in the disruptive effects on radio communication of atomic particles produced by a high altitude nuclear explosion. The experiment—"Starfish"—was also of interest to scientists because its effects might reveal significant data about the magnetosphere itself.

Three high altitude tests had been set off secretly in August 1958 over the South Atlantic. When they were made public some six months later, scientists in this country and abroad protested vigorously. The announcement of the new test brought renewed protests from scientists, especially radio-astronomers. Some scientists predicted that the experiment would cause large-scale persistent changes and hamper further study of the still poorly understood Van Allen belts, but others disagreed. Resolution of these disagreements was difficult, for secrecy restrictions limited the exchange of information among the disputants.

The U.S. Government then announced that it had called together a group of leading scientists to consider whether the proposed tests would substantially prejudice astrophysical and geophysical science or create a radiation hazard to manned space flights. On May 28, 1962, the U.S. Government announced that this committee was convinced that the effects of the Starfish experiment would

> "disappear within a few weeks to a few months," and that "... There is no need for concern regarding any lasting effects on the Van Allen belts and associated phenomena." [2]

The Starfish explosion took place on July 9, 1962, when a 1.4 megaton hydrogen bomb was detonated 250 miles above Johnson Island in the Pacific. Despite early confusion regarding the physical consequences of this explosion, it is now clear that it generated a long-lived belt of atomic particles in the magnetosphere and that it has obscured the properties of the natural radiation belts. According to a review of the experiment by McIlwain,

> "... it may be necessary to wait more than 30 years before the natural electron fluxes in the region around 1.5 earth radii can be measured with complete freedom from artificial effects." [3]

Several satellites, *Transit VIB*, *Traac*, and *Ariel*, were extensively damaged by the new radiation belt. *Ariel* is especially noteworthy as it was launched by the United States in order to carry British instruments, as a cooperative venture. *Telstar*, on the other hand, rode out the high radiation levels successfully for some time after the test, but radiation damage was noted in later reports. [4]

The Starfish experiment is a spectacular demonstration of present capabilities for human intervention into natural phenomena in space. It also reveals serious inadequacies in our present ability to predict the consequences of such interventions, and in the attendant experimental procedures.

Science involves not only passive observations of nature, but also experiments which influence natural processes, either to reveal new information or to provide a practically useful result. Scientific procedure provides for special rules to regulate experiments, designed to elicit from them observations of maximum validity. The scientist's experiments are disciplined interventions into nature. They are based on previously gained knowledge which is sufficient to estimate the practicality and usefulness of the operation. Experiments are designed not simply for the effect but to yield measurable results in interpretable form. Usually, this requires the establishment of an experimental control, in which the particular alteration induced by the experiment is deliberately excluded, so that, by direct comparison, one can determine the effect of the factor of interest.

Properly executed, the Starfish experiment would have been preceded by a survey of natural bands followed by general scientific discussion of the observations. Had this been done, the data derived from particles artificially injected by the explosion could have added to scientific knowledge without precluding information about the natural phenomenon. Instead, the Starfish experiment was carried out in the absence of adequate knowledge of the natural belts. By artificially injecting subatomic particles into the unique and worldwide magnetosphere, the experiment has limited the information which can be derived from inquiries into this aspect of nature. As Dr. Van Allen stated at our Committee's symposium at the AAAS annual meeting in Philadelphia,

> "Our failure as a nation to produce a substantial study of the scientific consequences of these tests long before the decision was made that they were to be conducted, is, it seems to me, quite inexcusable. With tests such as this, studies should have been conducted in such a way that they were subject to publication and general scientific discussion." [5]

Free dissemination of information and open discussion is an essential part of the scientific process. Each separate study of nature yields an approximate result and inevitably contains some errors and omissions. Science gets at the truth by a continuous process of self-examination which remedies omissions and corrects errors. This process requires free disclosure of results, general dissemination of findings, interpretations, conclusions, and widespread verification and criticism of results and conclusions.

The principle of free dissemination and open discussion has not been fully honored in the Starfish experiment. Although an attempt was made to forewarn the scientific community of proposed experiments (through the IGY-AGIWARN network), insufficient time and secrecy restrictions inhibited the normal predictive and analytical processes of science. A detailed and unhurried discussion of the possible effects of the high altitude explosion in the open scientific literature might have revealed—

in advance of the experiment—a consensus contrary to the conclusion of the government's *ad hoc* committee that the effects would be short-lived.

A second example of recent space research, the "West Ford" project, further illuminates the hazards of large-scale experimentation, and shows how they can be minimized by adherence to the proper procedures of science. [6]

In the summer of 1958, the U.S. Army Signal Corps convened a group of scientists to study, among other things, means of establishing an invulnerable worldwide military communications system. It was suggested that such a system could be established by encircling the earth with orbiting belts of fine copper wires. These would form radio mirrors for multi-channel microwave signalling, linking any two stations on earth if the heights of the belts were sufficiently great. The belts would be invulnerable to attack, since they would only reflect ground-controlled radio waves, and, unlike communications satellites, such as *Telstar* and *Echo*, would be physically indestructible.

The proponents of "West Ford" developed a technique for testing their idea experimentally on a small scale. By placing relatively few copper wires in a predetermined orbit, they could establish a temporary belt, which although not operational in a practical system of communications, could establish the feasibility of the full-scale scheme. Calculations showed that pressure from sunlight would gradually drive the proposed experimental belt down into the atmosphere and destroy it within a time dependent on location of the orbit.

The Department of Defense approved investigation of this approach and research on the project began at the Lincoln Laboratory. Because of its military importance, the project was "born classified." However, knowledge that some test of these ideas was planned soon became current among astronomers throughout the world. Many became concerned about the possibility that the bands would interfere with optical and radio observations. To be detected, a celestial object must yield a signal which stands out against the random optical or radio signals from the nearby regions of the sky. Contamination of the sky with metallic dipoles, if sufficiently dense to produce a signal which is discernible against the natural background, will hamper observations of faint celestial objects.

With the details of the proposed experiment under secrecy restrictions, actual knowledge about U.S. intentions and the possible effects of the experiment were scanty. As Dr. Edward Purcell of Harvard University reported later, regarding the decision to undertake the experiment,

> "In those days the project, directed as it was toward military communications, was secret. But the scientists in the project and the responsible officers in the government saw that the decision involved questions that could not be rightly disposed of by arithmetic in a classified report. To broaden the basis for a responsible decision, the Space Science Board of the National Academy was asked to assemble

a group of scientists to analyse the planned test. . . . Meanwhile the word was beginning to get around. Fragmentary knowledge and rumours naturally left many scientists apprehensive and suspicious. It was decided to meet the problem quite openly. The Defense Department and the Lincoln Laboratory, with the support of the President's Science Advisory Committee, effected a rapid declassification of all essential matters. . . ." [7]

After several meetings, including at least two that were restricted in size because of security requirements [8], the *ad hoc* committee assembled by the Space Science Board concluded that, while present astronomical experiments would not be seriously hampered, a permanent belt—even of small size—might become a hazard in later years, as greater sensitivity is achieved in radio and optical instruments. The committee recommended that the dipoles be established in a temporary orbit, that a non-military spacecraft be used to launch the equipment, and that a ground-controlled fail-safe device be installed to prevent dispersal of the copper dipoles if the rocket happened to achieve an undesirable orbit. These conditions were not fulfilled in the government plans for the first West Ford attempt (in May 1961). The committee advised that the proposed launch be called off, and it was. [9]

Neither the *ad hoc* committee studies nor the resultant consideration by the Space Science Board have as yet been published. However, for the reason given by Purcell (see above) in September 1960, a large part, but not all, of the technical information about the project was declassified and presented to a meeting of the International Scientific Radio Union by Walter Morrow of the Lincoln Laboratory. Later, several detailed considerations of the problem were prepared for publication in the *Astronomical Journal* by members of the Space Science Board's subcommittee and by members of the Lincoln Laboratory. [10]

An examination of these open discussions reveals the following information of interest: (*i*) All discussants agreed that the brightness of the experimental band proposed by Morrow would not interfere with astronomical observations from the earth. (*ii*) Certain parameters in the Morrow calculation (e.g., rate of needle dispersion) remained classified, although these data are essential to a calculation of the life-time of the belt. [11] (*iii*) The experimental belt could interfere with astronomical observations from satellites which might otherwise make novel studies of very faint celestial objects; this might be obviated by particular locations of the dipole orbit. [12] (*iv*) A dipole band sufficiently dense to support an operational communication system would seriously interfere with optical and radio observations. [13]

In August 1961, the Space Science Board announced to the scientific community that it had received government assurance that no additional launches of orbiting dipoles would take place until results of the first experiment had been analyzed and evaluated. The Space Science Board asked that full scientific and operational information about the project

be published as soon as possible. However, at the same time, the International Astronomical Union adopted two resolutions, one against the precedent of contaminating space, and the second, while thanking the U.S. Government for announcing the planned experiment, said that the IAU

> ". . . is completely opposed to the experiments until the question of permanence is clearly settled in published scientific papers with adequate time being allowed for their study." [14]

On October 6, 1961, an article in *Science* by Shapiro and Jones of the Lincoln Laboratory for the first time made public the altitude and inclination of the proposed orbiting dipole belt. Calculations for estimating belt lifetime in various orbits were described, leading to an estimate of seven years for the proposed orbit.

On October 21, 1961, the experiment was carried out. A military rocket was used. "Fail-safe" procedures were not provided. The dipole dispenser arrived not in the proposed seven year orbit, but in a different one, for which the calculated lifetime was "indeterminably longer than the seven years estimated . . . ." [15] However, the dipole dispenser failed to work properly and the belt of dipoles was not formed. The hazard of a long-lived belt, which might have interfered with future astronomical observations was avoided only because of an instrument failure.

A second attempt, on May 10, 1963, succeeded. A belt was established in a short-lived orbit and subsequent observations suggest that it is disappearing at the predicted rate. The instrument included a "fail-safe" device. Data obtained from this experiment provide a basis for firmer conclusions regarding the possible effects of large-scale operational belts on astronomical research. [16]

The events associated with the West Ford experiment show that adherence to the principles of disciplined experimentation and open communication between those responsible for the project and the scientific community generally succeeded in preventing possible hazards to further astronomical research. However, a number of serious difficulties were encountered in achieving the final laudable outcome of the project.

Because of secrecy, astronomers were at first unable to develop a critical analysis of the proposed experiment. Some data essential to the calculation of the belt's lifetime (i.e., mechanism of needle dispersion, altitude and declination of the proposed band) became available to astronomers only 15 days before the first experiment was actually carried out, a period much too short to permit any necessary corrective effects of open scientific scrutiny to operate.

The Space Science Board committee succeeded in preventing a proposed launch which did not conform to its recommendations in the summer of 1961. However, when the experiment was actually carried

out in October 1961, not all of the committee's recommendations were followed. The package of dipoles was launched into a long-lived orbit. If the unexpected failure of the package to disperse the dipoles had not occurred, this experiment might have realized the fears expressed by astronomers regarding its hazards to future astronomical research.

Experience with both "Starfish" and "West Ford" shows that the scientific procedures which must govern experimental investigations, if they are to further the accumulation of knowledge, can be broken down. Under pressure for results of military importance, the principles of disciplined investigation, and of respect for necessary experimental controls may be to a degree neglected if, because of attendant secrecy, the experiments are not open to the full scrutiny of the scientific community. In these experiments a predictive failure may be irreparable, for the experiments are not confined to a single laboratory, but intervene in phenomena which are unique on the planet.

III. Exploration of the Moon: Project "Apollo"

A considerable part of the U.S. space program is devoted to a particular mission, Apollo, which is designed to land one or more men on the moon and return them to earth. [17] The Apollo project is a technological enterprise in that it requires the application of basic scientific knowledge to the solution of a given problem—the accomplishment of a manned landing on the moon. However, since the project is to be achieved in an environment which is new to science, it requires the acquisition of certain basic knowledge about the moon and interplanetary space: for example, the gross physical character of the moon's surface, chemical properties and radioactivity of surface materials, intermittent changes in cosmic ray intensity in interplanetary space.

The chief independent body which has advised the government on the scientific aspects of space research is the Space Science Board (SSB) established by the National Academy of Sciences in 1958. In the scientific considerations developed by the specialists serving on committees reporting to the Space Science Board, the scientific values to be derived from the manned exploration of the moon have been given relatively little importance, as compared with a number of other investigations of the moon which do not involve a manned landing.

For example, a discussion of lunar research by H. C. Urey (published in 1961 in *Science in Space*, edited by Lloyd V. Berkner and H. Odishaw), of SSB [18], which summarizes SSB considerations during 1958–1961, proposes the following order of priorities: (i) a satellite of the moon; (ii) a hard landing lunar probe; (iii) a soft landing lunar probe; (iv) the return of samples; and (v) manned landing. In an earlier SSB committee report, manned exploration of the moon was one of nine projects, in a total of twenty-one, assigned to the lowest of three priority

categories.

In February 1961, the SSB discussed national goals in space research, and in a statement transmitted to the government in March 1961 [19], stated:

> "As a result of these deliberations, the Board concluded that scientific exploration of the moon and planets should be clearly stated as the ultimate objective of the U.S. Space program for the foreseeable future."

In this statement the SSB did not discuss the timetable for carrying out manned exploration of the moon in specific terms, but commented:

> "The Board concluded that it is not now possible to decide whether man will be able to accompany early expeditions to the moon and planets. Many intermediate problems remain to be solved. However, the Board strongly emphasized that planning . . . must at once be developed on the premise that man will be included . . .
>
> ". . . Planning for manned scientific exploration of the moon and the planets should be consummated only as fast as possible consistent with the development of all relevant information. The program should not be undertaken on a crash basis which fails to give reasonable attention to assurance of success or tries to by-pass the orderly study of all relevant problems."

A timetable for manned lunar exploration was established for the first time by the decision of President Kennedy, announced on May 25, 1961, which concluded that

> ". . . this Nation should commit itself to achieving the goal, before this decade is out, of landing a man on the moon and returning him safely to earth." [20]

The controlling reasons given for this decision by President Kennedy (*ibid.*) and by his scientific advisor, Dr. Jerome B. Wiesner, were not scientific, but social and political. Thus, in response to a question regarding his agreement with President Kennedy's decision to establish the project as a national goal, Dr. Wiesner stated:

> "Yes. But many of my colleagues in the scientific community judge it purely on its scientific merit. I think if I were being asked whether this much money should be spent for purely scientific reasons, I would say emphatically 'no.' I think they fail to recognize the deep military implications, the very important political significance of what we are doing and the other important factors that influenced the President when he made his decision." [21]

The decision to accomplish a manned landing on the moon by 1970 established new priorities in space research which are clearly reflected in subsequent considerations by scientists of investigation of the moon. In these later considerations, the importance of each proposed scientific investigation is usually evaluated in the light of the intention established by President Kennedy's decision. This can be seen in the report of the Summer Study Session at the State University of Iowa conducted under the auspices of the Space Science Board:

> "This report deals with the immediate future up to and in part including Apollo. Inasmuch as landing a man on the moon is an announced national goal, much emphasis is placed on those scientific experiments and observations which are

vitally necessary to accomplish this mission or which might contribute to its success. Our specific recommendations are strongly influenced by this consideration." [22]

In general, scientific observations required for the planning of the manned landing are now assigned higher priorities than other studies which are of greater scientific interest but not essential to the development of the technology needed for the Apollo project. Therefore, the pattern for development of scientific research in space has been altered significantly by the essentially political decision to undertake the Apollo program.

This procedure is seriously at variance with important precepts of scientific experimentation and technology. The preferable order of events is: basic scientific investigation, technological application based on the resultant basic knowledge, social use of the technological innovations. In the Apollo program this sequence has been reversed, so that a program for a particular technological achievement has been committed, even as to the date of its accomplishment, in advance of the orderly acquisition of the related basic knowledge. [23] The Apollo program, in its present form, does not appear to be based on the orderly, systematic extension of basic scientific investigation.

The Apollo program is extremely costly in funds and in required personnel. The total projected budget for space research, a large part of which is devoted to the Apollo project, represents a considerable part of the nation's entire expenditures for research. In 1964, out of a total federal expenditure of $1800 million for *basic research*, NASA accounted for $700 million, and the National Science Foundation about $300 million. Thus, more support for the nation's total program in basic research comes from a mission-oriented agency—NASA—than from NSF which is the agency established by Congress for the purpose of developing a science-oriented national program in basic research. At the same time, NASA accounts for the Federal Government's largest expenditures in *applied* research. Since, at the present time, the Apollo program represents the bulk of NASA activities, these estimates apply with only a minor reduction to the Apollo program itself.

The demand of the space program for scientific personnel, which is in large part due to the Apollo program, has been the subject of some confusion.

In 1962, Dr. Hugh L. Dryden, Deputy Director of NASA, stated:

"It has been estimated that by 1970 as many as one-fourth of the Nation's trained scientific and engineering manpower will be engaged in space activities, although I cannot confirm the accuracy of this estimate." [24]

NASA testimony before the Senate Committee on Aeronautical and Space Sciences in November 1963 reduced this estimate to 5.9% of the total supply of scientists and engineers in 1970. [25] Nevertheless,

a questionnaire sent to 2000 randomly selected members of the AAAS in July 1964 showed that the percentage of those answering who receive direct or indirect federal support in connection with space activities had already reached the level of 12%. [26]

A recent analysis of scientific manpower utilization by a committee of the National Academy of Sciences reported that

> ". . . the NASA program will accentuate the shortage of personnel with specialties such as systems technology, stability and control, guidance systems, and internal flow dynamics. Furthermore, there will be a pronounced effect on the market for less experienced mechanical, electrical, and aeronautical engineers, and for physicists and mathematicians." [27]

Thus, the NASA basic research program erects, in parallel with the NSF program, a new national system of scientific support, which, in contrast to that of NSF, is mission-oriented rather than science-oriented. Its major program, Apollo, which is justified by a social rather than scientific purpose, will significantly influence the direction of basic scientific research in the United States.

We are aware that there is a considerable precedent for such constraints in wartime research. An example is the World War II atomic bomb project, which represented one aspect of a far-reaching social decision to turn much of our nation's material and intellectual strength to the single task of winning World War II. However, in the present instance, there is no evidence that there has been an opportunity for our society to make a conscious choice to sacrifice the advantages of free development of basic scientific research, even temporarily, for the purpose of winning a purported race to the moon.

In this connection the following statement made by a NASA official in an address to the Annual Meeting of the AAAS in December 1962 is worth noting:

> "At this stage, the scientific community has the opportunity to assist in determining what man will do out in space, and in particular what he will do when he gets to the moon. If the scientific community does not give this matter its thought and attention and proffer its suggestions and advice, its ideas will be missed. But this will not bring things to a halt. Someone else will make the scientific decisions." [28]

If the scientific community is subjected to pressure or blandishments designed to solicit research activity which conforms to the purpose of the space research program, the free pursuit of knowledge will suffer.

Finally, we find reason for concern in the confusion regarding the social and scientific justification of the Apollo project. In the scientific considerations of the Apollo program, we have found several instances in which scientific advisory groups assert, in their reports, that the Apollo program is justified by such nonscientific motivations as "man's innate drive to explore unknown regions," or the pursuit of "national prestige." We believe that such appeals, made by scientists who are acting

in their professional capacities, or which are closely attached to professional scientific judgments, are inherently dangerous both to the democratic process and to science. If a scientist, as an individual citizen, wishes to promulgate a particular political course, he is of course free to do so. However, in our view, when such advocacy is associated with his organized professional scientific activity, the political or social intent acquires a wholly unwarranted cloak of scientific objectivity. This tends to obscure the fact that the political issue, despite its association with science, is, like all matters of public policy, open to debate. Such action on the part of scientists is likely to inhibit the free public discussion of the issue, and delay the development of an independent judgment by citizens generally.

IV. Large-Scale Technology and the Environment

In the last 25 years there has been a dramatic improvement in the capability of chemical technology to produce commercially important quantities of a large number of useful new synthetic compounds: plastics, pesticides, herbicides, food additives, medical drugs, detergents and a great many products for specialized industrial applications. This development reflects basic scientific advances in organic chemistry and chemical engineering. It is the basis of new, heavily capitalized industries that have yielded important economic and social benefits.

These advances have resulted in the dispersal into the biosphere of numerous synthetic organic compounds, some in the amounts of millions of pounds annually. Some of these substances are not immediately degraded on entry into the biosphere. As a result, living things, including man, have absorbed varying amounts of newly synthesized substances, with which they have had no previous contact.

A number of problems have arisen recently which are characterized by the appearance of undesirable side-effects incidental to the application of some of the new products. Examples are: killing of animals and fish by insecticides, increasingly troublesome pollution of water supplies by agricultural chemicals and by industrial wastes, accumulation of synthetic detergents in water supplies. Two informative examples of these problems are experiences with synthetic detergents and insecticides.

Within a few years after the large-scale introduction of new synthetic detergents in the early 1940's, untoward effects, especially foaming, were noted in water supplies in various parts of the country. It was then discovered that the new detergents, unlike soap, were not degraded by the biological processes in sewage disposal plants. As a result, in certain areas, detergents began to appear in streams and rivers, and in potable water supplies derived from these sources. Questions—as yet unanswered—arose concerning the possibly hazardous effects of ingestion of detergents by human beings.

It was then discovered that the resistance of detergents to degradation in sewage disposal processes was due to a particular chemical attribute —branched structure of the detergent molecule. Bacterial enzymes that degrade hydrocarbons are incapable of acting on branched molecules (in contrast, common soap is a straight-chain molecule and is degradable). The chemical industry is now making an intensive effort to develop economical, degradable, synthetic detergents. Legislation preventing sale of non-degradable detergents has been introduced into Congress, and the industry has announced plans to replace the non-degradable types.

The problems arising from the large-scale dissemination of pesticides, particularly insecticides, during the last 20 years are exceedingly complex and diverse. They have been the subject of intense discussion recently and have been considered in a report of the President's Science Advisory Committee. [29] We shall discuss briefly a relatively limited instance of the general problem—difficulties associated with the presence of insecticides in the waters of the Mississippi River.

In 1957, the first extensive spraying with the then relatively new pesticide endrin, a chlorinated hydrocarbon, began in the Mississippi Valley. It has since been used extensively by sugar cane and cotton farmers to control insects attacking these crops.

Between 1954 and 1958, investigators reported several instances in which dieldrin (an insecticide closely related to endrin, and which is produced metabolically from the latter) was found to cause killing of fish following application of the insecticide to nearby land. [30] In March 1964, the U.S. Public Health Service and officials of the State of Louisiana announced that "water pollution involving toxic synthetic organic materials appear to be the cause" of fish-kills in the Mississippi River. [31] The fish, the river waters, and the river mud were found to contain endrin and dieldrin, and several unidentified organic compounds.

The Mississippi River fish-kills have had significant economic effects. A long-established fishing industry in the bayou country of Louisiana has been reported to be seriously hampered by the problem. [32] Investigations are under way to determine if levels harmful to humans have occurred, but little detailed information about chronic toxic effects in humans is available as yet. Fish containing measurable amounts of insecticide have reportedly reached the market in some Louisiana towns, and health officials have expressed concern about possible hazards from drinking water of Louisiana cities which is taken from contaminated rivers. [32]

The sources of the insecticides found in Mississippi River fish are not firmly established at this time. Possible sources are runoff from farmlands routinely treated with insecticides and industrial wastes from

insecticide manufacturing plants.

Both of the instances cited represent a defect in the method of introduction of technological processes based on large-scale synthesis of new types of organic substances. In both cases, the substance introduced was demonstrably effective for its intended purpose—as a washing agent or as a pesticide. In each case, the hazard resulted from the substance's unanticipated secondary effects in the biosphere.

The effects of introducing these substances into the biosphere were not adequately studied, along with their other properties, when they were under laboratory development. For the case of detergents, this conclusion has been stated explicitly by Mr. C. G. Bueltman, of the Soap and Detergent Association, before this Committee's symposium on December 30, 1962.

> "The development of synthetic detergents was aimed primarily at improvement of functional properties associated with the *raison d'etre* of the product—i.e., to wash efficiently in a broad variety of water supplies; to possess good shelf life; to be pleasing in appearance, odor, feel; to suds freely and to sell at a price acceptable to the consumer . . .
>
> "It is admitted that total consequences and more particularly, susceptibility of biological decomposition, were not anticipated. In fact, so far as bio-degradability goes, hindsight shows that this was universally overlooked as a factor requiring consideration." [33]

That similar limitations were operative in the case of chlorinated hydrocarbon insecticides is evident from a review which stated in 1959, when these substances were already in wide agricultural use,

> "Except for DDT, little is known about the effects on fish and fish-food organisms of chlorinated hydrocarbon insecticides that have been used for a number of years." [34]

The introduction of synthetic detergents and insecticides into the biosphere represents a serious human intervention into natural processes. The evidence cited shows that this intervention was not based on an orderly, disciplined development of all the requisite basic scientific information. The full biological significance of the large-scale introduction of synthetic detergents and insecticides could have been discovered much sooner if there had been planned systematic studies of their effects on the water supply in small-scale field trials.

In the absence of such studies there was a large economic commitment to the production of these contaminants before their crucial faults were discovered. There has been inadequate contact between the scientific considerations operative in the development of these substances—their chemical structure and synthesis, and their efficiency as detergents or insecticides—and the equally well-known biological phenomena into which they are to intervene. In the development of these new products there has been a serious gap between the relevant branches of science.

Experience with nuclear testing provides a further insight into these

problems. Since 1948, nuclear explosions carried out for purely experimental purposes (i.e., for the technological improvement of weapons) by China, France, Great Britain, the United States and the U.S.S.R. have disseminated millions of curies of radioactive materials over the planet as fallout.

Considerable amounts of these materials have been absorbed by plants, animals, and man. Because radiation causes important damage to biological processes, this burden of radioactivity—added to radiation from natural sources and from medical procedures—increases the total risk of harm to man. The problem of estimating this medical risk has resulted in considerable confusion and controversy.

The iodine-131 problem provides a useful example of the biological aspects of this complex and still-confused subject. This radioisotope is produced by all nuclear explosions. Beginning in 1957, fallout-monitoring procedures in the U.S., especially those carried out by the Public Health Service, included measurements of iodine-131 in milk, which is a major source of human exposure to this isotope. The results show that, in a number of regions of the U.S., iodine-131 in milk reached levels in the range of 100–1000 pc/liter* during periods of active atmospheric testing. Although the precise medical significance of these levels is still a matter of debate, there is a common agreement that this degree of contamination results in a radiation dose to the target organ (the thyroid) considerably in excess of the biological dose from any other fallout isotope. Hence, iodine-131 is now recognized as the most severe radioactive hazard from fallout, at least during a time when testing is under way.

To what extent was the predominance of iodine-131 among the possibly hazardous consequences of nuclear testing known to the planners of the nuclear test program? The most comprehensive assemblage of data regarding nuclear explosions is "The Effects of Nuclear Weapons," published jointly by the Atomic Energy Commission and the Department of Defense in 1957, and in revised form in 1962. Both editions of this handbook discuss the possible biological hazards from fallout and describe the behavior of isotopes of chief interest in this connection. The 1957 handbook discusses strontium-90, and cesium-137; the 1962 edition discusses these radioisotopes and carbon-14 as well. Neither edition mentions the possibility that iodine-131 might contribute to the biological hazards from nuclear tests. There appears to have been no published notice of the iodine-131 problem until 1957, when the issue was raised in testimony before the Joint Congressional Committee on Atomic Energy. Although a considerable number of fallout-producing nuclear tests had already been carried out by 1957, the agencies responsible for them were

* Picocurie (pc) = 10^{-12} curie.

apparently unaware that iodine-131 constituted the most severe immediate hazard.

It would appear that nuclear tests, like other recent interventions into the biosphere, were undertaken without an adequate understanding of their possible biological hazards. As massive experimental effects on the biosphere, nuclear explosions therefore represent operations which have not been carried out in keeping with disciplined scientific procedures.

An important cause for this technological failure is that discussion of the fallout problem by the general scientific community was hampered by secrecy. Until 1954, nearly all data about fallout were unavailable to the scientific community because of security restrictions. Following the declassification of these data, when the general scientific community had an opportunity to consider the problem, a number of important changes in the understanding of the fallout problem took place through contributions made by the general scientific community. [35] The corrective effects that followed partial declassification testify to the importance of an independent community of scientists to the effectiveness of science as a source of knowledge.

The problem of evaluating environmental hazards, which is particularly exemplified by experience with fallout, reveals another issue for the integrity of science. By 1958–59, sufficient evidence had accumulated to convince most scientists that there is a linear relationship between radiation dose and biological damage. This viewpoint has been adopted by the Federal Radiation Council, which has the responsibility of establishing standards for radiation exposure in the U.S. [36]

If, as required by the linear theory of radiation damage, any increase in radiation exposure is accompanied by some added risk of damage, in determining the acceptability of the risk the hazard must be balanced against the benefit expected from the relevant operation. Such a balance between risk and benefit is a value judgment. Determination of a standard for radiation exposure which is based on such a judgment is no longer a scientific conclusion but a social one. This relationship is explicitly stated in FRC policy:

> "In establishing radiation protection standards, the balancing of risk and benefit is a decision involving medical, social, economic, political, and other factors." [37]

The subsequent history of the standards established by the FRC has been marked by chaos and controversy. This is illustrated by some recent events associated with exposures from iodine-131 due to fallout. During the summer of 1962, atmospheric tests by the U.S.S.R. and the U.S. caused iodine-131 levels in several areas of the U.S. to approach Range III (which, according to FRC guidelines, requires control measures), and local health authorities in Utah, Wisconsin, and Minnesota took appropriate action. However, on August 17, 1962, the Secretary of Health, Education and Welfare (also Chairman of the FRC) stated that control

measures were not called for because the guidelines had been developed for "normal peacetime conditions" and were not applicable to fallout from nuclear tests. This view holds that the benefits to the nation from nuclear testing have not entered into the balance which led to the establishment of the radiation standards and that, when these benefits are taken into account, higher levels of radiation exposure and correspondingly greater risks are warranted. The FRC has recently announced new and higher standards applicable to fallout which also take into account the possible risks involved in the protective actions. Thus the guidelines have been revised, but without any indication as to the weight given to the benefits of nuclear testing.

These reinterpretations of the iodine-131 standards have led to confusion and doubt regarding the inherent validity of the standards promulgated by the FRC. A challenge to a scientific body can ordinarily be met by analysis and discussion of its scientific findings by the scientific community. This process finally resolves the disagreement or leads to an equally acceptable statement of uncertainty. However, in the case of the revision of radiation exposure standards this procedure is not possible—for the question under discussion is not an evaluation of the risk from radiation (which is a scientific matter subject to scientific validation), but is rather the wholly non-scientific question of how the benefits of nuclear testing should be balanced against the increased medical risk from fallout. Such a challenge therefore cannot be resolved by the procedures of scientific discourse. Since the FRC is presumably a scientific body, an unresolved challenge to the validity of its conclusions will tend to be regarded—especially by the public—as a reflection on the ability of science to elucidate the problem.

Similar difficulties have occurred with disturbing frequency in the recent controversies regarding the effects of fallout, of nuclear war, and of environmental contamination in general. In a number of instances, individual scientists, independent scientific committees, and scientific advisory groups to the government have stated that a particular hazard is "negligible," or "acceptable" or "unacceptable"—without making it clear that the conclusion is *not a scientific conclusion, but a social judgment*. Nevertheless, it is natural that the public should assume that such pronouncements are scientific conclusions. Since such conclusions, put forward by individual scientists, or by groups of scientists, are often contradictory, a question which commonly arises among the public is, "How do we know which scientists are telling the truth?" Regardless of its origin, such a doubt erodes the confidence of the public in the capability of the scientific community to develop objective knowledge about scientific issues of crucial importance to public policy. To arrogate to science that which belongs to the judgment of society or to the conscience of the individual inevitably weakens the integrity of science.

V. *The Integrity of Modern Science*

The foregoing examples testify to the striking success of modern science. They show that science has developed powers of unprecedented intensity and worldwide scale. The entire planet can now serve as a scientific laboratory.

At the same time, new large-scale experiments and technological developments of modern science frequently lead to unanticipated effects. The lifetime of the artificial belts of radiation established by the Starfish nuclear explosion was seriously underestimated in the calculations which preceded the experiment. Synthetic detergents were committed to full-scale economic exploitation before it was discovered that an important fault—resistance to bacterial degradation in sewage systems—would eventually require that they be withdrawn from the market. The hazards of pesticides to animal life were not fully known until pesticides were massively disseminated in the biosphere; the medical risk to man has hardly been evaluated. Nuclear tests responsible for the massive distribution of radioactive debris were conducted for about 10 years before the biological effects of its most hazardous component were recognized.

It is a major responsibility of science to provide society with a proper guide to its interaction with nature. Apparently, in modern circumstances, science has not adequately met this responsibility, and it becomes important to inquire into the possible reasons for this defect.

There is a common tendency, in the execution of large-scale experimentation and technological operations, to neglect the principles of disciplined experimentation, of consideration for experimental controls, and of open disclosure and discussion of results. These erosions in the integrity of science reflect important changes in the relationships between the acquisition of new scientific knowledge and its use for the satisfaction of social needs. What are these changes and how do they account for the disturbing tendency to use the enormous power of modern science in the absence of adequate knowledge?

In the last 20–30 years an important change has taken place in the relationship between basic science and its technological application to social needs. During the period of intensive industrialization in the 19th century, basic scientific investigation was a major source of the required fundamental knowledge, especially of mechanics, electricity, and chemistry. In this period, basic scientific investigation pursued a course largely separate from current industrial needs, and practical applications developed only as a by-product of basic research or through chance discovery and invention. The development of electric power is a useful example: about 35 years elapsed between Faraday's basic scientific discovery of electromagnetic induction (1831) and the development of the first commercial electric generators (1866–67). Power systems developed gradually thereafter, as their usefulness became increasingly evident. The

first commercial power plant became operative in 1882, in New York City. Such delays permitted the development of a broad base of scientific knowledge in advance of its large-scale application. For this reason, and because the effects under scientific control in that period were, in any case, limited in intensity and scope, technological application of scientific advance was gradual and relatively disciplined. There are, of course, important exceptions to this generalization: the discovery of radium and X-rays led to certain abuses; the consequences of new industries for public health were often poorly understood; steam-boilers exploded, railroad bridges collapsed, and steamers foundered in the seas. But these defects were limited in effect and duration.

The present situation is very different. Now, the origin of technology in basic science is clearly understood and consciously exploited. Major socially useful applications are no longer based on the fortuitous appearance of the relevant scientific knowledge. *Instead, a social decision to accomplish a particular technological aim is often made in advance of the necessary scientific knowledge, and the latter is sought for with the express purpose of achieving the desired technology and satisfying a stated social need.* The decision to land a man on the moon was hardly a fortuitous outcome of the search for knowledge. It was, rather, a decision, largely on political grounds, consciously to develop the basic and applied science necessary to achieve this particular technological accomplishment.

Under these conditions, the laboratory of basic science inevitably loses much of its isolation from cultural effects, and becomes subject to strong social demands for particular results. This new relationship has, of course, greatly reduced the delays which previously intervened between discovery and application. However, the new relationship has also had a less fortunate effect—*it has resulted in technological application before the related basic scientific knowledge was sufficiently developed to provide an adequate understanding of the effects of the new technology on nature.*

Experience with nuclear testing provides an illuminating insight into this relationship. Nuclear weapons have been developed as a result of a strong social demand for a particular physical effect: an extremely powerful explosion. But a nuclear explosion is not only a physical process; it is also a vast ecological experiment. Since the ecological aspects of nuclear explosions had no apparent immediate relationship to the socially-determined goal of the program, the development of basic knowledge regarding the physics of nuclear explosions greatly outstripped what was learned about their complex biological consequences. As a result, the knowledge required for massive nuclear explosions was developed—and put to use—in the absence of an adequate understanding of the biological hazards that followed.

We are constrained to ask why the long established precepts of science have not been sufficiently strong, despite insistent social demand, to

prevent such hazardous use of partial knowledge.

Secrecy, which has become ubiquitous in modern, large-scale science and technology, hampers the processes of scientific discourse. In the normal procedures of science, errors or inadequacies in scientific information are detected and corrected through open dissemination of results and free discussion of their significance. When secrecy intervenes, such inadequacies are not subject to the scrutiny of the independent scientific community and may go uncorrected for relatively long periods of time; faulty action may result.

Another source of present weakness in the integrity of science is that the social agencies which are responsible for the pattern of research support—especially Congress—do not yet appreciate the hazards involved in developing support for science on the basis of immediate demands for particular results. Support for science which does not permit the free and balanced development of all aspects of a problem tends to narrow the range of available scientific information and dangerously unbalances our control of new interventions into natural phenomena.

The National Science Foundation was intended to provide just this sort of broad support for science. However, as a result of the rapid development of a massive space research program by NASA, the relative strength of science-oriented support has become significantly reduced. If this process is unchecked, it will further reduce the ability of science to develop a comprehensive understanding of large-scale interventions into nature, and constitute a further erosion in the integrity of science.

Even where an adequate balance of information regarding physicochemical and biological aspects of modern science exists, inadequate interdisciplinary communication often limits the effective use of this information. Had the developers of the present synthetic detergents been aware of available information regarding the relative resistance of branched molecules to biochemical degradation, they might have hesitated to impose a massive burden of such substances on the biosphere.

We see, in some recent developments, a noticeable tendency toward the separation of "modern" aspects of science from more traditional fields. There is a tendency to look upon synthetic chemistry or the biophysics of radiation effects as "modern science" while the fields of biology that are more closely related to natural phenomena—for example, ecology—are often regarded as old-fashioned and outmoded. Nevertheless, it is the complex natural system which is disturbed by the untoward secondary effects of massive physicochemical technology. Derogation of the sciences devoted to the study of natural systems is hardly conducive to the proper understanding of the consequences of the large-scale experiments that are so typical of modern science.

The scientist's position in society has changed considerably in the last decade and some of these changes have influenced the integrity of science.

Because of the rapid increase in the importance of science to major national needs (military, economic, international), scientists have been drawn into extensive participation in business, government agencies, and public affairs generally. The public has become willing to accept, with the respect accorded scientific conclusions, the scientist's view on numerous topics that have nothing to do with his special area of competence, or with science as a whole.

The scientist now often finds himself, by virtue of being a scientist, in a powerful position to influence social decisions which are not solely matters of science. For example, most major policy decisions about the space program require social judgments. Although scientists, as a group, have no greater competence or rights than other citizens in such matters, their close association with the space program has afforded them opportunities to exert disproportionate influence on these decisions. Scientists have also played a major role in advising the government on the development of the nation's military strength and on important international negotiations. Since such advice almost always involves non-scientific matters, which are not subject to the self-correcting effects of scientific discourse, there are often serious disagreements on these issues among different scientists and groups of scientists.

When scientists serve as advisors to a governmental or private agency which is committed to a particular point of view on a public issue, questions also arise concerning the influence of the parent agency's viewpoint on the advice given to it. Where such advisory bodies operate under rules of secrecy, or for some other reason do not make their deliberations accessible to the scrutiny of the scientific community, the normal self-correcting procedures of scientific discourse cannot be brought to bear. Conflicts may develop between the advisory group and other members of the scientific community regarding scientific matters, or the significance of non-scientific considerations. Such disagreements are difficult to resolve because, in the absence of open discussion, they do not become explicitly stated.

The growing interaction between science and public policy requires considerable attention to the problem of distinguishing scientific problems from those issues which ought to be decided by social processes. An example of the tendency to confuse scientific evaluation with social judgment is the matter of radiation standards. Here, a scientific body, the Federal Radiation Council, is engaged in setting standards of acceptability which are basically social judgments regarding the balance between the hazards and benefits of nuclear operations. These judgments are, or ought to be, wholly vulnerable to political debate, but their appearance in the guise of a scientific decision may shield them from such scrutiny.

VI. The Responsibilities of the Scientists and of Society

Under the pressure of insistent social demands, there have been serious erosions in the integrity of science. This situation is dangerous both to science and society. If society is to enter safely into the new age of science, steps must be taken to strengthen the competence of science, as a reliable guide to nature, at its source—the integrity of science. Scientists have an inescapable responsibility to maintain the integrity of science; this is required by their duties to their own discipline and toward society.

The set of principles and processes which comprise the integrity of science have been developed in a long and sometimes difficult history. The scientific community has a strong tradition of protecting this system of inquiry against insistent pressure or overt attack. This history has its heroes, its martyrs, its defeats. The lesson of both victories and defeats is the same. Wherever science has succumbed to social pressures that would subvert the objectivity of scientific data, foster perverted or undisciplined experimentation, or restrict the freedom of scientific discourse, the price has been paid in a costly coin—knowledge. The relationship between the integrity of science and knowledge is evident in each of the instances we have cited. In each of them, defects in the integrity of science have led to inadequacies in our understanding of nature.

What can be done to strengthen the integrity of science? In our view, replacement or even any extensive modification of the system of inquiry which science has developed is not required. On the contrary, each instance which has led to an erosion of the integrity of science has arisen not because the system was tried and failed, but because it was *not fully used.* We must find ways to ensure that this system of discourse is fully engaged in the development and social use of modern science.

Some will assert that the only effective way to insure that science adheres to the principles of objectivity, of open discussion of results, and of disciplined experimentation is to see to it that all scientists develop a personal devotion to these principles. This view suggests that to strengthen the integrity of science we need but to promulgate a suitable code of ethics for scientists.

However, despite the importance of the scientist's personal outlook in sustaining the integrity of science, this viewpoint is largely a reflection of the system of discourse in which the scientist must operate. We believe, therefore, that steps to strengthen the integrity of science should be centered, to begin with, on the *system* rather than on its participants.

The basic problem which we now face is the effect of the rapid expansion in the scope of scientific research and technology on the integrity of science. The scale of new scientific and technological operations places new strains on the process of scientific discourse. What are required, are means for bringing these new activities into more effective contact with the system of discourse that science has already established. Large-scale

experiments and technological operations that might lead to violations of the principles of disciplined experimentation, or which might interfere with controls necessary to other research, need to be exposed to open consideration by the scientific community *before* they are undertaken.

Such operations are important parts of governmental or industrial programs, and we recognize the difficulties which may inhibit their open discussion. Nevertheless, we see no way to ensure that these activities can be carried out without jeopardizing their ultimate value to the government or to industry, and to society generally, unless they *are* subject to the corrective effects of open discussion. Military or commercial secrecy seriously hampers proper development of large-scale scientific intrusions upon the natural world, and we believe that our society must now take decisive steps to reduce these restraints.

Any effort to minimize such secrecy will, of course, be confronted with serious and complex difficulties. Nevertheless, the need for open scientific discussion of the consequences of modern experimentation and technology is so urgent that the task, despite its difficulty, should now be undertaken.

To correct the difficulties which arise from present confusions between scientific opinion and social judgment, we can again rely on the principle of open discussion. In such discussion the scientific community should require that any public assertion, whether made by a scientist, by a government agency, or by a political figure, which makes use of scientific considerations include, if only by reference, verifiable sources for the latter. If a scientist, whether speaking professionally or as a citizen, asserts that fallout represents a calculable risk to health, he should be expected to indicate what scientific observations support this conclusion. If a public official declares that a particular public policy, such as exploration of the moon, is required for the advancement of science, he should be expected to indicate where the supporting scientific considerations may be found. The development of this type of discourse should do much to reduce the confusion between scientific evidence and social judgment and thereby help to restore the slackening public faith in the integrity of science.

All citizens bear serious responsibilities toward maintenance of the integrity of science. The basic social function of science is the development of objective knowledge about the natural world and of means of directing this knowledge toward the satisfaction of human needs. As we have emphasized, science has developed a set of principles and procedures which are designed to maximize the search for such knowledge, which is actually the unquestioned goal of science. But, in the rest of society, not every person or every institution may be equally well served by a particular scientific conclusion. A physician who asserts that smoking is harmful to health will gratify many parents, but will also displease cer-

tain farmers and manufacturers. A recitation of the hazards of nuclear testing, or of the untoward effects of a government-sponsored experiment in space on science may be resented by those who wish to promulgate these activities. Science needs to be protected from such constraints by being permitted to develop and disseminate knowledge in keeping with its own procedures, so that the usefulness or desirability of this knowledge to society may be determined by that society. This is a responsibility of all citizens, including those who are scientists.

In its own self-interest, society must respect, and indeed encourage, the integrity of science. Too often science is regarded only as a means of satisfying immediate social demands, and such demands sometimes produce pressures which erode the integrity of science. Society must recognize more clearly than it now does, that such pressures are self-defeating, and, given the hazards involved in a faulty understanding of the power of modern science, exceedingly dangerous as well.

If scientists work to strengthen the integrity of science, and if citizens learn to respect the importance of the integrity of science to society, we can enter the new age of science in the hope that it will properly serve the welfare of man.

Acknowledgments

The Committee is especially grateful to the following for valuable assistance in developing information relative to the report: Dr. Leo Goldberg, Dr. William Liller and Dr. A. E. Lilley, all of Harvard University, and Dr. J. A. Van Allen of the State University of Iowa. The report does not, of course, reflect their opinions, nor are they to be regarded as responsible for any errors of fact which may appear in the report.

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AMERICAN SCIENTIST, 53, 1965

LEMMINGS AND POPULATION PROBLEMS

By GARRETT C. CLOUGH

THE BEST scientific problems are never completely solved because they always lead to new questions. A good scientist seldom contemplates any accomplished piece of work without seeing new unsolved problems, scarcely suspected before, rising from it. A good example is the subject of lemmings, the legendary rodents which march into the sea—or at least so folklore proclaims. In recent years, the lemming question has been removed from the realm of myth, and biologists have collected much new information to clarify and redefine the problem. But study has uncovered new mysteries, and this particular biological phenomenon now appears even more complex and provocative than before.

The periodic wanderings of masses of Norwegian lemmings (*Lemmus lemmus*) in the mountain regions of Scandinavia stimulated many imaginative and superstitious explanations in former centuries. Some of these tales persist today. While I was in Norway in 1963 and 1964, some people questioned me about the suicidal urge which is supposed to cause the lemmings to fling themselves into the sea, or inquired if I had seen any angry lemmings burst apart with excess rage. Though serious scientists have long since dismissed such legends, the fanciful myths have been replaced in the scientific literature by a set of equally imaginative hypotheses. These have been built by critical biologists upon firm observations and experimental results, but they remain temporary ideas suggesting what new studies should be made in order to learn more. Much of what we have learned, in addition, may have bearing on the problems created by human population increase and rapid technological development of our environment.

Two facets of the lemming's life are of tremendous interest and importance. The first is that their population numbers change so drastically from time to time. The second is that, occasionally, great numbers of lemmings move considerable distances from their original homes. These two aspects of lemming biology may not be so closely related as was once thought.

The Norwegian lemming is one species of a rodent sub-family (*Microtinae*) whose 70 or so species are very common throughout the north-temperate and arctic zones. Various mice, voles, and lemmings are included here, all with the same general appearance of compact body, short neck, and small ears and tail. Most of these small rodents exhibit marked fluctuations in their numbers over periods of 3 to 4 years. Sometimes, as in an outbreak in Oregon and Washington in 1959–60, some of these animals will become so abundant that they cause much damage to man's crops. Five species of this group are called lemmings—three

in the New World, including Greenland, and two in the Old World. Lemmings are arctic residents.

A Peak Lemming Year

My wife and I were fortunate to begin our study in the middle of a peak lemming year. We arrived in the Dovre mountains of south-central Norway in June, 1963. Our home for the next two summers and periodic week-long trips throughout the winter was a small cabin just below the timber line. The first summer, lemmings were everywhere—under our cabin, in the fields next to us, along the stream from which we took our water. The colorful little animals ran from our path wherever we walked, whether through the upper forest zone of low birch trees, on the wet bogs,

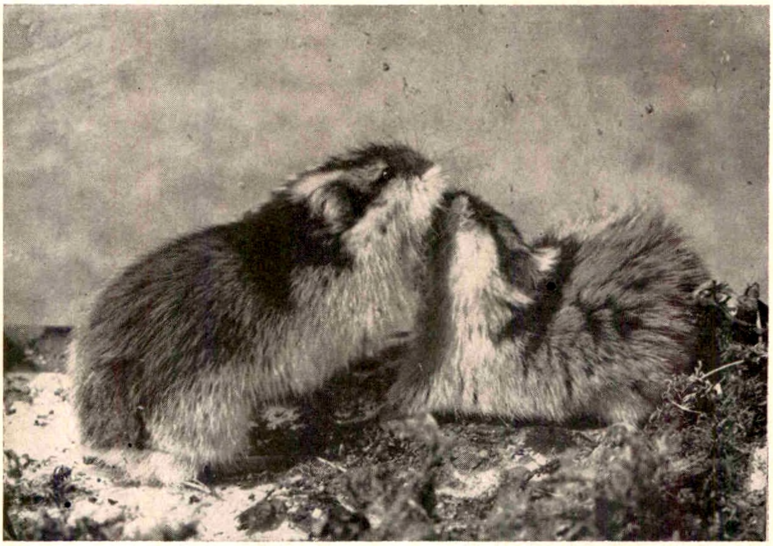

FIG. 1. An early stage in a lemming fight. The approach of the one on the left is being repelled with a defensive threat posture and squeaks of the animal on the right. Most of the encounters between wild lemmings never proceeded beyond this stage.

or over the high open tundra. Lemmings were the most obvious animals of the landscape. For the previous ten years, lemmings had been virtually absent from this area. Biologists had captured a few by extensive trapping in 1960, but most residents hadn't noticed any at all in the past decade. Then skiers first saw some animals above the snow during the 1963 Easter holidays. For a few weeks at the end of April and beginning of May, during the time of snow melt, wandering lemmings were seen by local residents in the birch tree belt. By the time we arrived on June 8, the animals had apparently settled down. I found them plentiful from the sub-alpine birch belt and above the tree line in the low-alpine dwarf

shrub belt to the mid-alpine grass-heath belt. The large adults which had survived over the winter were all sexually mature with the females either bearing young or suckling new-born. The small size of the juveniles indicated that these probably had been born after the spring wandering period. Four species of voles of the genera *Microtus* and *Clethrionomys* were abundant also.

A lemming year is accompanied by remarkable changes in the numbers of many other animals, too. These small rodents feed upon plant material exclusively—in the case of the lemming, on moss, sedge and grass. They occupy the basic animal position in the food chain which converts vegetable matter into animal flesh. The snowshoe hare, certain small birds, the willow grouse and the ptarmigan share this position with them. Predatory animals depend upon these herbivores for their food supply.

This summer, the lemmings and voles provided an extra-abundant source of food for the various arctic predators. There were ten successful nests of short-eared owls on the high marsh close to my study area. A Norwegian ornithologist told me that no owl nests had been found there in the last eight years. Rough-legged hawks and marsh hawks were more numerous than usual, and the nests I visited had many lemming and vole carcasses in them. The rarer snowy owls nested in the higher mountains to the south, where lemmings were also abundant. Foxes and weasels were plentiful and could sometimes be seen hunting in the daytime. The attention of these predators was so largely concentrated on the rodents that the grouse and ptarmigan were left relatively free to raise large broods to maturity in the summer, and in the fall the hunters had an exceptional season.

For many years the cyclic nature of the lemming populations has been known [1]. Most records of northern fur trapping show a fluctuating number of pelts. In Norway, the weasel and fox returns follow the lemming years closely. The Hudson's Bay Company fur records from Canada show regular fluctuations in lynx, fox, and weasel pelts. Population changes in bird predators are also noticeable. In the northern United States, winter invasions of snowy owls from Canada follow the decline of the lemmings and other small rodent populations farther north.

Population Theories

The attempted explanations of small mammal cycles may be put into three groups. The first approach looks for the cause of cycles in the environment. If some environmental factor, either physical or biological, could be found which varied in its expression at the same rate as the animal numbers, then a strong case for a causal relationship could be established. Such factors as temperature and rainfall, sunspots, lunar cycles, predator populations and disease organisms were examined. Although some of these may influence the rodent numbers, none of them

has been shown to be consistently connected to the cycles. This type of theory has been dismissed as the sole explanation by practically everyone who has looked into the question.

A second group of theories developed the idea of an interaction between the rodent population and some aspect of its biological environment. The rodent numbers and the other side of the balance were supposed to have an oscillating relationship. In these theories, the small mammals were considered to be one part of a dynamic predator-prey, parasite-host, or plant-herbivore interaction. We can imagine a simplified

FIG. 2. The author observes lemmings on their home grounds in the upper birch woods in south-central Norway. In Scandinavia, the trees growing at the highest altitudes are birch, not spruce as in the United States.

example of the latter case. Suppose the rodents are so numerous that they eat up all of their food supply. Most of them will die from starvation after the plants have been destroyed. In the following years, the grasses recover to full growth and the rodents can begin to increase again with the renewal of food. In a few years, the high powers of rodent procreation tip the balance all over again. A number of biologists have evidence which seems to fit such theories [2, 3, 4].

The third and most recent approach to the problem places the cause within the population itself. These are the theories of self-regulation or self-limitation which propose that the forces which promote increase are strongest when the population is lowest and weakest when the population is highest. This is the familiar concept of feed-back, applied to an ever-changing population.

A self-regulation theory was first suggested as an explanation of animal cycles by J. J. Christian in the United States in 1950 and has since

stimulated much work. Three things determine the numbers of animals of a single species at one place: the birth rate, the death rate, and the movements of individuals to and from the place. This group of theories argues that the birth and death rates are dependent in some way upon the numbers of animals present. Social stresses—such as the amount of fighting, interference with maternal behavior, or general antagonistic contacts between individuals—may increase as the population density increases. There has been ample evidence in laboratory rodents that such social stress acts detrimentally on various internal organs of the individuals [5]. The adrenal glands of animals experimentally exposed to crowded conditions or to trained aggressor animals [6] usually enlarge in the same way that they do when an animal is exposed to most physical or chemical hardships. According to these theories, the adrenal change makes the animal more susceptible to additional stresses and strains of life. As the body's master gland, the pituitary, is called upon to support additional adrenal hormone production, it reduces its own output of the hormones which serve in turn to develop the reproductive organs and hormones. Thus, fewer young may be conceived; and those that are born may not be so healthy or the mother may fail to care for them properly. This may be the key to the declining birth rates and mounting death rates which often culminate in a precipitate drop in the population. These ideas have much support [7], but are not yet proven.

There is a balance among the endocrine glands within one individual just as there is among the individual members within a species population and among the various species of plants and animals within a complex natural community. These three systems of increasing size—a single animal, a species population, and a community of plants and animals—are similar, in that each system is composed of many parts so organized that a change in one part results in a compensatory change in other parts.

Today, support and criticism for both the self-regulatory theories and the plant-herbivore balance theory can be found. Much of the evidence for the self-regulation comes from laboratory experiments on semi-domestic animals. It is much more difficult to conduct critical experiments or to gather sufficient material on wild populations. Some of the data concerning endocrine glands and resistance to stress of wild animals do not agree with the theory. For example, the weights of adrenal glands of Canadian lemmings from peak populations are no different [8], or lower than [9] those of lemmings captured at periods of low population. In earlier work I have done [10], there was no difference in the resistance to stress or in adrenal weight between free-living voles in very dense populations and those from sparse conditions, after allowing for the variability due to season and reproductive condition. The foremost Scandinavian student of lemmings, Dr. Olavi Kalela of the University of

Helsinki, emphasizes the plant-herbivore balance. There are now some hints that the plant food of lemmings may exhibit fluctuations in nutritive quality which in turn influence birth and death rates of the lemmings [11]. If these ideas are true, then the entire cycle problem is moved down one level and includes a problem of plant cycles. The men who have spent many summers studying lemmings in Alaska also place considerable emphasis on the plant-lemming interaction, for there the lemmings sometimes devour their entire food supply [2].

FIG. 3. These young rough-legged hawks have been supplied with more lemmings than they can consume.

Meaning to Humans

Whatever the underlying mechanisms are, the cyclic changes of lemming numbers have been attained after a long evolutionary history since the Pleistocene glaciers or even earlier. As might be expected, the ideas devised to explain animal cycles—especially the finding that life in crowded conditions can have profound physiological effects—are now being used to discuss human population problems. But, in my view, there are too many basic differences to justify much of this speculation. For one thing, historically, human populations have shown only a steadily increasing growth over thousands of years—or no significant change in the case of some isolated peoples. There has never been the regular, short-term rise and fall seen in the rodent populations. For another difference, although it is probably true that humans crowded into urban centers are plagued by certain mental and physical diseases of

civilization, their birth rates are not greatly inhibited (if at all) nor their mortality rates increased. In fact, the birth rates are comparatively high among the people who live with the poorest conditions of nutrition, housing and, perhaps, even emotional and mental hardships. Finally, I can see no evidence of an evolutionary adaptation for over-all limitation of human numbers, either by self-regulation or by a balance with the environment. By cultural and technological revolution, humans have so rapidly outgrown their original niche in the balanced system of nature that we no longer know *where* we belong in the ecosystem of the earth.

Great as the differences are between man and lemmings, we can learn some things about human populations by studying lemmings. Certain physiological comparisons are valid, for we and the lemmings are mammals who share many of the same biochemicals and internal functions. We are learning a great deal about how animal numbers are regulated in nature, about the mammal's physiological reaction to stress, about the delicate functions of the integrated hormone system, about the organization and function of natural communities, and about the evolutionary adaptations to various environments. In addition, when we learn to know intimately the lives and histories of wild creatures which we use as inspirations in our stories and mythologies, we often can look upon ourselves and our fellow men with a greater perception, appreciation, and tolerance. Biologists also study lemmings to satisfy deep personal curiosity about important unsolved phenomena of nature, partly because the animals are such delightful, pretty creatures to watch, and partly out of a desire to travel into the peaceful, adventurous arctic tundras and beautiful lonely mountains of the north.

Mass Wanderings

What makes the lemming famous to the general public is the animal's spectacular mass emigrations. Sensational accounts of great hordes of lemmings moving down from the mountains—some of the stories date back to the sixteenth century—are based on actual events, though the movements are by no means so overwhelming as they are sometimes painted. Finnish ecologists, working in the northernmost tip of their country, have learned that lemmings actually make two trips annually, usually short journeys made from the winter habitat to the summer home [12]. These relatively short trips occur during April and May and then again in late summer and fall before the snow comes. In the summer, lemmings prefer wet peat lands where the grass and moss grow best. In the winter, when these places are frozen, they prefer drier areas where a dense snow cover protects them and their food supply. Often the movements occur vertically up and down the mountain slopes. These biennial treks between summer and winter homes take place regardless of popula-

tion density, and may be a special phenomenon which evolved in the Norwegian lemmings to make best use of the topography, vegetation types, and the climate of the European arctic regions. The species of lemmings inhabiting Canada, Alaska, and Greenland seldom move from their homes in this manner. The topography there is flatter and the vegetation much more monotonous than in Norway. Since the cyclic fluctuations of lemming numbers are not a special characteristic, but one shared to some degree by all the small mammals from the temperate zone northwards, it appears that the underlying explanations for these two events are fundamentally different.

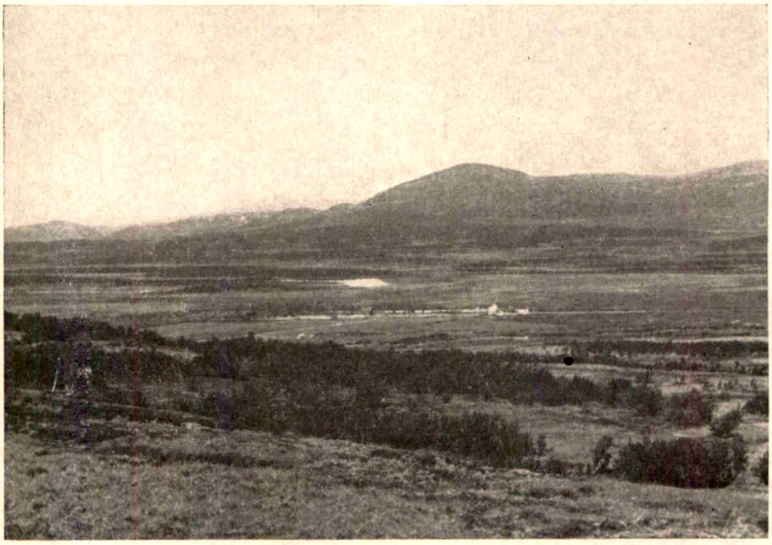

FIG. 4. A view from timberline in the Dovre Mountains. In late summer lemmings wandered down the slopes onto the fields and marshlands.

But all is not clear, by any means. Sometimes lemmings do behave in a way which seems completely maladaptive or neurotic. The peak population which I watched all through the summer began to move downward from the alpine shrub and lichen zones rather suddenly in mid-July. During the short night and early morning hours, I could see individual animals proceeding at a steady pace down along a hiking trail. While I sat at a convenient observation point where a road crossed a small river, as many as forty lemmings per hour passed by. These animals were not heading toward a good wintering ground. In fact, by September and October, when the first snows came, many of them had settled in the large, low-lying marsh and in a hayfield devoid of green grass. None of them survived the winter here. Other wandering lemmings began to appear at the nearest town and surrounding pine forests twelve miles away down the valley.

Social Antagonism

The great antagonism and aggressiveness in these lemmings probably plays an important role in such irrational wanderings of a portion of the peak population. It was easy to learn something about the detailed daily activities of some lemmings because, unlike many small mammals, they were readily seen in the daytime. Perhaps their broken black and light coloring serves as disruptive camouflage and makes them willing to expose themselves in the open. By marking lemmings with paint and by clipping away bits of fur, I could identify individuals on sight as I sat quietly in the birch woods.

Lemmings must be the most antisocial and antagonistic of all rodents. Every single time I saw two lemmings meet face to face, there was some sign of agonism (attack, defense, or escape behavior). At the mildest level, this agonistic behavior took the form of a slight squeak with the assumption of a threatening upright posture by one animal, and a corresponding low submissive stance by the other. They might then separate or might proceed into the second stage which involved mutual pushing of the forefeet in a boxing bout. In the wild, animals either parted peacefully at this stage or one animal turned and fled with the other in pursuit. Loud squeaks usually accompanied the boxing and chase. These encounters were very frequent during the summer. Newly-weaned litters were continually being added to the already crowded active ranks. At times, I saw five or six individuals sitting or eating at the same moment within fifteen feet of me. During observation periods on five days in mid-July, I heard an average of 5.6 squeaks per hour but this had declined to 1.8 squeaks per hour during five days at the end of August. The first period represents the start and the second period represents five to six weeks after the start of the wandering movements.

The large mature females were always the winners in the conflicts I witnessed in the birch woods. They chased away everyone, from males larger than themselves to young recently emerged from the nest. Perhaps these dominant, aggressive matrons triggered the social stress within the population. In any case, it seems reasonable to assume that all this social uproar accounted for the wanderings of part of the population. Only middle-sized lemmings were seen wandering at first. The very young and the older breeding segment of the population stayed behind at the good homesites where food was still available. At the beginning of the wandering period, two-thirds of the animals were males. Almost all of these males and most of the females were sexually mature, but the females were not pregnant. By the end of August, the sex ratio among the wanderers was equal and a few smaller, sexually immature animals were included. Once on the move, lemmings were antisocial to an extreme. They moved completely alone. Whenever two wanderers met by chance, there was squeaking and rapid avoidance. On a few occasions, I placed a

lemming in a small cage of wire netting on the emigration path. Lemmings who came moving downhill did not change their pace or direction even if they walked within a foot of the caged animal The aggression displayed among those who settled in the open hayfields was very great. Here they could not avoid each other and we could see and hear them outside our cabin, running, chasing, and squeaking at each other.

The agonistic encounters among the birch wood residents or between two wandering lemmings never reached the stage of biting and rolling together in a fight as it invariably did between captive lemmings confined to a limited space. I did not find any wild lemmings with scars and

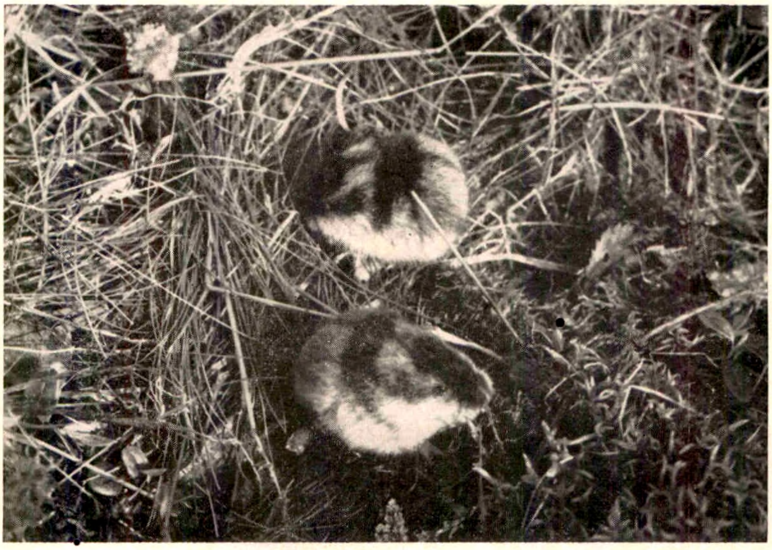

FIG. 5. Three week old lemmings are well able to care for themselves. Their eyes open at 12 or 13 days of age and weaning is over during the third week. More than twelve young can be born at once but the usual litter size is 5 to 8.

wounds. The difference between behavior of free-living and confined animals deserves greater emphasis in evaluating evidence on population problems. Whenever I placed two wandering lemmings together in a small cage, they fought savagely until one of them died, always within 24 hours. They met face to face and exchanged bites directed at the mouth and chin, their sharp incisors easily tearing through the skin. Even in large floor pens (four by eight feet), with plenty of food and shelter, six or eight lemmings would fight so much that all but two or three would be dead in less than a week. At the time of death, they were severely wounded and emaciated. In captivity, voles are much more tolerant than lemmings. I could keep ten bank voles together in a small cage. After a few

hours of shoving and squeaking they would quiet down and the next day would all be huddled together in a nest in one corner. Ten meadow voles, placed in a floor pen of 15 square feet, will fight and wound each other slightly but all will survive if adequate food and cover is provided [13].

Generally, the social structure of a population of mice and voles is based upon a spacing-out of the individuals in suitable habitat. Each individual has a limited range of movement—about a quarter of an acre, depending upon season, sex and type of habitat. Mature females tend to defend their own territory and to prevent other females from encroaching on it. Males have larger home ranges which usually overlap those of a few females. When the numbers of our common wood mouse increase, some individuals seek space in less favorable places nearby, or the home ranges and territories may be compressed [14]. Sometimes, the territorial defense of field voles may relax enough to allow two females to live amicably together, or the offspring of one mother to remain in her range even after she has another litter [15]. This does not seem to happen among the Scandinavian lemmings.

Open Questions

It is still unclear, if behavioral differences exist between individuals in low and high density populations, how great and how significant they are. Perhaps the social stress among the adults is reflected in the new-born. Prenatal influences have been demonstrated in white mice and rats in the laboratory [16, 17]. Young white mice, born to mothers who had been subjected to crowding or obnoxious stimuli, were different in some behavioral responses from those born to uncrowded and undisturbed mothers. The experimental young were less active and slower to explore unfamiliar areas than the control animals. However, different results were found in a comparable study with laboratory rats [18]. If it were true that prenatal influences were acting on wild lemmings born in peak years, these animals may be less able to adjust to the crowded conditions than their parents were.

It has also been suggested that behavioral changes through the generations might be related to genetic changes [19]. That genes may affect agonistic behavior and other behavioral characteristics of mammals is known [20]. The pattern of response to social encounters may also be partly the result of learning by the young after they leave the nest and meet dominant adults. Frequent experience of conflicts may reduce their tolerance towards other lemmings. Aggressiveness, like all other complex forms of behavior, is formed by close interaction between inherited (non-learned) and acquired (learned) factors [21]. This whole discussion must be highly speculative since the experimental evidence on these questions is meager.

Only a portion of the lemmings in my study area undertook the

wandering which led them into unsuitable habitats. A certain portion may have maintained their regular habits with the seasonal change of habitat. After the winter snow cover had arrived in late October and all wandering had ceased, I found lemmings living in all habitats; good and bad, from the lower pine forest, on the marsh and hayfields, in the birch woods, and above in the low and mid-alpine belts. But my traplines already were showing decreased populations. The population decline had started.

When I returned to the mountains on the first of March, I did not find a single lemming. In one week's time I saw only a few rodent tracks in the snow. This astounding disappearance of the lemmings was verified in

Fig. 6. The Dovre study area in March when the lemming population crash was nearly complete on all habitats.

the spring. Although we spent the entire months of May and June in a thorough and complete search, we found only five bank voles where the year before, lemmings and voles had overrun the ground. Traces of lemming work under the snow and a few decaying carcasses were present in all habitats but the overwinter die-off had been virtually complete.

There had been nothing severe in the winter weather to cause this population crash. Only sixty miles to the northeast, in the valley on the other side of the Dovre mountains, lemmings were still plentiful. There, the lemmings had successfully survived the winter in pine forests, birch woods, and even on close-cut hay fields. During the month of May, 1964, they wandered along the roads and in the woods.

I do not know the cause of the mass mortality of the lemmings. Lemmings and other microtine rodents do not always decline from a population peak so rapidly or always during the winter. The decline may occur slowly over a two-year period and, if a short crash does occur, it may be at any season of the year [22]. Much must be learned before the mysteries of the lemmings are solved. Whether the social stress in high density populations is a strain on the endocrine system, as it is in some animals, is still a question. Studies on the quality of wild rodents' food supplies are just starting. Lemming problems should be followed on all biological levels, from the organs and hormones within the individual to the complete community of soil, climate, plants, and related animals. Work in biochemistry, endocrinology, genetics, physiology, nutrition, ecology, and animal behavior will contribute to future studies of these problems.

The lemmings, as a species, are remarkably successful residents of the harsh arctic regions despite, or rather because of, the fact that their population explosions are so extravagant with individual lives. Without these periodic irruptions of individuals, the species might have met, long ago, either of two fates leading to extinction. With a lower capacity for reproduction, the species might not have survived the especially severe climatic hardships which occasionally occur. A greater reproductive capacity, coupled with better ability to tolerate social stress and crowding, might have led to total destruction of their food supply. As things are, the hordes of lemmings present during peak years never irreversibly damage their resources. Most of the individuals are lost, but some are always left to reestablish the populations. Behind the drastic periodic changes in lemming numbers measured in terms of years and decades, there exists the fundamental stability of the arctic community from times before the appearance of modern man.

One important thing the non-biologist can learn from all this work is an appreciation of ecological complexity, of the interdependence of every living creature. Even the self-regulation theory of animal cycles does not, by any means, imply that a population of animals is independent. I have already noted that lemming events have tremendous repercussions for many animals about them. The life of a single animal or plant, including the human, is understandable and definable in terms of a group of individuals occupying a certain place in a dynamic, living community. Knowledge and understanding of this fact seem very hard to get across to people all over the world, whether in the highly educated countries or in the semiliterate ones. And yet it is just this sort of appreciation which is needed to guide so many of our own activities. Man is one part of the whole system of nature. We have become such a powerful species that our activities have powerful effects, often resulting in great compensatory changes or even destruction in the ecosystems that we touch.

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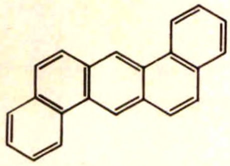
AMERICAN SCIENTIST, *53*, 1965

CHEMICAL CARCINOGENS, CARCINOGENESIS AND CARCINOSTASIS*

By NORMAN H. CROMWELL

CANCER has been defined by Willis [1] as an abnormal mass of tissue, the growth of which exceeds and is uncoordinated with that of normal tissues, and persists in the same excessive manner after the cessation of the stimuli which evoked the change. The study of chemical carcinogenesis has proven to be one of the most rewarding and definitive areas of cancer research. It was in 1932 that Kennaway [2] and his colleagues in England first demonstrated that cancer could be induced in experimental animals by a pure chemical. Many of the suspected environmental cancers of man have since been reproduced in animals.



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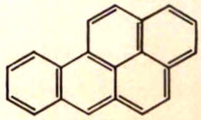

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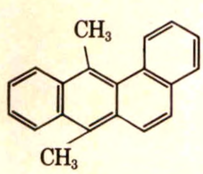

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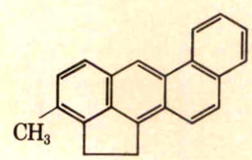

3-methylcholanthrene

In recent years, there has been a renewal and intensification of interest in the causes of cancer with the hope that a better understanding of the processes known as carcinogenesis (cancer formation) will eventually lead to the control of this scourge of mankind. Experimental cancer may be induced in three reproducible ways: by chemicals, viruses, and ionizing radiation. In this brief account an attempt is made to interrelate some contemporary facts and concepts concerned with the carcinogenic (cancer forming) and carcinostatic (cancer arresting) actions of certain organic chemicals, with special emphasis on the polycyclic hydrocarbon and heterocyclic compounds.

During the past several decades, a major effort in the general field of experimental oncology has been directed toward the discovery and

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identification of both carcinogenic agents and carcinolytic agents. During the latter part of this period, there has been a considerable shift in emphasis from simply synthesizing new agents toward the more basic question of the mechanisms of action for both of these classes of substances. There would seem to be no fundamental reason why, eventually, chemists should not be able to decipher both of these related processes in chemical terms. However, before this can come about, it may well be necessary for us to know more concerning the precise biochemical differences between normal and malignant cells and how these differences control cell division in each instance. Undoubtedly, both the causation of cancer and the control or reversal of malignancy will turn out to have closely interrelated mechanistic bases. Both for carcinogenic agents and carcinostatic agents, there must be a reaction between the agent and some component of the cell for cell growth to be inhibited. In both instances, the question is which component (or components) reacts and is responsible for the biochemical lesion that ultimately leads to the biological effect. Cancer seems to involve a change in the cells' hereditary mechanisms. A successful carcinostatic agent would be one which would either prevent or reverse this abnormal change in hereditary control of cell growth.

Of the three methods of treating established cancer: namely by surgery, radiation, or chemotherapy, only the first two can be said to effect cures, and these cures represent somewhat over one-third of the patients diagnosed as having cancer. If the best possible methods of diagnoses and treatment now available were to be applied, the cure rate might be raised to 50%. For the last 50%, it may well be that some form of cancer chemotherapy offers the greatest promise. It is of course also possible that we may eventually learn, through the study of carcinogenesis, how to eliminate from our environment or neutralize the effect of the major causative factors associated with the initiation and malignant propagation of human cancer.

It has been claimed that, from a practical standpoint, the major problem of finding a cancer cure is to learn how to fight against disseminated cancer cells hidden in remote parts of the body. It is felt by some that this will require a fight by chemical means. If it were not for the widespread dissemination of cancer cells in the patient, he could be cured by the removal of the localized tumor using surgery or radiation. Thus, it has been recognized that there is a great need to study the mechanisms of metastasis in detail, especially in laboratory animals.

Much more effort needs to be placed on the study of the mode of action of the drugs now known to have some beneficial effect. Of the some 20 to 30 agents now available for the chemotherapy of cancer in man, none provides an enduring cure except for very rare forms of

the disease [3]. The proper use of these agents, however, makes it possible to hold the disease in check in many cases and to relieve symptoms and prolong useful life. Agents with a much higher degree of selectivity need to be developed. Even the most effective agents available today are much too toxic to the patient. Either drugs with a lower level of toxicity must be found or new methods of administration must be devised which either protect the patient from the extreme toxic effects of the drugs or which potentiate the effectiveness of a lower non-toxic dose of the drug.

Some progress has been made in the more effective administration of known agents. Through use of the surgical techniques known as perfusion and infusion [4], localized portions of the body are treated with high doses of a cytotoxic agent while the rest of the body is protected by some neutralizing or counteracting chemical. Also, considerable progress in combination therapy, that is in the combination of the various mentioned methods, such as an application of chemotherapeutic agents after surgery to attempt to remove free-floating cells [5], and the combination use of cancer chemotherapeutic drugs with radiation to reduce more effectively tumor masses has been realized [6].

It was suggested sometime ago that it might be possible to discover special growth stimulants which would cause cancer cells to destroy themselves. Although this happy situation has not yet been realized, the synergetic response of animal tumors and tumors in man to therapy employing either growth factors (corticosteroids) or carcinogens (urethane) in combination with various anti-tumor agents has been reported by several groups of workers in the United States [6, 7, 8] as well as in other countries [9a]. It has been found that several types of compounds including known carcinogenic substances [8, 9a], or growth promoting agents [9b], are capable of sensitizing tumors to the growth inhibiting action of certain anti-tumor agents. These observations obviously interrelate carcinogens, carcinogenesis and carcinostasis.

Among the various drugs which have been found useful in the treatment and palliation of some types of disseminated neoplastic disease in man are chemical agents which have been classified as (*a*) cytotoxic agents, (*b*) anti-metabolites, (*c*) antibiotics, (*d*) hormones, (*e*) alkaloids, and miscellaneous compounds for which no basis of action has been suggested. It seems unlikely to expect that the mechanism by which these widely varying substances bring about their biological actions will prove to be precisely the same, unless we mean by "the same" the fact that a reaction between the agent, a metabolite or an induced substance and some component of the cell must be taking place when cell growth is inhibited. One may immediately decide that there are many modes by which this might come about. There has been no shortage of theories either to explain carcinogenesis by various types of agents or the car-

cinolytic effect (carcinostatic) of the better known anti-tumor drugs.

It must be admitted that theory has not yet played a very successful role in the development of cancer research except as a stimulus to further work. The developments which have been made in recent years have been based almost entirely upon empirical data. Many of the present-day theories are mere restatements of experimental or observational findings. The actual mechanism of carcinogenesis is still a matter of speculation. At the moment it seems promising to continue the study of carcinogenesis with the hope that, finally, an adequate picture will emerge which tells us what occurs in tissue and in the cell during abnormal development and growth. With this information at hand, we may then hope better to manage the disease in man.

The rebirth of interest in carcinogenesis has brought forth a wide variety of active agents of greatly different chemical properties. Such widely-differing substances as plastic films and metals on the one hand, and complex organic molecules, such as the biological alkylating agents, polycyclic hydrocarbons and polycyclic heterocyclic compounds, are known to initiate cancer. Despite the large variety of factors which may be involved in the initiation of cancerous growth, it appears that the necessary and sufficient alterations which take place with the genetic apparatus of the cells are similar in each case. The particular properties of the cancer cell are propagated to each succeeding generation even after the inducing agent is no longer present. Apparently, the chemical coding ability of the nucleic acid of the cell has been altered [10].

Theories of Carcinogenesis

Several books [11–14] have been written in the last few years which review the numerous theories and hypotheses which have been advanced to interpret carcinogenesis. In all instances, the authors conclude that these are as yet far from adequate. The most hopeful approaches to a solution of these problems still seem to lie along rather empirical routes such as the further discovery and detailed mechanistic study of agents known to take part in either or both the initiation and promotion stages of carcinogenesis.

One continuing approach to such empirical investigations has been the synthesis and biological study of new members of several series [15, 16, 17] of large flat molecules known as polycyclic heterocyclic compounds which have, in common with polycyclic hydrocarbons, the potential biological property of being rather strongly absorbed on protein surfaces [18, 19]. However, as has been pointed out previously, protein binding is not sufficient to explain carcinogenicity. Many compounds react with proteins but are not carcinogenic (i.e., acylating agents, dinitrofluorobenzene, etc.). The above-named compounds also

inactivate enzymes; however, they are not carcinogenic and they do not react with DNA [20]. There are many data which indicate that known carcinogenic hydrocarbons bind to proteins [18, 19], and although it has been widely suggested that these same carcinogenic hydrocarbons bind to nucleic acids, there was little information until recently [21, 22, 23] that this is actually the case [24]. It has been suggested that a possible function of a carcinogenic agent with the ability to bind to proteins may be its removal ("deletion") of a nuclear protein from coordination with DNA, thus allowing the DNA to "run wild" and initiate cancer [25]. The deletion hypothesis said that tumors resulted from the loss or deletion of an enzyme system responsible for the control of normal growth, possibly because of the combination between the carcinogen and protein. An alternative possibility would be the deletion of some repressor system [25b].

Clayson [26] has suggested that two theories which seem of most importance in considering a possible mechanism for chemical carcinogenesis are based on: (i) somatic mutation, or (ii) the immunological theory of cancer. In spite of the fact that many investigators appear to have assumed that carcinogenesis involves as a first step a mutation in chromosomal genes in the cell, there is evidence that powerful mutagens are not carcinogens and vice versa. Thus, it seems unnecessary to identify any portion of the carcinogenic process with mutation. Nevertheless, the change from normal to precancerous tissue during the process of initiation must involve a change in heritable features of the cell.

Green [27] considered malignancy to be the result of the loss of specific antigens by the animal. He suggested that the chemical induction of cancer takes place by two stages: (1) the interaction of the carcinogen with the specific antigens and/or other constituents of the animal tissues to form *complexes*, (2) an immune response by the host to the tissue—carcinogen *complexes* accompanied by the elimination of the tissue-specific antigen and the appearance of a neoplastic cell.

It has been generally found that all chemical carcinogens do combine with tissue constituents, i.e., proteins. Kobat [28] has suggested that antibodies react with only certain chemical groupings on the antigenic substance surface.

Some experiments [29] have indicated that the action of chemical carcinogens in newborn mice do not adequately protect them from later chemical carcinogenesis, i.e., no tolerance is acquired; however, see the section on tumor growth inhibition by polycyclic compounds and Ref. [30].

Indirect Carcinogens

Some effective agents appear not to be dircct carcinogens. Boyland [31a, b] has found that the aromatic amines, 2-acetamido-fluorene and

2-naphthylamine, are converted *in vivo* to the active compounds which are probably arylhydroxylamines which have chemical properties somewhat similar to the biological alkylating agents which are also carcinogenic. It was found [31b] that 2-naphthylhydroxylamine (50 mg/kg.) in arachis oil twice a week for three months, intraperitoneally, in 15 random inbred strains of albino rats induced six abdominal sarcomas, three abdominal carcinosarcomas of diverse histological types and one lymphosarcoma. An identical dose of 2-naphthylamine induced sarcomas in $2/14$ and possibly a salivary gland tumor in $1/14$. These results agree with the hypothesis that the 2-naphthylamine and some other aromatic amines exert their carcinogenic action after a metabolic conversion to hydroxylamine derivatives.

The arylhydroxylamines react with sulfhydryl compounds to form 5-aminophenylderivatives (many carcinogenic substances react with sulfhydryl compounds). A few years ago, the Millers and their co-workers [32] at Wisconsin showed that N-hydroxylation is an important reaction which leads to an active intermediate of carcinogenic amino compounds. Ring hydroxylation is the other chief metabolic reaction but seems to be a detoxification route. However, this area is still under active investigation [33] and the precise details of the mechanism of carcinogenesis by the aromatic amines have not been clearly established. A somewhat complicating factor is the fact that there are sex-linked differences in the metabolism of N-2-fluorenylacetamide by rats [34].

Boyland [31a] also suggests that the carcinogenic hydrocarbons, and presumably other polycyclic carcinogens, may be converted by metabolic processes (i.e., conversion to epoxides by oxygen) to carcinogenic derivatives; they may also be oxidized to dicarboxylic acid derivatives leading to combination with proteins and sulfhydryl compounds. These latter metabolic processes with hydrocarbons, however, are probably detoxifications and not involved in the carcinogenic effect which is probably initiated by a charge-complex formation of the purine bases of DNA with either the hydrocarbon itself or an epoxy or peroxy derivative.

Kotin and Falk [35] have studied a large number of organic peroxides and epoxides, and have concluded that they have a definite carcinogenic and radiometric (DNA chain breaking) effect. This has been pointed out in specific instances previously by several groups of workers and various members of the class have been known for sometime as effective experimental anti-tumor agents against transplanted tumors.

Cancer Forming DNA

Recently, a group of scientists from the Sloan-Kettering Institute for Cancer Research in New York, headed by Bendich, collaborated

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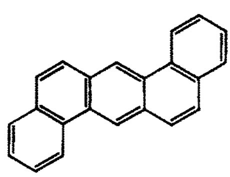

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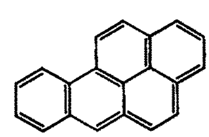

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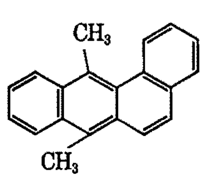

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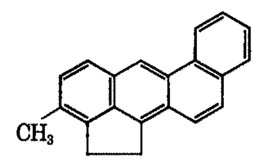

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During the past several decades, a major effort in the general field of experimental oncology has been directed toward the discovery and

* A Sigma Xi-RESA National Lecture, 1963–64.

identification of both carcinogenic agents and carcinolytic agents. During the latter part of this period, there has been a considerable shift in emphasis from simply synthesizing new agents toward the more basic question of the mechanisms of action for both of these classes of substances. There would seem to be no fundamental reason why, eventually, chemists should not be able to decipher both of these related processes in chemical terms. However, before this can come about, it may well be necessary for us to know more concerning the precise biochemical differences between normal and malignant cells and how these differences control cell division in each instance. Undoubtedly, both the causation of cancer and the control or reversal of malignancy will turn out to have closely interrelated mechanistic bases. Both for carcinogenic agents and carcinostatic agents, there must be a reaction between the agent and some component of the cell for cell growth to be inhibited. In both instances, the question is which component (or components) reacts and is responsible for the biochemical lesion that ultimately leads to the biological effect. Cancer seems to involve a change in the cells' hereditary mechanisms. A successful carcinostatic agent would be one which would either prevent or reverse this abnormal change in hereditary control of cell growth.

Of the three methods of treating established cancer: namely by surgery, radiation, or chemotherapy, only the first two can be said to effect cures, and these cures represent somewhat over one-third of the patients diagnosed as having cancer. If the best possible methods of diagnoses and treatment now available were to be applied, the cure rate might be raised to 50%. For the last 50%, it may well be that some form of cancer chemotherapy offers the greatest promise. It is of course also possible that we may eventually learn, through the study of carcinogenesis, how to eliminate from our environment or neutralize the effect of the major causative factors associated with the initiation and malignant propagation of human cancer.

It has been claimed that, from a practical standpoint, the major problem of finding a cancer cure is to learn how to fight against disseminated cancer cells hidden in remote parts of the body. It is felt by some that this will require a fight by chemical means. If it were not for the widespread dissemination of cancer cells in the patient, he could be cured by the removal of the localized tumor using surgery or radiation. Thus, it has been recognized that there is a great need to study the mechanisms of metastasis in detail, especially in laboratory animals.

Much more effort needs to be placed on the study of the mode of action of the drugs now known to have some beneficial effect. Of the some 20 to 30 agents now available for the chemotherapy of cancer in man, none provides an enduring cure except for very rare forms of

the disease [3]. The proper use of these agents, however, makes it possible to hold the disease in check in many cases and to relieve symptoms and prolong useful life. Agents with a much higher degree of selectivity need to be developed. Even the most effective agents available today are much too toxic to the patient. Either drugs with a lower level of toxicity must be found or new methods of administration must be devised which either protect the patient from the extreme toxic effects of the drugs or which potentiate the effectiveness of a lower non-toxic dose of the drug.

Some progress has been made in the more effective administration of known agents. Through use of the surgical techniques known as perfusion and infusion [4], localized portions of the body are treated with high doses of a cytotoxic agent while the rest of the body is protected by some neutralizing or counteracting chemical. Also, considerable progress in combination therapy, that is in the combination of the various mentioned methods, such as an application of chemotherapeutic agents after surgery to attempt to remove free-floating cells [5], and the combination use of cancer chemotherapeutic drugs with radiation to reduce more effectively tumor masses has been realized [6].

It was suggested sometime ago that it might be possible to discover special growth stimulants which would cause cancer cells to destroy themselves. Although this happy situation has not yet been realized, the synergetic response of animal tumors and tumors in man to therapy employing either growth factors (corticosteroids) or carcinogens (urethane) in combination with various anti-tumor agents has been reported by several groups of workers in the United States [6, 7, 8] as well as in other countries [9a]. It has been found that several types of compounds including known carcinogenic substances [8, 9a], or growth promoting agents [9b], are capable of sensitizing tumors to the growth inhibiting action of certain anti-tumor agents. These observations obviously interrelate carcinogens, carcinogenesis and carcinostasis.

Among the various drugs which have been found useful in the treatment and palliation of some types of disseminated neoplastic disease in man are chemical agents which have been classified as (*a*) cytotoxic agents, (*b*) anti-metabolites, (*c*) antibiotics, (*d*) hormones, (*e*) alkaloids, and miscellaneous compounds for which no basis of action has been suggested. It seems unlikely to expect that the mechanism by which these widely varying substances bring about their biological actions will prove to be precisely the same, unless we mean by "the same" the fact that a reaction between the agent, a metabolite or an induced substance and some component of the cell must be taking place when cell growth is inhibited. One may immediately decide that there are many modes by which this might come about. There has been no shortage of theories either to explain carcinogenesis by various types of agents or the car-

cinolytic effect (carcinostatic) of the better known anti-tumor drugs.

It must be admitted that theory has not yet played a very successful role in the development of cancer research except as a stimulus to further work. The developments which have been made in recent years have been based almost entirely upon empirical data. Many of the present-day theories are mere restatements of experimental or observational findings. The actual mechanism of carcinogenesis is still a matter of speculation. At the moment it seems promising to continue the study of carcinogenesis with the hope that, finally, an adequate picture will emerge which tells us what occurs in tissue and in the cell during abnormal development and growth. With this information at hand, we may then hope better to manage the disease in man.

The rebirth of interest in carcinogenesis has brought forth a wide variety of active agents of greatly different chemical properties. Such widely-differing substances as plastic films and metals on the one hand, and complex organic molecules, such as the biological alkylating agents, polycyclic hydrocarbons and polycyclic heterocyclic compounds, are known to initiate cancer. Despite the large variety of factors which may be involved in the initiation of cancerous growth, it appears that the necessary and sufficient alterations which take place with the genetic apparatus of the cells are similar in each case. The particular properties of the cancer cell are propagated to each succeeding generation even after the inducing agent is no longer present. Apparently, the chemical coding ability of the nucleic acid of the cell has been altered [10].

Theories of Carcinogenesis

Several books [11–14] have been written in the last few years which review the numerous theories and hypotheses which have been advanced to interpret carcinogenesis. In all instances, the authors conclude that these are as yet far from adequate. The most hopeful approaches to a solution of these problems still seem to lie along rather empirical routes such as the further discovery and detailed mechanistic study of agents known to take part in either or both the initiation and promotion stages of carcinogenesis.

One continuing approach to such empirical investigations has been the synthesis and biological study of new members of several series [15, 16, 17] of large flat molecules known as polycyclic heterocyclic compounds which have, in common with polycyclic hydrocarbons, the potential biological property of being rather strongly absorbed on protein surfaces [18, 19]. However, as has been pointed out previously, protein binding is not sufficient to explain carcinogenicity. Many compounds react with proteins but are not carcinogenic (i.e., acylating agents, dinitrofluorobenzene, etc.). The above-named compounds also

inactivate enzymes; however, they are not carcinogenic and they do not react with DNA [20]. There are many data which indicate that known carcinogenic hydrocarbons bind to proteins [18, 19], and although it has been widely suggested that these same carcinogenic hydrocarbons bind to nucleic acids, there was little information until recently [21, 22, 23] that this is actually the case [24]. It has been suggested that a possible function of a carcinogenic agent with the ability to bind to proteins may be its removal ("deletion") of a nuclear protein from coordination with DNA, thus allowing the DNA to "run wild" and initiate cancer [25]. The deletion hypothesis said that tumors resulted from the loss or deletion of an enzyme system responsible for the control of normal growth, possibly because of the combination between the carcinogen and protein. An alternative possibility would be the deletion of some repressor system [25b].

Clayson [26] has suggested that two theories which seem of most importance in considering a possible mechanism for chemical carcinogenesis are based on: (i) somatic mutation, or (ii) the immunological theory of cancer. In spite of the fact that many investigators appear to have assumed that carcinogenesis involves as a first step a mutation in chromosomal genes in the cell, there is evidence that powerful mutagens are not carcinogens and vice versa. Thus, it seems unnecessary to identify any portion of the carcinogenic process with mutation. Nevertheless, the change from normal to precancerous tissue during the process of initiation must involve a change in heritable features of the cell.

Green [27] considered malignancy to be the result of the loss of specific antigens by the animal. He suggested that the chemical induction of cancer takes place by two stages: (1) the interaction of the carcinogen with the specific antigens and/or other constituents of the animal tissues to form *complexes*, (2) an immune response by the host to the tissue—carcinogen *complexes* accompanied by the elimination of the tissue-specific antigen and the appearance of a neoplastic cell.

It has been generally found that all chemical carcinogens do combine with tissue constituents, i.e., proteins. Kobat [28] has suggested that antibodies react with only certain chemical groupings on the antigenic substance surface.

Some experiments [29] have indicated that the action of chemical carcinogens in newborn mice do not adequately protect them from later chemical carcinogenesis, i.e., no tolerance is acquired; however, see the section on tumor growth inhibition by polycyclic compounds and Ref. [30].

Indirect Carcinogens

Some effective agents appear not to be direct carcinogens. Boyland [31a, b] has found that the aromatic amines, 2-acetamido-fluorene and

2-naphthylamine, are converted *in vivo* to the active compounds which are probably arylhydroxylamines which have chemical properties somewhat similar to the biological alkylating agents which are also carcinogenic. It was found [31b] that 2-naphthylhydroxylamine (50 mg/kg.) in arachis oil twice a week for three months, intraperitoneally, in 15 random inbred strains of albino rats induced six abdominal sarcomas, three abdominal carcinosarcomas of diverse histological types and one lymphosarcoma. An identical dose of 2-naphthylamine induced sarcomas in $\frac{2}{14}$ and possibly a salivary gland tumor in $\frac{1}{14}$. These results agree with the hypothesis that the 2-naphthylamine and some other aromatic amines exert their carcinogenic action after a metabolic conversion to hydroxylamine derivatives.

The arylhydroxylamines react with sulfhydryl compounds to form 5-aminophenylderivatives (many carcinogenic substances react with sulfhydryl compounds). A few years ago, the Millers and their co-workers [32] at Wisconsin showed that N-hydroxylation is an important reaction which leads to an active intermediate of carcinogenic amino compounds. Ring hydroxylation is the other chief metabolic reaction but seems to be a detoxification route. However, this area is still under active investigation [33] and the precise details of the mechanism of carcinogenesis by the aromatic amines have not been clearly established. A somewhat complicating factor is the fact that there are sex-linked differences in the metabolism of N-2-fluorenylacetamide by rats [34].

Boyland [31a] also suggests that the carcinogenic hydrocarbons, and presumably other polycyclic carcinogens, may be converted by metabolic processes (i.e., conversion to epoxides by oxygen) to carcinogenic derivatives; they may also be oxidized to dicarboxylic acid derivatives leading to combination with proteins and sulfhydryl compounds. These latter metabolic processes with hydrocarbons, however, are probably detoxifications and not involved in the carcinogenic effect which is probably initiated by a charge-complex formation of the purine bases of DNA with either the hydrocarbon itself or an epoxy or peroxy derivative.

Kotin and Falk [35] have studied a large number of organic peroxides and epoxides, and have concluded that they have a definite carcinogenic and radiometric (DNA chain breaking) effect. This has been pointed out in specific instances previously by several groups of workers and various members of the class have been known for sometime as effective experimental anti-tumor agents against transplanted tumors.

Cancer Forming DNA

Recently, a group of scientists from the Sloan-Kettering Institute for Cancer Research in New York, headed by Bendich, collaborated

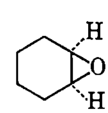
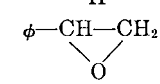
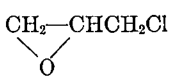
with Stewart and Eddy of the National Institutes of Health to study the nature of the infective and cancer-inducing portion of polyoma virus [36].

When polyoma virus was inoculated into tissue culture cells of mouse embryo, the virus multiplied and produced the characteristic cytopathic changes and released new virus particles. The virus particles were isolated from the cell culture fluid and their protein coatings stripped off to leave only the bare nucleic acid. This isolated virus nucleic acid was then added to a fresh virus-free mouse embryo cell culture and again the same cytopathic effects resulted as had been observed with the whole virus. The new virus particles developed in this latter culture were isolated and inoculated into mice or hamsters to produce the disease characteristic of polyoma virus infections. Also, when the isolated nucleic acid itself was injected into the animals, polyoma

TABLE 1

EPOXIDES WHICH CATALYZE THE DEPOLYMERIZATION OF DNA AND RNA

KOTIN AND FALK (1963) [35]

Epoxidated soy oil	
Butyl-9,10-epoxystearate	
	Diepoxides

tumors developed in a small number of them. The isolated nucleic acid was inert to the enzyme RNase but rapidly detoxified by DNase. It was thus established that an infective cancer-producing DNA had been isolated. Since then, the nucleic acid of the Shope virus has been isolated and found to be an infectious DNA.

These findings are of considerable importance and serve as a verification of the general concept that cancer is associated with a permanent change in the cell's genetic center which is controlled by a deoxyribose nucleic acid. Since DNAs from various species differ in details of their chemical structures, this is the basis of difference in genetic character which leads to variations in the characteristics and functions of different types of cells.

Thus, it seems inevitable that cancer cells differ from normal cells in some specific fashion in the structure of the deoxyribonucleic acid genetic center. The malignant powers of these new cancer cells must be directly associated with the particular characteristics of this special

DNA. Intensive, continuing investigations in this basic area of research are strongly indicated.

In the past, it has been assumed that the chemical carcinogens operate by removing the protective protein sheaths from the latent virus, releasing the infectious DNA in the cell. Although there is little direct evidence, it may well be that the overall transformation induced by the chemical agent in a less specific fashion and over a much longer length of time is closely related, in part of the process, to that accomplished more rapidly by the virus (or its bare DNA).

Energy Transfer and Carcinogenesis

Several investigators have attempted to associate the functioning of the carcinogenic polycyclic compounds with an energy transfer between the carcinogen and the DNA molecule at the genetic center of the somatic cell, leading to a chemical change which results in an altered and malignant DNA [37]. It has been shown by Szent-Gyorgyi and co-workers [38] that there is an apparent correlation between carcinogenic activity and the ability of some compounds to form charge-transfer complexes with molecular iodine.

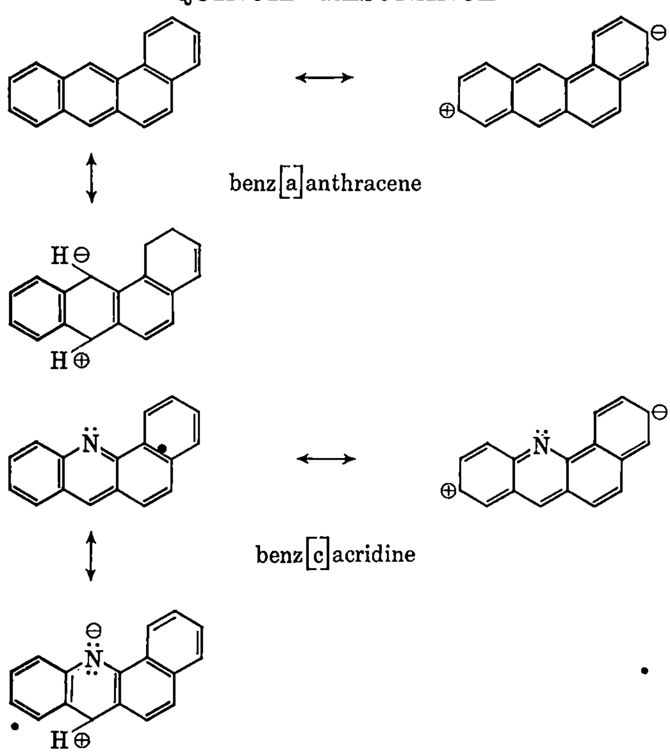
Electron donation and acceptance by carcinogenic compounds has been studied by Allison and Nash [39a] for polycyclic hydrocarbons, steroids, polycyclic heterocyclics and aromatic amines. Charge-transfer complex formation with chloranil in acetonitrile and with 1,3,5-trinitrobenzene in benzene, and electron spin-resonance in complexes with tetracyanoethylene were measured and the results found to show a broad general correlation (with noticeable discrepancies) between carcinogenicity and electron donation and acceptance.

The simultaneous ability of many carcinogenic compounds to donate and accept electrons is ascribed to the quinoid or pseudoquinoid character of resonance for the polycyclic compounds. Steric factors and the stability of the carcinogen under physiological conditions were also suggested as important factors.

It has been suggested [39a, 40, 22] that theory can be related to biological systems by means of a "sandwich" type model wherein the near coplanar carcinogen (polycyclic compound) lies between two active sites in the metabolic system, one serving as an electron donor, the other as an electron acceptor, forming charge-transfer complexes with both. Allison and Nash suggest that this might [39a] be expected to cause the carcinogen to exert a primary effect on oxidative phosphorylation leading to the altered DNA molecule. From a practical standpoint, their work [39a] clearly indicated that these tests demonstrating charge-transfer with trinitrobenzene and acridine would have found all of the major carcinogens studied. However, several non-carcinogens would also have been included in the positive group.

The Pullmans [39b] have attacked the theory of Allison and Nash [39a] and claim to have demonstrated that there is no correlation between the electron donating and accepting properties, and carcinogenicity. They cite examples of many compounds presenting ideal conditions for carcinogenicity according to the Allison and Nash hypothesis which are devoid of carcinogenic activity. Allison and Nash [39c] have replied by pointing out that the energy coefficient measurements presented

QUINOID RESONANCE



by the Pullmans are estimates rather than actual measurements of charge-transfer formation. Also, the original theory had taken into account the existence of several exceptions.

Sung [41] has attempted to relate absorption spectra and carcinogenicity. No proven carcinogen was found in 73 compounds with absorption spectra below 354.5 mμ, but 42% of compounds with absorption spectra above 354.5 mμ (log $\epsilon > 1$) were carcinogenic, and all with λ max between 354.5 and 368 mμ were carcinogens. Included in this latter group were 3,4-benzpyrene; 3,4,8,9-dibenzpyrene, cholanthrene, 20-methyl-cholanthrene, various methylbenzanthracenes and methylbenzacridines. The results are thought to be in agreement with previous work by this author who demonstrated that, for twenty-eight methyl-

benzacridines, 13/14 carcinogenic compounds have ΔE_2 below 1132 kcal and 11/14 noncarcinogenic compounds have a ΔE_2 above this value.

Wentworth and Becker [42a] have investigated a method for determining the electron affinities of organic molecules which employs an electron capture cell used as a sensitive detector in gas chromatography. They have discussed the relationship between the electron absorption coefficient determined in this way and the electron affinity of the molecule. The electron affinities and ionization potentials of various aromatic hydrocarbons have been correlated [42b]. The stability of complexes formed between methylbenzenes and polycyclic aromatic hydrocarbons were found to be proportional to increased electron affinity of the polycyclic compounds adding further proof to a charge-transfer complex interpretation [42c]. It would be interesting to attempt a correlation of electron affinities as determined with carcinogenic activity for a suitable series of compounds.

Huggins and Yang [40] studied the ability of a wide variety of carcinogenic and non-carcinogenic substances to induce mammary cancer in rats. They came to the conclusion that the molecular dimensions of the active polycyclic hydrocarbons had a close resemblance to steroids in so far as the planar dimension was concerned but their thickness was less, being 3.6Å as compared with 5.6Å for the non-carcinogenic, non-planar steroidal compounds. It was suggested that these thin, flat molecules containing 4 to 5 condensed rings could be expected to interact with base-pairs (guanine-cytosine or adenine-thymine) in the Watson-Crick model of DNA leading to a modification and carcinogenesis.

Boyland and Green [21] and Liquori and DeLerma [22] have recently reported *in vitro* reactions between DNA and benzpyrene, dibenzanthracene and pyrene. It is suggested by both groups that the hydrocarbons intercalate between base pairs in the DNA molecule. The results obtained by the latter group [22], in solubilization experiments of benzpyrene and dibenzanthracene in DNA-water solutions in conjunction with observed changes in U. V. absorption spectra and fluorescence spectra of the dissolved benzpyrene, are reported. It is suggested that the aromatic molecules become inserted between base pairs in the "disordered" stretched sections of the DNA molecule. The nature of the sites are not specified but it is strongly suggested that the empty spaces between occasional bases, present in very small numbers may exist in "undenatured" DNA and may be filled by the aromatic hydrocarbons. These may increase on denaturation of DNA, on dilution, or on heating. Weak charge-transfer and dipole-induced dipole-dipole forces between the polar purine bases and the polarizable aromatic molecules may stabilize the complexes. Lerman [24a] used studies of sedimentation, low-angle X-ray scattering, flow dichroism, flow-polarized fluorescence, and chemical reactivity to obtain evidence

for intercalation of acridines between two otherwise sequential base-pairs in DNA. The binding is thought to require a local untwisting and extension of the double helix. Although intercalation seems to be a prerequisite for mutagenicity of the insertion-deletion type, the acridine structure is not essential nor are all intercalating molecules found to be mutagenic.

Brookes and Lawley [24b] studied the binding of a variety of tritium-labeled hydrocarbons with RNA, DNA and protein in mouse skin. In one case, a C^{14}-labeled compound was used. No binding occurs immediately after application and maximal binding takes place after 24–48 hours. The results of a large number of experiments showed that the partition of each individual hydrocarbon between the cellular constituents, RNA, DNA and protein gave a consistent pattern which differed for the various hydrocarbons in a fashion related to their carcinogenic potency as expressed by the Iball index [60]. The extent of binding to total protein or RNA showed no correlation in this regard but binding to DNA showed a significant positive correlation with carcinogenic potency. It is suggested by Brookes and Lawley [24b] that DNA is the essential cellular receptor of the carcinogens. The nature of the binding is not clear but it is suggested that a metabolite of the hydrocarbon (i.e., an epoxide) is actually the bound form.

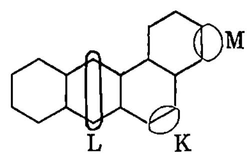
It seems probable that many compounds can be expected to form charge transfer complexes with DNA which are not carcinogens. The important factor may be whether or not the charge transfer complex then proceeds to form an addition product with DNA in the case of carcinogens.

Heidelburger [24c] admits that DNA can be shown to bind to aromatic hydrocarbons but claims there is no correlation with carcinogenicity since no localization of radioactivity in the nuclei could be detected using C^{14}-containing hydrocarbons. He has shown that the Boyland [21] and Liquori [22] experiments are probably invalid. Actually, a collodial suspension between the hydrocarbon particles and the DNA seems to form rather than an intercalated molecular complex. The hydrocarbon seems to stabilize the DNA colloid to some extent. Also the Boyland DNA was mainly single strand rather than double strand.

The K-L-M *Region Theory of Carcinogenesis*

Although the precise details of the interactions of tissue with carcinogens at the molecular level are still not known with certainty, considerable progress in this direction has been made since the important pioneering work by the French group of workers, reviewed by the Pullmans in 1955 [43]. This school developed an electronic theory based upon molecular orbital calculations of activation energies to predict

which polycyclic hydrocarbon should be the most carcinogenic. They concluded that, in carcinogenic molecules, the energy of the phenanthrenoid bond, designated as the K (Krebs) region must not exceed a certain limit while the activation energy of the anthrenoid or meso (L) region of the molecule must exceed another value.



It now appears that the statement of the original theory was an oversimplification of the complex structural requirements for carcinogenesis. Some recent elaborations of this theory, taken in conjunction with a consideration of steric factors and the physical properties of the molecules, are more encouraging and suggestive of further fundamental studies.

On the basis of new structural variation studies and on previous data, the role of the K-region of polycyclic compounds in carcinogenesis has been recently reviewed by the Millers [44] and the theory made more definitive. For example, while substitution of fluorine in the 5-position of 7-methylbenz[a]anthracene destroys carcinogenicity, the 6-fluoro derivative has nearly the same activity as the parent compound. These and other data suggest that, of the two K-region positions, availability of an unsubstituted 5-position is far more important than a free 6-position.

In summing up their studies with the angular benzacridines, Lacassagne and co-workers [17] concluded in 1956: "it now seems certain that the greater or lesser carcinogenicity of a molecule is linked to very slight structural differences, and it is probable that with only very refined techniques will there be success in understanding these differences. The important steric effects—which govern the affinity of the molecule to its biological carrier—must not be overlooked. These effects are clearly demonstrated by the disappearance of carcinogenicity when side chains on the hydrocarbon are lengthened (a phenomenon which has been pointed out in the angular benzacridines)" (see p. 367 of Ref. 17). Thus, eight years ago, the French workers recognized the importance of the major parameters involved in carcinogenic activity of polycyclic compounds which have recently been restated and elaborated by new workers entering this field.

Using the Hammett substituent constant, σ, and a substituent constant, π, defined as $\pi = \log P_x - \log P_H$ (P_H is the partition coefficient of a parent compound and P_x that of a derivative), regression analyses have been made of the effect of substituents on the biological activity of carcinogenic compounds on mice. The results clearly indicate

that the lipo-hydrophilic character of these molecules should be considered in attempts to rationalize structure activity relationships. Specifically, these studies have led to a simple explanation for the inactivity of the large molecules of six or more rings and the higher alkyl derivatives of 1,2-benzanthracene. The large inactive molecules are generally too lipophilic (larger values of log P) while small inactive molecules are not lipophilic enough (low values of log P) Hanceh and Fuita [45]. They suggest that this parameter is a more fundamental one to consider than variations in steric factors or the importance of K and L regions in these molecules.

Chemical Reactivity and Carcinogenicity of Polycyclic Compounds

In general, both polycyclic aromatic hydrocarbons and heterocyclics show a low order of chemical reactivity under laboratory conditions of pH and temperature approximating physiological conditions. Thus, except for charge-transfer complex formation, discussed previously, attempts to relate carcinogenicity of a series to the various members' ability to react with various standard reagents have met with only limited success.

In 1954, Badger [46] suggested that three reagents which add to bonds (i.e., the K-region) rather than by replacing functional groups might prove useful. These reagents were: ozone, diazoacetic acid and osmium tetroxide. Badger reported a "very satisfactory correlation" between the rate of addition of osmium tetroxide and the bond order at the K-region, calculated by the molecular orbital method for a group of non-substituted aromatic polycyclic compounds. The agreement was very good when the rates of reaction were compared with the Pullmans' electronic index. Thus, there is good agreement between the excess charge at the K-region and the rate of reaction with osmium tetroxide and it is thus shown that the osmium tetroxide reaction confirms the relationship between the calculated electronic indices for the K-region and the rate of reaction with a reagent which attacks this part of the molecule.

Badger found that both electron repelling (methyl) and electron attracting (cyano) groups convert benz(a)anthracene into carcinogenic material; one might note that some of the electron-attracting groups may engage in H-bonding with centers in DNA. The OsO_4 results indicated that the more carcinogenic materials often reacted the more rapidly with OsO_4, which in turn reacts with those aromatic hydrocarbons having the greater charge at the K-region as calculated by either the valence bond or molecular orbital method.

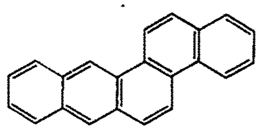
The ozonization of polycyclic aromatic hydrocarbons has been extensively investigated by Moriconi [47] who has shown that ozone can react at all three of the reaction sites designated by the Pullmans as the K-, L-, and M-regions. Attempts have been made to correlate K-, L-,

and M-region activity of both carcinogenic and non-carcinogenic compounds with the path and mechanism of the ozonolysis reaction. It turns out that ozone attacks the potent carcinogens mainly at the L-region rather than at the K-region.

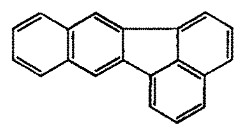
It seems probable that the metabolic reactions likely to be related to the course of these ozone reactions are the reactions of detoxification rather than the charge-transfer complex formation with tissue constituents most likely to be responsible for the carcinogenicity in these series.

Tumor Growth Inhibition by Polycyclic Compounds

Green [27] reported that anthracene-1,2-2′,1′-naphthalene (I) and 11,-12-benzfluoranthene (II) had tumor inhibitory properties with a rat tumor which had been induced by 1,2-5,6-dibenzanthacene and transplanted through 500 generations. These two compounds, however,



(I)



(II)

were ineffective in preventing growth of over 100 spontaneous or induced tumors in rats and mice. One cannot be sure that these compounds (I) and (II) are actually non-carcinogenic as originally defined by Green. Recently Wynder and Hoffman [48] have reported that compound (II) may be weakly carcinogenic. It is certain, however, that these two compounds, with tumor inhibiting properties, are much weaker as carcinogens than the strong polycyclic hydrocarbon carcinogens. Green [49] finally concluded that no basic difference in mode of action had been demonstrated for tumor inhibition by carcinogenic and non-carcinogenic polycyclic hydrocarbons.

Green [27] has suggested that the tumor-inhibiting hydrocarbons act by enhancing the defense mechanism of the host against the *transplanted* tumors. This idea is supported by the observation that conditions which suppress anti-body formation prevent the inhibition of tumors by these hydrocarbons; i.e., when high doses of cortisone or charcoal are applied at the same site, tumor inhibition is ineffective. Cortisone or charcoal is expected to suppress the action of anti-body forming cells. More specifically, Green [27] postulates that the tumor-inhibitory hydrocarbons act by combining with the host proteins to simulate the iso-antigens of a transplanted tumor. The complex of the tumor inhibitor and the body protein calls forth an anti-body response from the host; these anti-bodies were then thought to react with the iso-antigens of the *transplanted* tumor and led to rejection or at least a reduction in its rate

of growth. Since *spontaneous* or *induced* tumors develop from the same animal, *these* tumors and their hosts do not show iso-antigenic differences and the polycyclic hydrocarbons, effective in suppressing the growth of *transplanted* tumors, are now ineffective.

Buu-Hoi [50] has recently reviewed the use of carcinogenic polycyclic hydrocarbons; i.e., 3,4-benzpyrene, 9,10-dimethyl-1,2-benzanthracene and 20-methylcholanthrene, as anti-tumor agents, as well as the use of certain non-carcinogenic polycyclic compounds as tumor inhibitors, including such compounds as the angular dibenzophenothiazenes, benzophenothiazenes and compounds (I) and (II) described above. Buu-Hoi [50] has reported that 6-aminochrysene inhibits skin carcinogenesis induced by 20-methylcholanthrene and spontaneous mammary adenocarcinomas in mice, but it has no effect in chronic lymphatic leukemia or Hodgkin's disease. Chrysene itself is apparently not carcinogenic [51] but has also been found to show an inhibition effect on the growth of tautologous (inbred strain) tumors and less of an effect on the growth of homologous transplants [52].

Dao and Tanaka [30] have reported that a single feeding to rats of an effective dose (i.e., 10 mg.) of six different polycyclic aromatic hydrocarbons including four carcinogens, 9,10-dimethyl-1,2-benzanthracene (DMBA), 20-methylcholanthrene, 3,4-benzpyrene and 1,2-benzanthracene, as well as anthracene and phenanthrene, 48 hours prior to administration of 30 mg. of DMBA, completely inhibited the DMBA-induced adrenal necrosis. It took 10 mg. of the carcinogens to afford 100% protection and 25 and 100 mg., respectively, for the two non-carcinogens. It is interesting that the non-necrotizing dose of DMBA protects the animal from the necrosis-inducing effect of larger amounts of the same compound. These studies indicated that the presence of a critical amount of adrenocortical steroids in the adrenal gland was a prerequisite to the induction of necrosis by DMBA.

Photodynamic Activity and Carcinogenic Action

As long ago as 1900, Raab [53] found that the time required for various dyes to kill paramecia depended on the light intensity. Since then, dyes and related substances have been referred to as "sensitizers" in the light-catalyzed processes designated as "photodynamic." This field was summarized up to 1941 by Blum [54]. The chemical reaction which occurs in these processes appears to be a photosensitized oxidation by the dissolved oxygen. Doniach and co-workers [55] reported that carcinogenic hydrocarbons were 10^3 to 10^4 times as active for killing paramecia as the dyes which had normally been used as photodynamic sensitizers.

Koffler and Markert [56] have shown that DNA is degraded photodynamically by methylcholanthrene and 1,2-benzanthracene. It is thus apparent that photodynamic action can degrade macromolecules as does

ionizing radiation and the carcinogenic hydrocarbons are unusually effective sensitizers for these reactions. Blum [54] has described the process by the following steps:

$D + h\nu \rightarrow D'$ (activation of sensitizer)
$D' + O_2 \rightarrow D + O_2'$ (activation of oxygen)
$O_2' + X \rightarrow X$ (oxidized) (photodynamic reaction)

where D is the sensitizer and X the substrate (e.g., macromolecule) reacting.

Mottram and Doniach [55a] reported a rather impressive correlation between photodynamic toxicity and carcinogenicity.

The Doniach test system has been used by Epstein and co-workers [57] to study photodynamic toxicity in several series of polycyclic aromatic hydrocarbons and polycyclic heterocyclic compounds with wide structural variations. A general correlation between photodynamic toxicity and carcinogenicity has been found. The test system was also successfully applied to the bioassay of pure samples of 3,4-benzpyrene and some other polycyclic carcinogens in the concentration range of $10^{-5} - 10^{-10}$ gm/ml [57b].

The photodynamic response of paramecium to 3,4-benzpyrene appears to be oxygen-dependent, and marked protection is afforded by anti-oxidants such as oxotocopherol and butylated hydroxy anisole when added to the incubation system [57d]. The sensitivity of the response could also be modified by a variety of other factors such as tryptophan, fractions of plasma proteins, non-ionic wetting agents, and inorganic salts. No evidence for the involvement of -SH groups or of peroxide formation could be demonstrated in the photodynamic process. 3,4-benzpyrene, or its presumed water-soluble photodynamically active product, was found not to be firmly bound to *P. caudatum* [57d].

After studying the photodynamic response of 157 carcinogenic and non-carcinogenic polycyclic aromatic hydrocarbons and heterocyclic compounds, Epstein and co-workers [57e] came to the following general conclusions in connection with the relationships between structure, carcinogenicity, photodynamic activity and light absorptivity:

(1) significant absorption of light from the experimental irradiation system was shown to be a prerequisite but not sufficient requirement for high photodynamic activity;
(2) high photodynamic activity is largely confined to 4- and 5-ring compounds whether homocyclic or heterocyclic;
(3) inactivity cannot be ascribed to difficulties in compound solubilization or cell uptake alone;
(4) a statistically significant association between photodynamic activity and carcinogenicity was demonstrated;

(5) using a defined threshold, it was shown that compounds with high photodynamic activity are likely to be three times as carcinogenic as compounds with low activity, but the photodynamic assay cannot identify a particular compound as being carcinogenic or non-carcinogenic.

It appears that the preliminary use of the photodynamic assay method would select a *passing group* for which the odds of carcinogenic activity would be three times as great as for compounds in the *failing group*.

Concerning the effect of structural variations, high photodynamic activity, like strong carcinogenicity seems to be limited to compounds with 4 or 5 rings. Charge-transfer complex formation may be a common factor to both carcinogenicity [37–40] and photodynamic response. Charge transfer complex formation with tissue constituents such as DNA [22] or proteins [18] could account for the observation of the critical molecular geometrical requirements for the polycyclic compounds both for carcinogenicity [40] and for photodynamic toxicity [57e]. However, Epstein and co-workers have now found that there is no direct correlation between carcinogenicity of a series of aromatic compounds and their involvment in forming charge transfer complexes with iodine, chloranil, trinitrobenzene or acridine [57f].

Photodynamic assay has been applied to the assay of crude organic extracts such as atmospheric pollutants [57b]. This application is based [57e] upon the fact that the principal known carcinogen in the atmosphere is benzo(a)pyrene, which is strongly photodynamically active, and, generally in industrial areas, there is a high correlation between the amount of this strong carcinogen and other polycyclic hydrocarbons in the air.

In general it was found that amino groups on the 1-position of pyrene, 6-position of chrysene (6-aminochrysene inhibits growth of induced tumors [50]) and 4-position of fluoranthene markedly increased photodynamic response of the inactive or slightly active parent compounds. Both methyl and especially amino groups on the 6-position of benz(a)-pyrene result in a marked decrease in photodynamic activity as when these same groups are introduced into the 7-position of benz(a)anthracene.

Among a series of benz(c)acridines prepared by Cromwell and co-workers [15] and tested by Epstein [57e] there is a marked increase in activity of the 7,9-dimethyl substituent with successive decreases for the 5,6-dimethyl, 7,10-dimethyl and 1,4-dimethyl derivatives.

The 5,6-dimethyl structure shows increased activity with chlorine in the 7-position, decreased with chlorine in the 10-position, and decreased further with 9- or 11-chloro substituents. It is interesting that, for the dimethylbenz(c)acridine series, the carcinogenicity is known to be

strong for the 7,9-derivative, generally weak for the 5,6-derivatives, and weakest for the 5,6-dimethyl-11-chlorobenz(c)-acridine [58]. The carcinogenicity of the new [59] 7-chloro derivative has not yet been measured.

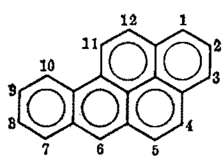
In contrast with the high photodynamic activity for the 7-chloro-5,6-dimethylbenz(c)acridine there is a successive decrease in activity for the corresponding 7-morpholino, 7-phenoxy and 7-acetoxy derivatives [59]. This reduction in activity may be assigned to the increased steric requirements for these larger groups. It was also found that the 5,6-dimethylbenz(c)acridone [57] shows no photodynamic activity.

Another series for which carcinogenicity studies are as yet unavailable are the 11H-indeno-quinoxalines [16c]. The parent compound shows strong photodynamic activity with decreasing activity for the 11-hydroxy-11-methyl and 5,10-di-N-oxo-11-methyl-11-hydroxy derivatives, respectively. The 11-one and 5-N-oxo-11-one derivatives have a similar degree of photodynamic activity to the 11-hydroxy-11-methyl derivative. On the basis of these studies, it is seen that a K-region does not appear to be necessary for photodynamic activity and linear as well as angular compounds are found to be active. It seems of interest to study the carcinogenicity of some of the indenoquinoxalines.

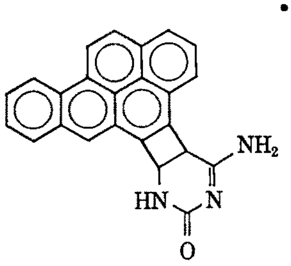
From the *in vivo* data reported by Epstein [57], it appears that significant light absorption is a prerequisite but not sufficient for photodynamic activity. The exact location of absorption peaks does not appear to be important and fluorescence is inconsequential.

The work of Rice [23] shows clearly that benzo(a)pyrene forms stable addition products with uracil, thymine, cytosine, 5-methyl-cytosine, guanine and 6-azothymine upon irradiation with ultraviolet light above 320 mμ in aqueous solution at pH 6–7.

It is suggested that the general structure of the benzpyrene-pyrimidine adducts are as indicated in structure II.



I



II

These workers hope to be able to correlate carcinogenic activity of various hydrocarbons with their ability to form photo-adducts in 4% aqueous sodium dodecyl sulfate solutions. It would seem to be of more importance to seek a correlation between carcinogenicity and the dark

reaction of a series of carcinogens with these biologically important bases.

Syntheses of New Polycyclic Heterocyclic Compounds of Biological Interest

A working hypothesis for carcinogenesis and carcinostasis to guide further research with the polycyclic compounds is based on the following multifacet rationale: The carcinogen initiates an infectious DNA through a dual interaction with the cell nucleus. The nucleoprotein complexes with the polycyclic compound and the DNA is laid bare for further attack. Intercalation of the large flat polycycles between base pairs in the more disordered stretched sections of the DNA chain follows. Such intercalation involves charge-transfer complexation (electron donation and acceptance) which would be expected to provide increased stability to such disordered sections of the DNA through π-orbital overlap. Such specific complex formation might mask part of the nucleic acid chain (some of the bases) in the case of compounds with critical steric requirements leading to interference with the replication process for the normal DNA and alteration in the assembling of amino acids in the nucleoprotein by messenger RNA. Thus, a new cell nucleus emerges. When it happens that there is no controlling immune response available from the host, uncontrolled growth begins and neoplasia is started which no longer requires the presence of the original carcinogen for its propagation. It is also possible that the carcinogen plays an important role in the suppression of the normal immune response expected from the host to deal with appearance of strange cell nuclei. Complex formation between the carcinogen and specific antigens might result in their loss by the animal at this critical time, allowing for the uncontrolled growth of the abnormal cell.

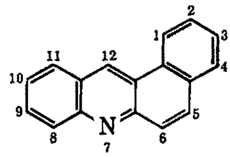
Concentration factors are of importance in the related process of carcinogenesis versus carcinostasis; see ref. [30 and 50]. Thus, it is suggested that chemically induced immunization against certain forms of cancer may become a reality.

During the course of a general program involving the synthesis of potential carcinogenic (cancer-forming) and carcinolytic (anti-tumor) agents, new methods of synthesis were developed which provide for a wide variety of new types of derivatives of the biologically important benzacridines [15] and the less well-known indenoquinolines [16a, b], indenoquinoxalines [16a] and the benzophenazines [16d]. The biological interest in compounds of this nature may be said to have developed when it was discovered in France [17] that benzacridines of the angular type caused cancer on the skin of inbred strains of mice when properly applied.

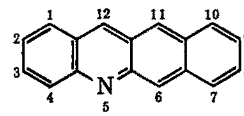
These new compounds are intended for testing both as carcinogenic

Polycyclic Heterocyclic Compounds

The Benzacridines



(a)



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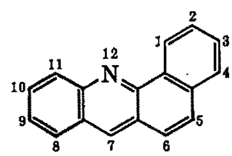
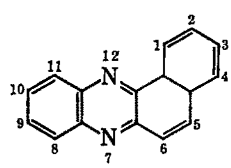


(c)

The Benzophenazines



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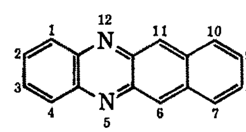
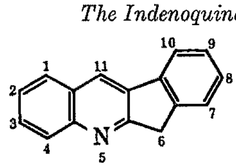
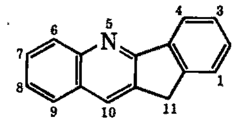


(b)

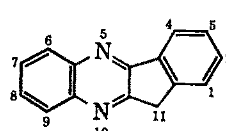
The Indenoquinolines and Indenoquinoxalines



6 H—(2, 1-b)—



11 H—(1, 2-b)—



11 H—(1, 2-b)—

and anti-tumor agents, although, as a class, they have been mainly recognized in the past as carcinogenic substances. Since most known anti-tumor agents have also been found to be carcinogenic in action with the proper system, it seemed logical to us that series which have essentially been known for their carcinogenic action might include some examples which would prove to be effective anti-tumor agents. Thus, by subtle changes in chemical structure, one might be able to convert a series of compounds from essentially carcinogenic substances to mainly anti-tumor agents.

The relationship between chemical constitution and carcinogenic activity (and the related process of carcinostasis) for these several series of large flat molecules is becoming clearer and it is hoped that further work will provide for the elucidation of these important biological processes.

It seems obvious that, if the organic chemist is to help solve these related problems of carcinogenesis and carcinolytic action of various agents, he must play a role far broader than merely supplying new agents showing these effects. In the years ahead, he must bring to bear upon these related problems his understanding of the mechanisms of organic reactions to help solve the biological mechanisms of action of carcinogens and carcinolytic agents. Such studies will undoubtedly involve investigations of immunological problems. Each type of scientist must bring to the battlefield those weapons with which he is most competent to fight. Literally, thousands of scientists and physicians have contributed either directly or indirectly to the attack on the cancer problem in the last few years. For all these people, there is only one major goal. That is, the discovery of fundamental information which will lead to the control of human disease.

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AMERICAN SCIENTIST, *53*, 1965

SOME PROBLEMS OF HOMINID CLASSIFICATION[1]

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THE EARLY 1950's were years of controversy in human paleontology; argument, discussion, and sometimes polemic were focused on the relationship to other primates of the genus *Australopithecus*, primates sometimes called near-men or man-apes. These were known mainly, at that time, from finds made in the Transvaal, South Africa. Although the species of *Australopithecus* are now considered hominid, many students were reluctant at first to accept such a status, believing that small brain volume and "imperfect" adaptation to upright posture prevented their assignment to the Hominidae, the taxonomic family which includes living man. Instead, these creatures were regarded as aberrant apes.

Subsequently, it was realized that it is not possible to exclude species from the Hominidae on account of small brain size; the ancestors of modern man must obviously, at some stage, have had smaller brains. The "imperfections" of the *Australopithecus* pelvis were also overstressed; there is good reason to believe that the species of *Australopithecus* were now habitual and efficient bipeds. The concept that different functional systems may evolve independently and at varying rates (mosaic evolution), has also been assimilated by most anthropologists, and, in addition, it is no longer considered reasonable to discriminate taxa on the basis of a few characters alone.

Another sort of misunderstanding derives from one's reference point in studying hominid evolution. Perhaps we have been looking down the wrong end of the telescope, so to speak, trying to understand the evolution of man by looking backward through time "from the vantage point of the Recent" (Patterson, 1954). If *Homo* species evolved from the species *Australopithecus africanus*, as many believe, we should expect that these species would have many features in common. Those characters in which they differ, however, should be regarded as specializations of *Homo*, not as peculiarities of *Australopithecus*. One must not ask "How *Homo*-like is *Australopithecus*?" but rather the opposite.

Origin of the Hominidae

Washburn has stated recently that "most of the characters of *Homo* seem to have evolved well within the Pleistocene, and there is no need to postulate an early separation of man and ape" (1963: 203). But the fossil record, although limited, instead seems to indicate a pre-Pliocene

[1] First draft received July 15, 1964; Revised manuscript received February 18, 1965.

separation. Our Pliocene ancestors evidently were socially and adaptively more like man than great apes. By the early Pleistocene, except in the matter of brain-size, man's relatives (men or near-men) were almost as different morphologically from both living and extinct great apes as are men today. *Australopithecus* in fact resembles *Homo* far more than either resembles the African apes, man's closest living relatives.

Ever since the nineteenth-century inception of human paleontology, the comparison of early man with living apes, the only other well-known hominoids, has been overemphasized. This tendency has led, on the one hand, to stressing supposedly ape-like features in late forms of fossil man (Boule's classic studies of European Neanderthals, for example). On the other hand, this has led to surprise whenever "advanced" characteristics are found in early forms (for example, the modern looking hominid foot from Bed I at Olduvai Gorge, Tanganyika).

Ramapithecus punjabicus (Fig. 1) is known from the Siwalik Hills of North India, from Fort Ternan, Kenya, and possibly from the Swabian Alps in Europe (Simons, 1964), appearing first in the latest Miocene. This species also occurs in sediments of uncertain age in Keiyuan, Yunnan, China (Simons and Pilbeam, 1965). The known morphology of *Ramapithecus* is discussed in Simons (1961, 1964); it is sufficient here to point out that the incisors, canines, and premolars are reduced relative to those of known species of dryopithecines and of present-day apes. In *Ramapithecus*, as in man, internal cingula are absent and molar crowns simple. *Ramapithecus* could have evolved with almost equal probability from a dryopithecine, sometime between early and middle Miocene, or from species more like the Egyptian Oligocene primate *Propliopithecus haeckeli.* Hominoid species evolving respectively in the direction of *Homo* and of *Pan* need not have shared a common ancestor later than early or middle Miocene times.

In our opinion, assignment to Hominidae can reasonably be made for all those species that show evolutionary trends toward modern *Homo*, whenever these trends appear. The evolutionary shift in a major adaptive zone indicated in the case of *Ramapithecus* by its reduced snout and anterior teeth (premolars, canines, and incisors) may well correlate with an increased use of the hands and the incipient development of bipedality, although direct fossil evidence for both these developments is presently lacking. Even if *Ramapithecus* and *Pan* had a more recent common ancestor than either did with the orangutan, the Hominidae are presumably definable in terms of this adaptive shift, as is indicated in Figure 2. Moreover, *Ramapithecus* is presently best regarded as a hominid, not a hominid-like pongid, because it already exhibits the basic dental adaptations of *Homo* and *Australopithecus.* If the other parts of the skeleton, subsequently found, should all be ape-like, this position could require alteration. But why should this be so? Of course, it is quite

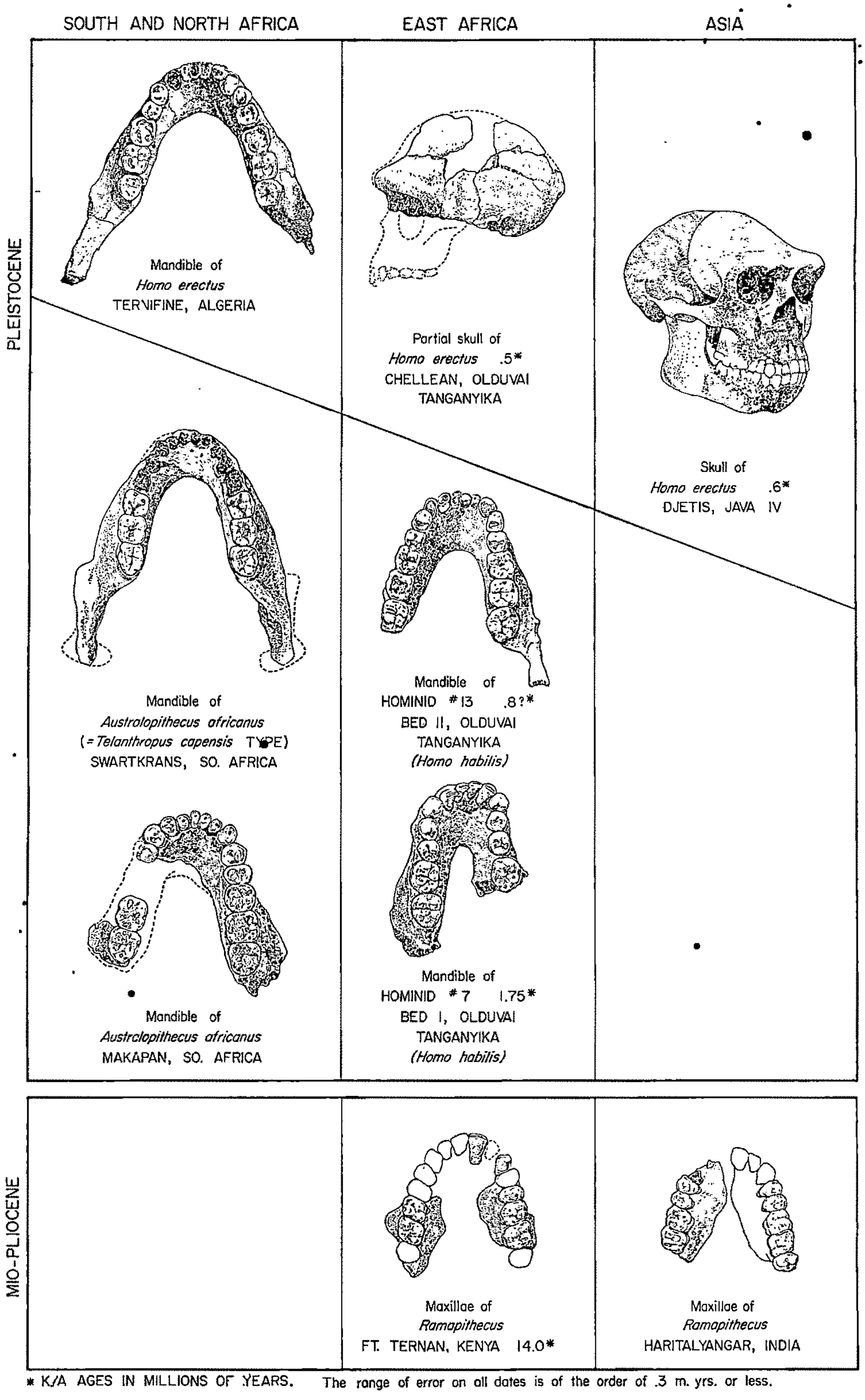

FIG. 1. Mio-Pliocene and Pleistocene hominids from Asia and Africa with potassium/argon dates. Specimens on the left and right are matched with morphologically similar forms from the K/A-dated East African sequence.

possible that, if and when cranial and post-cranial remains of *Ramapithecus* are discovered, they will prove to be rather more *Dryopithecus*-like than are those of *Australopithecus*, because *Ramapithecus* lies several million years closer in time to the common ancestor of apes and men. However, the known parts are not ape-like; *Ramapithecus* cannot logically be lumped any longer with the apes.

Fossil ancestors of the living orangutan of Asia are either unknown or unrecognized. Biochemical and serological research underlines the close relationship of modern man to the great apes of Africa. This supports other, mainly paleontological, evidence suggesting that chimpanzee, gorilla, and men are more closely related cladistically, that is, in recency of a common ancestor for all (Harrison and Weiner, 1963), than all three are to the orangutan and the Asiatic gibbon. Even if the orangutan and the African apes are patristically related, that is, if all are more similar in outward (phenotypic or phenetic) morphology than any one of them is to *Homo*, they still could be less similar genetically. However, although the ancestral line leading to the orangutan probably differentiated from an early unknown member of the dryopithecine complex before the hominids did so, the hominids have differentiated more rapidly and now occupy an adaptive niche quite different from that presently filled by the African and Asian apes.

Classification of Hominoidea

In comparison with most other superfamilies, too many distinctions of higher taxa have been drawn among hominoids. This is presumably because much of the taxonomic work on these categories has been done by persons unacquainted with the manner in which higher categories have been, or should be, proposed in the light of the new systematics. Most mammalogists would probably now prefer to use three basic principles in justifying the erection or retention of families and subfamilies. Briefly, these principles are: (1) the group which is thought to deserve such status should have had a considerable time duration as a separate stock, (2) the proposed taxon (a formal unit grouping organisms) should show considerable diversity in terms of contained species and genera, (3) the category should be characterized by a reasonably thorough-going structural distinctiveness shared by its members and not like that of related families or subfamilies. Admittedly, criteria of morphological distinctiveness are subjective, but material on which to base such judgments can be derived from comparative anatomy. For instance, one can make an approximate answer to the question: Do members of the families of man and apes differ *more*, or *less*, in total skeletal and dental morphology than do species of other related families within one order, such as Canidae and Felidae?

Because classification should also reflect both the past evolution of

given taxa and the morphology and adaptation of the present members of such taxa, a compromise must be reached which reflects both cladistic and patristic relationships (Fig. 1). Consequently, it is preferable in a classification of the superfamily Hominoidea to retain a separate family for the genera *Ramapithecus*, *Australopithecus*, and *Homo*, a family sustained primarily by the morphological features which indicate the adaptive shift to hominid feeding patterns and habitual bipedalism.

Species of orangutan, chimpanzee, and gorilla, together with those of *Dryopithecus* and *Gigantopithecus*, should be retained in the Pongidae. Dryopithecinae can be justified as a separate subfamily. Although two further subfamilies, Ponginae and Paninae, could be used, these would contain at the most only one or two genera and species, and the distinction would not, in our opinion, be particularly meaningful. It is even more difficult to justify the division of Hominidae into such subfamilies as Australopithecinae, Homininae, Praehomininae, and the like.

Ecology and Adaptation of Early Hominoids

Modern pongids and hominids are characterized by relative trunk erectness, a feature shared with more "primitive" primate species (Schultz, 1961). For example, many prosimians are vertical tree-clingers, like *Indri*, *Propithecus*, and *Tarsius*, while the New World monkeys *Ateles*, *Lagothrix*, and *Brachyteles* are partial brachiators (Erikson, 1963). Even the more terrestrial Old World monkeys sit erect while feeding, grooming, and resting. Avis (1962) has suggested that the hominoid superfamily differentiated from other primates by becoming arm-swingers confined to a small-branch niche in a forest habitat; in her opinion this differentiation occurred as early as the Eocene. This matter of arm-swinging raises the largely academic question of what is or is not formal "brachiation." (This form of locomotion requires suspension from alternate hands, forward movement being produced by pronation of the arm and trunk around the fixed hand; propulsive force in such locomotion comes entirely from the upper limbs.) *Proconsul africanus*, for instance, is said to have been a "probrachiator" (Napier, 1963), and thus in some ways similar morphologically to modern colobine monkeys. *P. africanus* does not exhibit the extreme forelimb elongation characteristic of modern apes (Napier and Davis, 1959). Does this mean that *P. africanus* could not have "brachiated" in the formal sense? Perhaps so; perhaps not. Several genera of New World monkeys move through the trees in this manner without showing all the anatomical "brachiating" specializations of the Old World pongids.

Whether "brachiators" or "probrachiators," Miocene apes and their Oligocene ancestors probably showed a high degree of trunk erectness and doubtless spent much time walking and running bipedally, either

in the trees or on the ground. As Gregory suggested (1928), some brachiation would probably have been essential for the hominids before they could become habitual bipeds. The structure of the human arm, thorax, and abdomen all suggest that, at some stage, our early Tertiary forerunners may have, on occasion, moved by armswinging in the trees.

The idea of the "emancipated forelimb" has been greatly overstressed in discussions of hominid tool-using. Hands have been important throughout all of primate evolution; the higher primates, in particular, use their hands in a wide range of activities such as feeding and grooming. The habit of sitting erect, widespread in primates, insures that the hands are free for these activities. However, in nonhominid primates the hands are typically used in locomotion as well; it is in this respect that hominid forelimb "emancipation" becomes important. As Kortlandt (1962) and Schultz (1961) have suggested, stone-and branch-throwing in defense were probably important in early hominid behavior before tool-using became widespread. It is also clear from the dentition of *Ramapithecus* that the early hominids could not have fed by stripping vegetable material with the canines and incisors as do the African apes; such feeding behavior requires relatively large front teeth for nipping, tearing, and shredding. It is possible, as a consequence of both these factors, that hominids were *ad hoc* tool-users at least by the early Pliocene (Napier, 1963). Once hominids became committed to a terrestrial way of life and some degree of habitual bipedal locomotion, the freeing of their forelimbs would have greatly facilitated tool use.

The earliest hominids may, of course, have functioned fairly well bipedally long before noticeable skeletal alterations increasing the efficiency of this manner of locomotion had become genetically fixed. Such an hypothesis gains strength from observation of the crude walking of the living spider monkey (*Ateles*), gibbon (*Hylobates*), and even some lemurs (*Propithecus*). While it can be debated whether this type of progression should formally be called bipedal, it certainly cannot be written off as quadrupedal movement; none of these primates use the hands habitually to support the forebody during locomotion on the ground. Despite such tendencies toward bipedal walking, however, none of these primates show any man-like skeletal adjustments that make upright walking more efficient.

Selection pressures were doubtless strongly in favor of such trends when the hominids were evolving, but the nature of the ecological readjustments involved in the origin of the hominid line is likely to remain a matter of conjecture. A change in diet also occurred during the course of hominid evolution, but, as Schultz (1961) has pointed out, this shift has been overstressed. The modern great apes are often described as vegetarians, but Schultz suggests that, among the primates, perhaps only the colobine monkeys can be termed truly vegetarian. Most of the other

primates tend to supplement their diets with animal protein. However, Miocene apes probably ate mostly vegetable material just as modern baboons, chimpanzees and gorillas do, and the earliest hominids presumably had a similar diet. During the Pliocene, the amount of animal protein in the hominid diet surely would have increased as scavenging and hunting, feeding habits evidently well established by the early Pleistocene, became more widespread in hominid populations. As this dietary shift brought with it increased calorific values, less time would have been required for feeding, and the habit of food-sharing could develop.

Bartholomew and Birdsell (1953) have discussed theoretical concepts of early hominid ecology and believe that the early hominids were wide-ranging, food-sharing, weapon-using omnivores. It has been said that "tool-using and tool-making were very probably associated with the tendency for early members of the phyletic line leading to man to take to a certain amount of meat-eating" (Robinson, 1963a, 393). The reduced canines and incisors of *Ramapithecus punjabicus* suggest that tool-using may have been established by the late Miocene, because smaller front teeth require the use of other means to prepare food, either animal or vegetable.[1] Noback and Moskowitz have emphasized that the increasing dexterity of the hand seems to have played a major role in the evolution of the central nervous system among higher primates, particularly in the case of the Hominidae, and this influence may well have been acting in *Ramapithecus* populations.

Robinson (1962, 1963) has discussed at length the alterations in pelvic anatomy and muscle function consequent to erect bipedalism. Unfortunately, no pelves of Miocene or Pliocene dryopithecines are yet known. It is generally assumed that the pelves of mid-Tertiary higher primates will prove to be similar to those of modern pongids. In this view, the human pelvis is considered to have been derived from an ancestral morphology similar to that of modern apes. It seems more likely, however, that some morphological differentiation has occurred in the pongid as well as the hominid line. Indeed, such pieces of evidence as the hominid-like pelvis of *Oreopithecus* and the broad ilia of New World brachiators, taken together with the considerable probability that early pongids were arboreal arm-swingers with erect trunks, suggest that the immediate ancestors of the hominids were actually pre-adapted as brachiators in terms of behavior, and perhaps to bipedal running and walking in terms of anatomy.

Chimpanzees and gorillas have prognathous, that is, projecting faces;

[1]It may be argued that all three *Ramapithecus* maxillary specimens are those of small-canined females. This is not too likely, but, even if true, it would not alter the fact that the premolars and incisors are also smaller relative to molar size than is the case in any pongid.

their mid-Tertiary ancestors were evidently more orthognathous (straight-faced), and it seems that facial lengthening occurred, at least in part, as a response to demands of ground living, defensive display and vegetarian diet. Baboons and macaques have prognathous faces also. Large teeth and powerful muscles are required in order to chew tough plant food. Considerable sexual dimorphism in canine size is shown among various species of apes and Old World monkeys and, consequently, it is probable that (among other factors) elongated canines are associated with defensive display behavior. Increased stress on olfaction among mainly ground-feeding species such as baboons probably was important in the development of the snout. In contrast, colobine monkeys, New World monkeys, and the erect-postured tree-clinging prosimians such as *Indri, Propithecus,* and *Tarsius* all have relatively short faces and show less sexual dimorphism. Among fossil forms the small Miocene species *Proconsul africanus* (Davis and Napier, 1963) was also relatively orthognathous as was *Oreopithecus.* New evidence secured by the recent Yale expeditions (Simons, 1965) indicates that several of the Egyptian Oligocene hominoids were short-faced too.

Trunk erectness and orthognathism are apparently closely linked. Erect posture is associated with changes in orientation of the cranial base, typically exemplified by downward rotation of the facial axis on the basicranial axis (DuBrul, 1950; DuBrul and Laskin, 1961; Biegert, 1963). Mills (1963) has suggested that the assumption of habitual erect posture would cause further flattening of the face. He points out that, in primates with large canines, the lower canines pass behind the upper incisors during chewing. With facial shortening, the canines no longer pass behind, but rather in the plane of, the maxillary incisors. As this happens, selection favors reduction of canine crowns. If tool use (and possible changes in male display behavior as well) had removed the selective advantage of large canines, canine reduction inevitably would have taken place.

Thus, it appears that several anatomical, behavioral, and "cultural" elements—erect posture, orthognathism, changes in diet and display behavior, an increasing use of tools and reduction of the anterior dentition—are here closely linked one to another. Members of the genus *Ramapithecus* have small front teeth and were apparently wide-ranging even in the late Miocene; the dental evidence implies that profound behavioral, dietary, and locomotor changes had already occurred among species of *Ramapithecus* by this time. The commitment to a hominid way of life could conceivably have been made by the late Miocene, and our earliest known probable ancestors, with brains perhaps comparable in size to those of chimpanzees, might have already adopted a way of life distinct from that of their ape contemporaries.

Appearance and Speciation of Man

Near the beginning of the Pleistocene, hominids are represented in the fossil record by two or more species of *Australopithecus*, a small-brained, large-jawed form similar dentally to *Homo*. Post-cranially, *Australopithecus* is similar to, although not identical with, later men and evidently was an habitual biped. Early Pleistocene sites at Olduvai in Tanganyika have yielded hominid remains, together with crude stone tools (Leakey, 1959); stone tools of similar type are known from North Africa (Biberson, 1963) and the Jordan valley (Stekelis, 1960), and it seems likely that, at about the same time, bone tools were being made in South Africa (Dart, 1957). Regular tool-making, utilizing bone, stone, wood, and perhaps other material too, possibly began more than two million years ago.

It has been suggested by many authors that the transition from tool-using to regular tool-making was a step of crucial importance in the evolution of man. This may well be true but, like many generalizations in anthropology, this one has been oversimplified. As Napier has pointed out (1963), tool-making may often have been invented and forgotten in the late Tertiary. During Villafranchian time (that is, during earliest Pleistocene time), some hominids were doubtless tool-makers while others were still only tool-users. Differing environmental demands would produce different behavioral responses. However, the advantages of regular tool-making are clear and, once invented, the spread of this skill would probably have been fairly rapid.

Speciation in the Hominidae

Simpson (1961: 90) remarks:

"Supposedly intergeneric hybridization, usually with sterile offspring, is possible among animals, for instance, in mammals, the artificial crosses *Bos* × *Bison*, *Equus* × *Asinus*, and *Ursus* × *Thalarctos*. In my opinion, however, this might better be taken as basis for uniting the nominal genera. I would not now give generic rank to *Bison*, *Asinus*, or *Thalarctos*."

There is considerable evidence that the African and Arabian baboons, previously thought to belong to several separate genera, can produce viable hybrids, and may, instead, be classified in perhaps as few as two or three species of a single genus. Hybrid studies, by the Russians and others, suggest that species of *Macaca* (the rhesus monkey) and perhaps of *Cercocebus* (the mangabey), too, should be classified as belonging instead to the baboon genus *Papio*. *Papio* in this sense, can be regarded as a wide-ranging genus with local species populations which show variations in morphology, coloring, and behavior. Freedman's metrical work on cranial variation in *Papio* "species" (1963) lends support to this view. Some of the populations may be sibling species, others may warrant only subspecific rank; only interfertility studies can de-

termine their validity as genetical species. Among members of *Papio*, greater morphological variability is to be seen among samples of adult males than among samples of adult females, particularly in cranial features. This, together with pronounced sexual dimorphism, suggests that differences in mating and display patterns have selected for a great deal of the specific and subspecific morphological differences. The small amount of speciation within the genus is also significant when compared with *Cercopithecus* (guenons) or colobine monkeys, and this is almost certainly a direct reflection of *Papio*'s wide-ranging terrestrial way of life. Among these primates, there is apparently a rough correlation between species range size and the degree of speciation within a genus. Highly arboreal primates, such as species of langurs and gibbons, tend to have more restricted ranges and tend to be less mobile as groups; isolation and speciation become more likely under these circumstances.

The earliest hominids probably were at least as wide-ranging and mobile as the baboons, and presumably would have been much more so by middle and late Pliocene time. As noted already, fossil evidence suggests that *Ramapithecus punjabicus* was already present in East Africa, India, China, and possibly Europe, by the early Pliocene; there is no reason to suppose that hominids have not been widely dispersed since then. Man has capitalized on plasticity rather than becoming restricted to narrow morphological and behavioral adaptations. His mobility, his ability to occupy a highly diversified ecological niche, and his apparently slow development of isolating mechanisms (Mayr, 1963: 644) all tend to reduce speciation. It is a reasonable working hypothesis, therefore, that not more than one genus, and perhaps no more than one or two species of hominid, has existed at any particular time. Like most other mammals, man is polytypic; that is, a number of races are found within the species. We should expect fossil hominid species populations to be polytypic too.

Taxonomy of Australopithecus

Before the student can erect new fossil genera and species he must demonstrate that the new proposed taxon differs significantly from previously described taxa in a number of particular characters. Thus, any diagnosis should take full account of known variability in living related species and genera. Different specific and generic names also imply certain other differences. If two individual fossils or fossil populations have different specific names, this implies that they could not have been members of a single freely interbreeding population; this, in turn, requires a period of reproductive and probably geographic isolation. Different generic names, in their turn, generally imply that the taxa concerned were completely incompatible genetically; to develop such incompatibility would require a long period of isolation. As we

have already noted, however, such isolation would probably have been an unusual event during the course of hominid evolution.

Early Pleistocene hominids are known from a number of African localities. The first to be described and discussed was *Australopithecus africanus* from South Africa. It is now fairly generally agreed that two species of this genus are known: *A. africanus* (Fig. 1) from Villafranchian deposits at Taung, Sterkfontein, and Makapansgat, and *A. robustus*, from possibly latest early Pleistocene and middle Pleistocene deposits at Kromdraai and Swartkrans (Robinson, 1963). Another form, "*Telanthropus capensis*" (Fig. 1) has been recovered from Swartkrans; its status is equivocal and will be discussed later.[1] Robinson (1963a) discussed these australopithecine forms at length, pointing out that *A. africanus* is, in his opinion, closer to the ancestry of later men than is *A. robustus*.

Definite or probable australopithecines have been reported also from Java, North Africa, Israel, and East Africa. *Meganthropus palaeojavanicus*—represented by finds from the Djetis beds of Java—is said by Robinson to be closely similar to the African form *A. robustus*. Although Clark (1955, 86) considers the Java specimen's generic separation from another hominid form, *Homo erectus* (Fig. 1), unjustified in view of the fragmentary nature of the material, the fact is that these jaw fragments do not provide enough information to allow students to draw species distinctions between the Javan and African material.

Coppens (1962) has reported the recovery of an australopithecine skull from Koro-Toro, near Lake Chad south of the Sahara. The associated fauna suggests an early Villafranchian age. Arambourg (1963: 564) states that this fossil is intermediate in morphology between *A. africanus* and *A. robustus* but with perhaps a greater cranial capacity. Robinson (1963b: 601) considers it closer to *A. robustus*. No pebble tools are associated, although they are present in later Villafranchian deposits of North Africa (Biberson, 1963). "*Meganthropus africanus*" from the Laetolil beds of early Pleistocene age near Lake Eyassi in East Africa has been referred to *Australopithecus* by Robinson (1955). In addition Stekelis (1960) has discovered fragmentary hominid remains of early or middle Pleistocene age associated with a pebble tool culture at Ubeidiya in the Central Jordan valley.

Finally, Leakey has recently described a number of hominid finds in deposits of early and middle Pleistocene age at Olduvai Gorge, Tanganyika (Leakey, 1959, 1961, and Leakey, Tobias, and Napier, 1964). Two distinct species have been recognized and described, *Australopithecus* (= *Zinjanthropus*) "*boisei*" from Bed I at Olduvai and from possibly middle Pleistocene deposits near Lake Natron, Tanganyika,

[1]Invalid or doubtful taxonomic terms are indicated here in quotes on initial citation only.

and "*Homo habilis*" (type, hominid #7) from Bed I and other specimens from Bed II at Olduvai. *A. boisei* is bigger and more robust than *H. habilis* (see Fig. 1) and has larger molars and premolars; we believe that it cannot be distinguished at the specific level from *A. robustus*.

The hominid sites from Bed I have been dated by the potassium-argon method (Leakey, Curtis, and Evernden, 1961), the three sites FLK I, FLKNN I, and MK falling between 1.57 and 1.89 million years. The Bed II hominids from FLK II, VEK IV, and MNK II are younger than 1.02 million years, the youngest date for the top of Bed I, and older than 0.49 million years, the date of a post-Chellean II tuff in Bed II (Hay, 1963). The *Homo habilis* material therefore falls into two groups, separated by perhaps as much as one million years. Once again, during the period of deposition of the lower parts of Bed II, two distinct taxa appear to have been present, *H. habilis* at Olduvai and *A. boisei* at Lake Natron.

Altogether, then, some half dozen supposedly distinct taxa (both genera and species) have been proposed for early and early Middle Pleistocene hominids; several of these are based on the most fragmentary and limited material. Schultz has repeatedly stressed the very high level of morphological variability among species of the Hominoidea (see bibliographies in Schultz, 1961 and 1963). Nevertheless, small and taxonomically trivial differences in dental, cranial, and postcranial anatomy have still been used to establish or justify specific or generic distinctions among the earlier Pleistocene hominids. What follows is an attempt to bring some taxonomic order to this situation.

At present, *A. robustus* is known from South Africa (Swartkrans, Kromdraai), Olduvai Bed I, Lake Natron, and possibly Java. The other African forms of roughly equivalent age have been referred to three taxa, *A. africanus*, *Telanthropus capensis*, and *Homo habilis*. The relative dating of the North, East, and South African sites presents a number of problems. Faunally, there are few mammal species as they are presently defined common to all three sites, and those which are common are often unsuitable for purposes of correlation. Cooke (1963) has discussed this problem at length. He believes that the South African sites which have yielded *A. africanus* are broadly contemporaneous with the later Bed I levels at Olduvai. However, he points out that:

> "Although ecological differences prevent too close a comparison, the faunas suggest strongly that the sequence in East Africa from the Kaiso and the Kanam levels through Omo and Laetolil to Bed I corresponds fairly closely in time to the sequence Sterkfontein and Makapansgat to Swartkrans and Kromdraai. Although the evidence is extremely tenuous, the Villafranchian (equivalent) fauna of North Africa could well be contemporary with these East Afrian deposits and the ape-man breccias. The occurrence of pebble tools in similar relationships in all three areas may be significant and if the North African beds are truly pre-Cromerian as has been suggested by several authorities this would provide additional grounds for keeping at least the major part of the ape-man deposits within the Villafranchian." (Cooke, 1964: 104)

Biberson (1963) considers the Koro-Toro site at Lake Chad contemporary with Kaiso and Kanam in East Africa, and, if this is so, the Chad hominid could well be the oldest *Australopithecus* known. *Telanthropus capensis* from Swartkrans is probably the same age as *Homo habilis* from Olduvai Bed II, while *A. africanus* from Taung, Sterkfontein and Makapansgat and Bed I *H. habilis* are possibly of approximately equivalent age.

The New East African Hominids and the South African Australopithecines

Known *Homo habilis* material, as already noted, falls into two main groups separated by perhaps as much as a million years. The type specimen of *H. habilis* consists of a juvenile mandible, two parietal fragments, a hand and an upper molar. A foot and clavicle belonging to an older individual (or individuals—associations of all Olduvai individuals are certainly not clear) have been recovered from the same site (FLKNN I). Two other sites in Bed I have yielded teeth, mandibular and skull fragments, and a tibia and fibula belonging either to *H. habilis* or to *Australopithecus* (*Zinjanthropus*) *boisei*. Among remains from Bed II are: a complete mandible (with associated maxillae) together with the occipitals and broken parietals and temporals of possibly the same individual, cranial fragments, isolated teeth, and a damaged palate. Although there is insufficient material from which firm inferences can be drawn, the sequence through Bed I to Bed II suggests that teeth and mandibles became progressively reduced during this time, while there was little alteration in cranial capacity. (This seemingly unchanging brain size may correlate with the presence throughout the section of similar crude pebble tools, or the two faunas may be closer in time than K/A-dated horizons appear to indicate.) The juvenile mandible and teeth from FLKNN I in Bed I are some 10 per cent greater in all measurements than the mandible from MNK II in Bed II. Such a small number of mandibles cannot, of course, be regarded as necessarily typical of the populations from which they come, but it is possible that, during this period, jaws and teeth became reduced while cranial capacity remained fairly constant.

A large number of *A. africanus* specimens have been described. Because this taxon is known so well, some have tended to think typologically in terms of an "australopithecine stage" of human evolution during which all hominids would have been morphologically similar to these South African forms. However, if the hominids evolved in the main as a single, widespread, polytypic species, the Transvaal *A. africanus* more likely represents a sample, drawn from a time segment of unknown length, of a peripheral and perhaps aberrant race of this species (Mayr, 1963: 640). We should not expect contemporaneous or near contemporaneous races within this same species to be morphologically identical

with, or even quite similar to, the South African *A. africanus*. A large number of fossil specimens, say, from a restricted geographic area and from a relatively small segment of time, may well resemble each other more than any one of them resembles a small sample of the same species from a different part of the geographic range and from an earlier or later time. In such a case, however, we must be careful not to regard the large sample as morphologically "typical" of the species. By so doing, we would prevent correct assignment of other specimens of the same taxon. Morphological differences between specimens due to age and sex

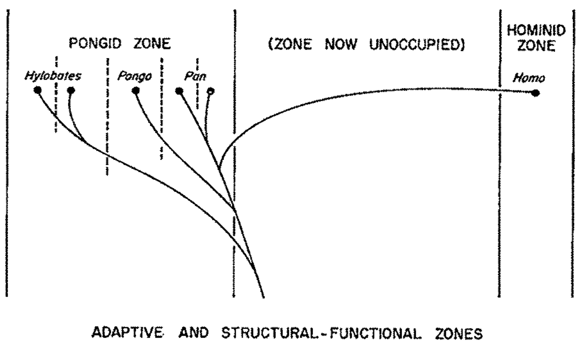

FIG. 2. Diagram showing relative closeness of phylogenetic relationships of living hominoids and their radiation into adaptive-structural-functional zones, from Simpson (1963).

differences and to geographic and temporal separation, as well as racial variation, must be carefully considered. In erecting new taxa, it is necessary for the discoverer to demonstrate that the new finds are *significantly* different from previously defined taxa.

Thus, when we consider the hominids from Lake Chad and Olduvai Bed I we must ask ourselves, could these represent taxa which are already known? *A. africanus* and *H. habilis* were both habitual bipeds. Unfortunately, we have pelves of the former but not of the latter, whereas we know the foot of the latter but not of the former. It is possible that the *H. habilis* foot from Olduvai Bed I is no more nor less *Homo*-like than is the pelvis of *A. africanus* from S. Africa.

The parietal bones from Olduvai FLKNN I have been reconstructed by Tobias (1964) to indicate a cranial capacity of between 642–724 cc. This volume estimate must be regarded with great caution because of the fragmentary nature of the specimen.[1] Tobias (1963) gives the australopithecine range of cranial capacity as 435–600 cc., this being lower than his estimates for this skull of *Homo habilis*. However, there seems to be no

[1]The slightest mis-setting of the two bones at the midline, for instance if flared too much laterally, would markedly increase the brain volume estimate for this individual.

good reason for separating early species of the genera *Australopithecus* and *Homo* on grounds of differences in cranial volume. None of the crania from the Transvaal breccias or from Olduvai Beds I and II indicate volumes outside the range now known among the single species of living gorilla, and all these crania are distinctly smaller than those of *Homo erectus*.

The teeth and mandible of *H. habilis* from Olduvai Bed I FLKNN I do not differ greatly from specimens of *A. africanus* (Fig. 2 and Dart, 1962: 268, Fig. 7) in shape or morphology. The molars are similar in size and shape to those of *A. africanus* from Sterkfontein (Robinson, 1956). The premolars of *H. habilis* are somewhat narrower than those of the South African forms; but shape, as well as size, of teeth is known to vary greatly in all modern primates, including man, and this seems to be a relatively unimportant character on which to base generic and specific separation of the two forms. Unfortunately, some of the Olduvai specimens are crushed and broken and this limits our ability to make comparisons. Collection of further hominids from the Olduvai beds is thus of the greatest importance; recovery of a more adequate sample should enable students to assess the range of variability within the local population represented at Olduvai, and would allow comparisons of this population with others of approximately equivalent age. *H. habilis* and *A. africanus* may represent nothing more than two variant populations within the same widespread species, but this hypothesis can only be verified or rejected when more information becomes available.

The validity of *Homo habilis*, as any new fossil taxon, depends on the reality and the plausibility of its diagnosis. As Campbell states (1964, 451):

> "It is here that the hypothesis of the new species must stand or fall; . . . The diagnosis must support the hypothesis for the species to stand, not in law, but in reality. . . . (Many examples of this state of affairs could be quoted. The most topical, and one of the most important, concerns the creation of the taxon *Homo habilis*. In their original publication the authors stated that *Telanthropus capensis* "may well prove, on closer comparative investigation, to belong to *Homo habilis*"; thus the effective demonstration of a novel taxon was negated. The name is valid, but the species has not been effectively shown to have existed, as a distinct taxon.)"

Telanthropus capensis from Swartkrans is probably broadly contemporaneous with Bed II *Homo habilis*. In spite of its fragmentary nature, some general remarks can be made about *Telanthropus*. The teeth are smaller than those of *A. africanus*, although cusp patterns are similar. The mandible is smaller, too, and is said by Robinson (1961) to be reminiscent of other African and Asian hominids of Middle Pleistocene date. This is to be expected for a form transitional in time between the early Pleistocene hominids and middle Pleistocene *Homo erectus*. It is probable that Bed II *H. habilis* and *Telanthropus capensis* represent two populations of a single species or subspecies, but, once again, further

material will be required before firm conclusions can be drawn. Both are similar to the mandible of *H. erectus* from the Djetis beds in Java (so-called "*Pithecanthropus* B").

The primitive stone tools associated with *Homo habilis* throughout Bed I and the lower part of Bed II are evidently of uniform type for what may be a very long period of time; this is perhaps correlated with the equally protracted apparent stability of cranial capacity. Tools of similar type are found in Morocco (Biberson, 1963) and at Ubeidiya (Stekelis, 1961). It should be noted that the early Pleistocene hominid from Chad, found unassociated with tools, has a cranial capacity of the order of that of both *H. habilis* and *A. africanus*. Regular tool-making, as we noted earlier, seems to have been invented during the early Pleistocene, perhaps in Africa, at a time when hominids had already become efficient bipeds. Dates from Olduvai suggest the possibility that a full

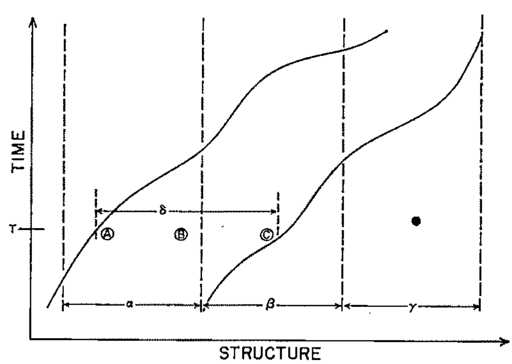

FIG. 3. An attempt to represent Pleistocene hominids as a single genetic continuum, evolving through time. Modified from Simpson (1963).

million years passed after the invention and spread to tool-making during which cranial capacity—and presumably manual dexterity, "intelligence," and hunting skill—remained fairly constant. Some time roughly between one million and five hundred thousand years ago, human brain size increased by more than 50 per cent. Also, during this period, the change from simple pebble chopping tools to hand-axes of Chellean type apparently took place. Elaboration of tool types and expansion in brain size were probably interrelated, and both, perhaps, were associated with the final anatomical perfection of hand structure. Napier (1962) has suggested that the hand of *Homo habilis* was not as refined structurally and functionally as that of later men. Undoubtedly, limits of both mental ability and hand anatomy affected the form of tools.

The earliest Pleistocene appearance of tool-making, then, seems to have heralded no immediate anatomical changes in the hominid line.

Cranial capacity, in particular, remained constant. Jaws, teeth, and faces became reduced during the era of crude pebble tools, but exactly why this was so remains conjectural. Anthropologists have frequently suggested that the appearance of tool-making was causally related to the expansion of the brain. There was never any fossil evidence to support this view, and now we have some evidence to the contrary. Increase in brain size evidently lagged behind the regular making of tools. This skill had altered the lives of early Pleistocene hominids; apparently it did not immediately alter their morphology.

Taxonomy of Early African Hominids

We have concluded that *A. africanus* and Bed I *Homo habilis* may not be specifically distinct and also that the bulk of Bed II materials and *Telanthropus* may be specifically identical. *Telanthropus* itself is regarded by many students as an invalid genus which should be referred either to *Homo* or to *Australopithecus*. If referred to *Homo* the trivial name *capensis* can no longer be used, since this has already been applied to a Late Paleolithic skull from Boskop (for further discussion of this point see Oakley and Campbell, 1964). If the Olduvai Bed I and Bed II *Homo habilis* material is regarded as belonging to a single taxon, it is not unreasonable also to include therein *A. africanus* and the *Telanthropus* material from South Africa. This taxon would extend over a very considerable time period; in fact it would be more than twice as long as the time covered by *H. erectus* and *H. sapiens* combined. Some hominid evolutionary change, particularly trends towards orthognathism and reduced dentition, occurred during this considerable span of time, although the amount of variation within the taxon is probably not greater than that observed in modern mammal species (including man).

If all early Pleistocene hominids ancestral to later men are regarded as members of the genus *Homo*, as the proposal of *H. habilis* by Leakey, Tobias, and Napier suggests, the prior binomial for this taxon would be *Homo africanus*. The evolution of early Pleistocene hominids, first to *Homo erectus*, and finally to *Homo sapiens*, can be shown diagrammatically in Fig. 3 (modified from Simpson 1963, Fig. 3c), which shows a single hominid species evolving through time. A, B, and C are hypothetical contemporaneous individuals, C being more "modern" than B, B more so than A. Simpson (1963: 14) suggests as a possibility that:

"... there is only one lineage or evolutionary species and only one genetical species at any one time. In that case, the species would have been highly variable, and even more so during much of past time than *Homo sapiens* is at present. At some time around the middle Pleistocene it might have varied all the way from what in purely morphological (or typological) terms could be called marginal australopithecoid through pithecanthropoid to marginal neanderthaloid. Such variation would be improbable within a single deme or local population. It would be less improbable among geographically separate "allopatric" populations or subspecies. Such geographic semi-isolates would

of course be variable in themselves, but some might, for instance, vary about a more australopithecoid modal morphology and others about a more neanderthaloid mode. Discovery that fossil hominids fall into such modally distinct, synchronous but allopatric groups would favor this interpretation. Whether current data do or do not tend to follow such a pattern I leave to the specialists in such matters."

We prefer to accept Simpson's model for the moment.

A, B, and C of Figure 3 are contemporaneous at time T and are members of a single species. However, typologically A and B might be placed in one species α while C is referred to species β. The paradox is imagined rather than real, because the taxa α and β, and the taxon δ are of different types. If the hominids have evolved as a single unitary species, δ will represent a sample of the genetical species which existed at time T. α, β, and γ are morphospecies, species "established by morphological similarity regardless of other considerations" (Simpson, 1961, 155). The problem here is largely one of definition and should not be overstressed. Nevertheless, this model will be useful in dealing with problems which will appear should "undoubted" (morphological) *Homo* be found contemporaneous with "undoubted" (morphological) *Australopithecus*.

Both *Australopithecus africanus* and *A. robustus* are bipeds, both have greatly reduced canines and incisors, both almost certainly would have been tool-users, and both probably made tools. If tool-making spread by copying within one species of small-brained hominids, it would presumably have been copied by any other species of equally small-brained hominids living in the same area.

Australopithecus robustus is said by Robinson (1961) to have been a vegetarian because of its massive premolars and molars. *A. robustus* specimens from Olduvai and Natron exhibit pronounced wear patterns on both upper and lower molars. Such wear patterns are found in certain Australian aboriginal tribes that eat roots and other vegetable material together with large quantities of sand and grit. In contrast, *Homo habilis* has wear patterns similar to those of meat-eating African tribes such as the Masai (Leakey, personal communication). *A. robustus* is said to have been a vegetarian and *A. africanus* (Robinson, 1963) and *H. habilis* more exclusively meat-eaters. Although the *A. robustus* wear patterns suggest that gritty vegetable material constituted a large part of the diet, they do not, however, enable us to state categorically that *A. robustus* did *not* eat meat. Nor are we entitled to assume that *A. robustus* and *A. africanus* were at all times vegetarians or omnivores respectively; diet can vary greatly (within contemporary *Homo sapiens* groups for example), and presumably changes with time too. Hominids were successful because they were behaviorally plastic and adaptable; we must take great care before we place ecological limitations on fossil hominids known only in relatively poor detail.

In summary, *Australopithecus robustus*, like *Ramapithecus punjabicus*, *Homo habilis* and *A. africanus*, has small canines and incisors. This

implies that *A. robustus* prepared its food, presumably with tools, and there is no reason to suppose that it could not have eaten prepared animal as well as prepared vegetable food. The diet of modern "primitive" men is varied; could not earlier hominids have been similarly omnivorous? *A. robustus* and *H. habilis* were evidently co-existent in the same general areas for perhaps a million years, if the new dates and stratigraphic-faunal data of Leakey are right. They were, therefore, sympatric species, that is, species occupying the same geographic area. Both were bipeds, both were presumably tool-users and probably tool-makers; their diets might well have been similar at times. As mentioned earlier, there are theoretical difficulties involved in preparing a model of hominid speciation. If we are to distinguish these taxa at a specific level, we need to know far more about geographical barriers during the Pliocene and Pleistocene. Comparative study of closely related pairs of animal species indicates that they must have separate origins in different geographic areas, that is, they must originally be allopatric (Kohn and Orians, 1962). Perhaps one species of hominid evolved in Africa and one in Asia only to mingle at the beginning of the Pleistocene when land connections between Eurasia and Africa presumably became reestablished. The picture as to the number of *Australopithecus* species really indicated by known material is still obscure and the available evidence can be interpreted in a number of ways.

Post-Villafranchian Morphological Changes

The Pliocene was probably a time of great morphological change in hominid evolution. Locomotor adaptations were being improved and, by middle Pleistocene time at least, the skeletons of hominids were essentially like those of modern man even though the skulls were not. Throughout this time, hominids were getting larger; this size increase was probably associated with increased speed and efficiency in running and walking. Brain size increased in consequence. Some relative increase in brain size also occurred during the early Pleistocene, although the time and extent of this expansion is difficult to assess. The changes in cerebrum and cerebellum which must have taken place are still not satisfactorily documented, nor are the selection pressures that produced them fully understood. These problems are discussed by Garn (1963: 232), who says that:

> "human brain size did increase, either because brainier *individuals* were at an adaptive advantage, or because *groups* with larger brains survived and groups with smaller brains did not. It gratifies our ego to believe that selection favored intelligence, that our own ancestral lines came to genetic fulfillment because they were so very smart. But it may be that our vaunted intelligence is merely an indirect product of the kind of brain that can discern meaningful signals in a complex social content generating a heavy static of informational or, rather, misinformational noise."

Ryle (1949) has discussed "intelligence" and "intellect" from the philosopher's viewpoint. He states (p. 26) that:

"... both philosophers and laymen tend to treat intellectual operations as the core of mental conduct; that is to say, they tend to define all other mental-conduct concepts in terms of concepts of cognition. They suppose that the primary exercise of minds consists in finding the answer to questions and that their other occupations are merely applications of considered truths or even regrettable distractions from their consideration... (However) there are many activities which directly display qualities of mind, yet are neither themselves intellectual operations nor yet effects of intellectual operations."

Brains expanded as the cultural environment became more and more complex, and larger brains enabled more complex cultures to develop. The actions which we choose, arbitrarily, to term "intelligent," that is those involving theorizing and the manipulation of true propositions or facts, form merely one aspect of our responses to a complex environment.

An increase in adult brain size involves a larger fetal and infantile brain and a prolonged growth period, two of the important trends in higher primate evolution noted by Schultz (1961). Bigger fetal brains require larger maternal pelves, and it is possible that the structural refinements in the hominid pelvis which have evolved since the Villafranchian are, to a large extent, due to the problems posed by the birth of large-brained offspring. During middle Pleistocene times, the brain increased in size with consequent remodeling of the cranial vault (Moss and Young, 1960). The facial skeleton and the teeth became reduced, presumably because of further refinements in food preparation and tool-making (Dahlberg, 1963). Changes in relative size of the brain-case and the jaws and related muscles produced changes in shape and size of the cranium, and in size and form of the supra-orbital ridges. By the late middle Pleistocene, the brain had probably reached approximately its present-day volume, and the morphological evolution of the Hominidae was almost complete.

Conclusions

Earliest hominids known to date are recognizable, in the form of *Ramapithecus punjabicus*, in the late Miocene. This sets back the differentiation of hominids from pongids to the early Miocene or earlier. Circumstantial rather than direct evidence suggests that *R. punjabicus* could have been a tool-using animal and, at least, a partial biped. It was widespread throughout the Old World apparently because of the great mobility in range extension afforded by ground dwelling and/or bipedalism. These factors would have reduced tendencies towards speciation among early Hominidae. Known geographic distribution of *Ramapithecus* (East Africa, India, China) shows that hominids have been wide-ranging, as they are now, at least since late Miocene time.

The early Pleistocene hominids can be classified in *no more than* two

species; one of these, *Australopithecus* (or perhaps *Homo*) *africanus*, found in South, East and North Africa, probably inhabited other regions too. The evolution of this species saw the final perfection of the foot and pelvis for habitual bipedal walking, the invention and spread to tool-making and the development of associated refinements in the hand, and finally, late in its history, the rapid expansion of the brain. *Homo erectus*, found throughout the Old World during much of the middle Pleistocene (from 500,000 or 600,000 years ago on), is barely distinguishable taxonomically from *Homo sapiens*.

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AMERICAN SCIENTIST 53, 1965

LIQUEFIED NATURAL GAS—A NEW SOURCE OF ENERGY*

PART I, SHIP TRANSPORTATION

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THE POPULATION explosion accompanied by an ever rising standard of living is dramatically reflected in the world's gigantic demand for more energy. As aptly stated by Schurr [1]:

> "Modern man has made himself largely by burning fuel. The supply of fuel appears to be almost inexhaustible, and a high level of fuel consumption is not a prerequisite of development but a result of it."

Sporn [2] emphasizes that the availability of energy does not guarantee industrial development; rather the capacity to consume energy, not to produce it, is the factor. Sporn's point is directed toward misconception by world leaders following the 1955 Geneva Conference on the Peaceful Use of the Atom. They believed that the advent of the atom as an energy source would not only provide a cheap, inexhaustible source of energy, but it would also convert overnight even the most underdeveloped countries into an economy and civilization comparable to the United States and Western Europe. What they failed to recognize was that the mere presence of available energy could accomplish little without the capital equipment to utilize it.

Man's social, economic, and technical progress can be measured by his progress in utilizing energy effectively [3]. The United States serves as a vivid example, with only 6% of the world's population it consumes about 35% of the world's commercial (excluding wood, animal power, etc.) energy; whereas India with about 15% of the world's population used only 1.5%. A study of our annual energy consumption reveals the economic and technical history of the United States as it emerged from an agricultural economy during the colonial period to the present age of spectacular industrial growth [4].

As late as 1880, wood was our major source of energy, but, by 1890, coal was supplying as much energy as wood. With the growth of electricity, coal quickly replaced wood and, by 1910, was accounting for 75% of the energy. Coal consumption peaked in 1918 but, with the advent of the automobile, liquid petroleum products began to rise rapidly in prominence. Following World War II, as a result of the development of welded, seamless pipe which made long distance gas transmission lines feasible, combined with an enormous upsurge in new residential housing,

* A Sigma Xi-RESA National Lecture, April 1962.

natural gas further depressed the position of coal in the world's energy picture. The growth in energy consumption and the shift in distribution of energy sources over the past three decades are summarized in Table 1.

TABLE 1

CHANGES IN ENERGY CONSUMPTION BETWEEN 1929–1960[1]

	World		United States	
	1929	*1960*	*1929*	*1960*
Total consumption in million metric tons of hard coal equivalents[2]	1711	4235	777	1448
Distribution in per cent				
Solid fuel	79.8	52.3	67.9	24.6
Liquid fuel	14.9	31.1	22.3	40.8
Natural gas	4.5	14.6	9.3	33.3
Hydroelectric	0.8	2.0	0.5	1.3

[1] *Sci. Am.*, September 1963, 114.
[2] The thermal value of all energy sources is converted to equivalents of hard coal.

It will be noted from Table 1 that natural gas experienced the greatest rate of growth, 350%; most of it has occurred in the United States. While the growth of liquid fuels in the last few years has stagnated, natural gas has maintained about a 7% annual growth rate such that, by the end of 1965, it could surpass liquid fuels as the major source of energy.

The world production and proved reserves of natural gas for 1961 are presented in Table 2.

TABLE 2

WORLD PRODUCTION AND RESERVES OF NATURAL GAS[1]
(Trillions of Cubic Feet)

	Cum. Prod. Through 1961	*Prod. 1961*	*Proved Reserves End 1961*
North America	242.8	14.6	321.0
Middle East	10.0	1.14	178.3
Iron Curtain	19.4	2.65	83.4
Africa	0.3	0.08	54.5
South America	19.5	1.7	44.8
Far East	2.8	0.17	19.6
Europe	3.0	0.55	19.1
Total Free World	278.4	18.24	637.3
Total Iron Curtain	19.4	2.65	83.4
Total World	297.8	20.9	720.7

[1] Summarized from *Oil and Gas J.*, March 12, 1962, 75.

The amount produced as shown in this table does not include the vast quantities of gas that are wasted by flaring into the atmosphere, particularly in the Middle East and South America. The proved reserves, particularly in Europe, are subject to substantial revision as of 1965 due to

the large amount of gas discovered in Holland in 1959 but only recently disclosed as the third largest gas field in the world. About 70% of the gas produced in 1961 was in the United States. The distribution among users and the corresponding revenues are summarized in Table 3.

TABLE 3

U.S. SALES OF NATURAL GAS DURING 1961[1]

Category	*Average Number of Customers*	*Thousands of Therms*(1)	*Revenues*
Residential	29,105,000	31,790,900	$3,183,981,000
Commercial	2,392,000	9,599,200	760,033,000
Industrial	136,000	46,844,400	1,622,595,000
Other	38,000	4,845,100	165,236,000
Total	31,671,000	93,079,600	$5,731,845,000(2)

Note: (1) One therm = 100,000 Btu = 100 cu ft of 1000 Btu gas
(2) Represents a gain of 8.0% in revenues over 1960 as opposed to a 2.8% gain in therms

[1] Summarized from *Oil and Gas J.*, January 1, 1962, 56.

It is interesting to observe in Table 3 that the residential customer provides more than 55% of the annual revenue which approaches 6-billion dollars. The revenue produced by the residential customer amounts to 10 cents per unit of thermal energy (equivalent to 100 cu ft of gas), whereas the industrial customer provides only 3.5 cents per therm. As will be explained later, it is this superficial bargain price that industry receives—based on interruptible supply—which actually catalyzed the development of the liquefaction and transportation of natural gas on an international scale.

TABLE 4

COMPARISON OF VARIOUS SYSTEMS FOR STORING AND TRANSPORTING NATURAL GAS[1]

System	*Cu Ft of Volume Req'd. to Contain 1000 Cu Ft of Gas*	*Conditions* Temperature, °F	Pressure, psi
Liquefaction	1.6	−250	14.7
Adsorption (Fuller's Earth)	5.0	−250	14.7
Hydrate	5.9	35	450
Absorption (in propane)	6.5	− 52	600
Compression	25.0	100	600

[1] *Chem. Eng. Progr.*, November 1962, 47.

Judging from its effective utilization in the United States, particularly in the last 20 years, natural gas plays a prominent role in an expanding, industrial economy. The situation in the United States has been unique

in that it not only has about half of the world's proved reserves of natural gas, but also the gas can be distributed competitively from the major producing areas in the southwest* to all the consuming centers by means of pipelines. On the other hand, a number of the highly industrialized nations like England and Japan are (as yet) neither blessed with indigeneous gas reserves nor can they be reached by pipeline from major producing areas. Since it is impractical to transport natural gas as a gas

TABLE 5

PROPERTIES OF METHANE

Formula	CH_4
Molecular weight	16.042
Gas density at 60°F and 1 atm. in lb/cu ft	0.0424 (0.679 gm/liter)
Specific gravity at 60°F and 1 atm (air = 1)	0.555
Critical temp., °F	−116.5°F (−82.5°C)
Critical press., psi abs.	673.1 (45.8 atm.)
Boiling point at 1 atm. press.	−258.68°F (−161.5°C)
Freezing point at 1 atm. press.	−296.46°F (−184°C)
Density of liquid at −263°F in lb/cu ft	25.9 (415 gm/liter)
Heat of vaporization at boiling point in Btu/lb	219.7 (122.1 cal/gm)
Net heat of combustion at atm. press. in Btu/cu ft	911 (21,240 Btu/lb or 11,800 cal/gm)
Flammability limits volume per cent in air	
Lower	5.0
Higher	15.0

in bulk form, one obvious solution would be to liquefy the natural gas and transport it as such in ocean-going tankers. The advantage in liquefaction is that roughly 600 cu ft of natural gas at atmospheric pressure shrinks to 1 cu ft of liquid. Other techniques which have been considered are transporting in the gas phase at high pressure, by absorption in liquids, by adsorption on solids, and by reversible chemical combination [5]. A comparison of these various systems is given in Table 4.

Since it is impractical to store or transport large volumes of gas at any pressure above atmospheric, the most competitive alternative to liquefaction is absorption on fuller's earth. Other solids such as carbon can be

* Natural gas has been found under less than 1% of the land area of the United States. This gas is transmitted in pipelines (10 to 36 inches in diameter) to consuming markets as far as 2000 miles away at pressures between 200 and 1000 psi.

used, but its adsorptive capacity is less than 10% of fuller's earth. Considerable development work was done, including the operation of a pilot plant in Warren, Pennsylvania, by the Floridin Company and J. F. Pritchard Company in 1949–50, on fuller's earth adsorption [6]. However, because of its 3:1 disadvantage on containment volume as compared to liquefaction, and the fact that the same temperatures and pressures are required, this process has never been commercialized.

At this point, it may be well to review some of the more pertinent properties of natural gas. It is found in porous, subsurface, imperviously-capped formations in various parts of the world (see Table 2). Some wells are more than three miles deep. Natural gas is frequently found with petroleum; about one-third of the U.S. production comes from oil wells.

TABLE 6

BOILING POINTS OF SOME COMMON GASES

Substance	*Normal Boiling Point in °F*
Ammonia	− 28.1
Freon 22	− 41.4
Propane	− 43.7
Carbon dioxide	−109.3 (sublimes)
Ethane	−127.6
Ethylene	−154.8[1]
Freon 14	−198.4
Methane	−258.7
Oxygen	−297.3
Nitrogen	−320.5
Hydrogen	−423.0
Helium	−452.1

[1] Temperatures below −150°F are usually considered cryogenic in the trade.

Natural gas in the United States generally contains between 80 and 95% methane;* the balance includes ethane, propane, butane, pentane, etc. Small amounts of carbon dioxide, nitrogen, helium, water vapor, and hydrogen sulfide are present in most natural gases. Natural gas, along with petroleum, oil shale, natural gas liquids, coal, and lignite, is classed as a fossil fuel.

Where the natural gas is composed primarily of methane, much of the characteristic behavior of natural gas can be predicted from the properties of methane, which are summarized in Table 5.

Since the critical temperature of methane is −116.5°F, it cannot be liquefied at any pressure, however great, above this temperature. The liquefaction temperature or boiling point at one atmosphere pressure is −258.68°F. This boiling point is compared with other gases, which are commonly liquefied, in Table 6.

Liquid methane is a colorless, clear liquid that resembles liquid air;

* In the Middle East, much of the gas contains less than 50% methane.

its density is about one-half that of liquid air. Because it possesses superior wetting characteristics, liquid methane produces a more severe irritation in contact with the human skin than liquid air does. It is this characteristic wetting property that may serve a useful purpose in cryogenic surgery as a replacement for liquid nitrogen.

Historical

The commercial liquefaction of natural gas dates to a small plant which was built in West Virginia in 1910 to compress natural gas, refrigerate and separate what was called liquid natural gas—mostly ethane and propane—which was bottled and sold locally. A patent application had been made by Cabot as early as 1914 for the liquefaction, storage, and barge transportation of liquid natural gas [7] and, in 1917, a patent was issued (U.S. Pat. 1,225,574) to him covering the apparatus for condensing natural gas under high pressure and cooling.

In 1917, during World War I, the United States Government comissioned the Linde Company, working in cooperation with the Bureau of Mines, to construct a plant in Forth Worth, Texas, for extracting helium from natural gas. The helium was to be used in dirigibles for the Allies. By the time enough helium (90% purity) had been produced to fill one dirigible and was readied for shipment overseas from New York, the Armistice was signed [8, 9]. The process of recovering helium was based on liquefaction of natural gas. Some 10 years earlier, Professors H. P. Cady and D. McFarland at the University of Kansas discovered that many natural gases contained around 1% helium.

After the war, the government, under the jurisdiction of the Navy, constructed a larger extraction plant at Forth Worth in 1921. In 1925, Congress placed all helium activity under the U. S. Bureau of Mines at Fort Worth. When the gas field near Forth Worth played out, the plant was closed and dismantled. A new one was erected near Amarillo, which went into operation in 1929 and has continued to produce helium ever since.

In the early 1920's, patents were issued on insulated containers for river barges suitable for transporting liquefied gas [10]. In 1937, two patents were issued (U.S. Pat. 2,082,189 and 2,090,163) to L. Twomey on methods of liquefaction, storage, and delivery of liquefied natural gas through distributing lines [11].

In 1937, H. C. Cooper*, then president of the Hope Natural Gas Co., became interested in liquefaction of natural gas with the result that a pilot plant was erected at Cornwell compressor station of the Hope Natural Gas Co. of West Virginia in 1940. The liquefaction capacity was 300,000 cu ft of natural gas per day into a cork—insulated storage con-

* About this same time, Egerton in England was trying to promote the separation and storage of liquid methane by the British Gas Industry to meet seasonal variations in demand on manufactured gas [12].

tainer for 1 million cubic feet of gas (equivalent to 14,500 gal of liquid). Because of the successful operation of this plant, it was used as the basis for the design of a larger installation at Cleveland [10].

The Cleveland Natural Gas Liquefaction Plant of the East Ohio Gas Company went into operation on January 29, 1941. Total construction costs were $1.25 million. This plant was known as a *peak-shaving plant* since its purpose was to liquefy surplus natural gas from the pipeline during the periods of low customer demand in the summer and to regasify the liquid from storage to supplement the pipeline gas during peak demands in the winter. This plant was the first and only one of its kind in the United States: however, at least two liquefied natural gas, peak-shaving plants are scheduled to go on stream in 1965.

The Cleveland plant had a capacity to liquefy 4 million cubic feet of gas a day, to store a total of 150 million cubic feet of gas as a liquid (equivalent to 1.8 million gallons of liquid) in three cork-insulated, (3 ft thick) spherical tanks (57 ft in diameter), and to regasify the liquid at a daily rate of 72 million cubic feet. After three years of successful operation, a fourth tank was installed in 1944. This tank was a vertical cylinder, 70 ft in diameter and 43 ft high surrounded by 3 ft of rock-wool insulation. Its capacity was 90 million cubic feet of gas (equivalent to 1.1 million gallons of liquid). Eight months after installation, the new cylindrical tank failed on October 20, 1944. Because of inadequate dikes, liquefied natural gas (hereafter referred to as LNG) flowed over the ground surface and into the sewers of the city. The resulting explosion and fire caused widespread destruction ($6.8 million) and loss of lives (128). A team of investigators from the Bureau of Mines observed that, among several possibilities, the most likely cause of failure was due to the improper selection of metal. The 3½% nickel steel, which was used, is not considered adequate for this service even by present day standards of greatly improved fabrication techniques. The resulting damage, following tank rupture, was attributed to the lack of confinement of the storage tanks by earthen dikes. Despite the magnitude of the disaster, a significant conclusion by the investigation team is quoted from the Bureau of Mines report [13]:

> "Regardless of the cause of the disaster at the liquefaction, storage, and regasification plant of the East Ohio Gas Company, the application of the system for liquefying and storing large quantities of natural gas is not invalidated, provided proper precautions are taken."

Nevertheless, the Cleveland plant was not operated again.

In 1947, Dresser Industries Limited of Dallas, Texas, designed and constructed a plant near Moscow, Russia at a cost of $6,000,000. This plant had a liquefaction rate of 4.5 million cubic feet of gas per day (comparable to the Cleveland plant) with an equivalent gaseous storage volume of 162 million cubic feet. Little information is available on the

operating record of this plant other than it has been giving continuous, satisfactory service. Although the original purpose of the plant was to supply standby gas for Moscow, it is being used to supply vehicle fuel and to supply large customers at points not on gas lines. A description of this plant has been published [14, 15].

In 1949, detailed designs were completed and approved by regulatory authorities for the liquefaction and storage of 400 million cubic feet (equivalent gaseous volume) of LNG by the People's Gas Company of Chicago. This plant would have been built had it not been for the concurrent development of the alternative and cheaper means of storing gas in depleted, undergound, oil and gas reservoirs.

Throughout the 1940's, there was substantial activity in the nature of engineering studies and designs for the liquefaction, storage, and transportation of LNG. Huntington observed in 1950 [16]:

> "Though it may sound fantastic and impractical to many, the proposed transportation of liquid methane by tanker from South Texas to the Atlantic seaboard has been given serious consideration."

However, despite the wishful dreams of the petroleum, gas, and utility companies to capitalize on LNG in its various ramifications and the fact that the technology of liquefaction and storage (particularly in the rapid growth of air liquefaction plants in the 1940's) was well established, none seemed willing to make the capital investment. Two explanations are suggested:

(1) The so-called Cleveland disaster was still fresh in their minds.
(2) The transportation of LNG by tanker or barge involved the solution of a number of technological problems which were not straightforward. For this reason, economic analyses of this phase of the venture always tended to be so ultra-conservative that the overall economics became unattractive.

It was left to a relatively small power company, which was generating power for the Chicago Stock Yards, and its dynamic chief executive, William Wood Prince, to break the ice and take the bold plunge. Armed only with his self-imposed motto of awareness: "Remember Cleveland," he rolled-up his sleeves and went to work on LNG in 1951.

The Constock Breakthrough

Most of the gas supplied to industry falls into the so-called interruptible category. Contracts with gas companies recognize that domestic consumers have first priority on the supply. In periods of severe weather, the supply to industry frequently has to be restricted. For this reason, industry must maintain standby facilities such as coal, liquid fuels, or manufactured gas to carry them through the period of interrupted service. However, industry is willing to accept this inconvenience so long

as the gas companies will grant them a bargain price for the gas when there is a surplus during the summer months. As was noted in the comments related to Table 3, the average price paid by the industrial customer is about one-third as much as the domestic customer per unit of thermal energy.

According to one source [17] "a Chicago gas company injudiciously tried to raise the price of the natural gas (interruptible) it was selling to a power company controlled by William Wood Prince," then president of Union Stock Yard and Transit Co. and a managing trustee of the 30-company Prince trust.* It was then—in 1951—that W. W. Prince and one of his consultants, Willard Morrison, conceived the idea of liquefying natural gas on the Gulf Coast and barging it up the Mississippi River to Chicago. The original plan, which was handed to his Chicago Stock Yards Research Division to develop, was to construct a barge-mounted liquefaction unit which could move around to nearly-depleted or remote gas wells (where the cost of gas is very low) and to liquefy the gas directly into another barge for transporting the cargo to Chicago. Another feature of the plan was to utilize the refrigeration contained in the LNG, during regasification at Chicago, to freeze and preserve the various products from the stockyards operation.

After considerable preliminary investigation, research, and development, construction of the barge-mounted liquefaction plant and the transport barge were undertaken in 1954 at the Ingall's Ship Yards in Pascagoula, Mississippi. About this time, Prince decided it would be desirable to have a close working association with a company experienced in gas processing. After talking to several prospects, he found an understanding ear in E. F. Battson, senior vice-president of Continental Oil Company. Continental Oil took a one year's option on a possible joint venture with the Stock Yards Group. A task force composed of Continental Oil Company personnel and several consultants was organized under the direction of J. A. Murphy, who at that time was serving as Battson's technical advisor. This group not only made a detailed evaluation of the work done by the Chicago Stock Yards Research Division, but they also carried out an independent, economic and engineering feasibility study. These studies concluded that the Mississippi barge venture was not economical, but that ocean transportation of LNG from gas-surplus countries to gas-deficient countries offered a very attractive potential. With this understanding, Continental Oil exercised its option to a joint venture, and, in 1955, Constock International Methane Ltd. was organized with equal ownership by Continental Oil Co. and Union Stock Yards and Transit Co. The name Con-stock was obviously derived from the parent companies.

* Currently, also, Chairman of the Board and Chief Executive Officer of Armour & Co.

Even though the original barging scheme was abandoned, it was decided to utilize both barges as pilot plants for demonstrating the technical feasibility and obtaining valuable design data for a commercial venture. The barges were completed in late 1955 and were then transfered to Bayou Long, Louisiana, for extensive testing throughout 1956.

While these tests were underway in Bayou Long, Constock concentrated on a crash program of research, development, and engineering analyses on all phases of the project, directed toward the commercial venture. Included were innovations in gas processing and liquefaction techniques, material evaluation and development, ship designs, cargo handling, storage tanks, etc. The most amazing aspect of the program was the way it was accomplished by its subsidiary, Constock Liquid Methane Corporation. They operated with a skeleton staff directed by J. A. Murphy and housed in less than 500 square feet of space in a New York office building. Specific research and development assignments were doled out to the laboratories of the parent companies in Chicago and Ponca City, Oklahoma. In addition, several consultants from universities were employed on a part-time basis, primarily to translate the research results into design criteria for practical applications. The bulk of the engineering design and construction was handled by the following:

J. F. Pritchard Co., Kansas City, Mo. (gas processing, liquefaction, and plant construction)
Gamble Brothers, Inc., Louisville, Ky. (wood and insulation specialists)
J. J. Henry Co., N.Y. (naval architects and marine engineers)
A. D. Little, Inc., Cambridge, Mass. (storage and cargo handling methods)

By the spring of 1957, complete designs, specifications, and drawings for the liquefaction plant, tanker, and terminal facilities had been completed. Comprehensive analyses of potential gas sources and markets were also made. It was therefore possible to establish the economics for LNG shipments between a variety of ports all over the world. At this stage, a number of foreign countries, among which were England, Germany, France, Italy, Sweden, and Japan, expressed interest in importing LNG. By the fall of 1957, the British Gas Council made a declaration of intent to import LNG at a rate equivalent to 100 million cubic feet of gas per day, which amounted to about 10% of their total gas consumption. However, the signing of a firm contract was deferred until after several trial shipments of LNG were made between the Gulf Coast and London. Even though the Constock ship designs appeared to be sound, there was as yet no proof that LNG could be transported overseas by tanker, particularly in rough weather.

Constock agreed to erect liquefaction and land storage facilities on the

Calcasieu River near Lake Charles, Louisiana, while the British Gas Council supplied the unloading and terminal storage facilities at Canvey Island near London. Constock and the Gas Council agreed to share in the costs of converting a dry cargo tanker; for this purpose, a new company, British Methane, Ltd. was formed to own and operate the ship.

Lake Charles Facilities: The liquefaction plant, land storage, and loading terminal were erected on a 20-acre site and placed in operation during the latter part of 1958. The barge-mounted liquefaction unit,

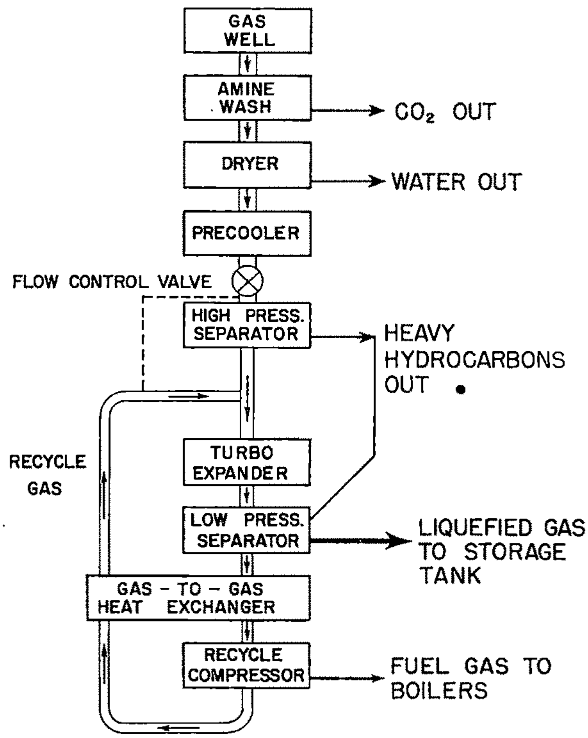


FIG. 1. Flow sheet of methane liquefaction barge

which was used in the Bayou Long tests, was moved to Lake Charles. A simplified flow sheet of the modified Claude (expander) cycle used on the barge is shown in Figure 1. Although this cycle is relatively inefficient, it has the advantage of being lighter and more compact, and therefore more adaptable to installation on a barge where space is limited. It is interesting to note that this pilot liquefaction unit with a rated liquefaction capacity of 7 million cubic feet of gas per day was 1.7 times larger in capacity than the Cleveland or Moscow plants of the 1940's.

The liquid from the plant was stored in a tank having a capacity of 1.47 million gallons (equivalent to 120 million cubic feet of gas). Up until 1964, this tank was the largest ever built for storing cryogenic liquids (below $-150°F$). This container is double-walled, with an aluminum inner tank separated from an outer steel tank by 3 ft of perlite insulation. Its outer dimensions are 73 ft in diameter by 61.5 ft high. A photograph of the site at Lake Charles, Figure 2, shows the storage tank and barge-mounted liquefaction unit [18, 19].

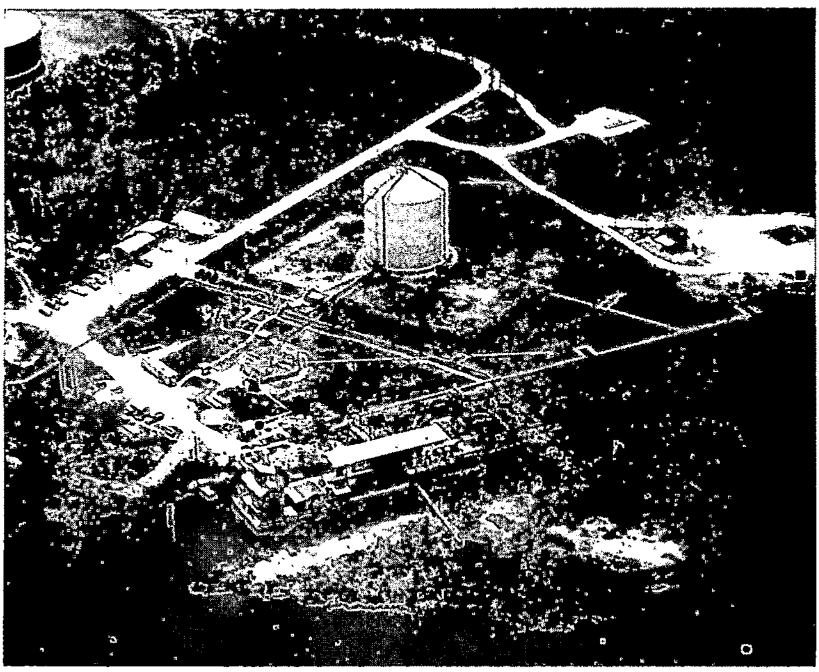


FIG. 2. Constock's LNG liquefaction terminal at Lake Charles, La.

Methane Pioneer: For transporting the LNG, a Cl-M-AVl dry cargo ship (5000-ton class) was converted to the MV Methane Pioneer at the Alabama Drydock & Shipbuilding Co. in Mobile, Alabama, during 1958, according to plans and designs developed by Constock and J. J. Henry Company. A dry cargo ship was selected because it has large double bottoms and wing tanks which can be used for ballasting, a particular problem raised by the low density of liquid methane (about 40% of the density of water). In addition, a number of other innovations in outfitting the ship for cryogenic service were required [19, 20, 21]. However, only the insulation and cargo tanks will be mentioned here since they represent the major accomplishments upon which the success and economics of the project hinged [22].

The horizontal cross section of a tanker's hold space is essentially rectangular. Therefore, in order to obtain the maximum utilization of this space for liquid cargo, the horizontal cross section of the cargo tanks must likewise be rectangular. Cylinder tanks would be easier and much cheaper to fabricate, but unfortunately they would utilize only $\pi/4$ or about 80% of the cargo space. Because of the high cost of the ship—almost twice the conventional tanker—for LNG service, the economics dictate that the space utilization should be greater than 90%; thus

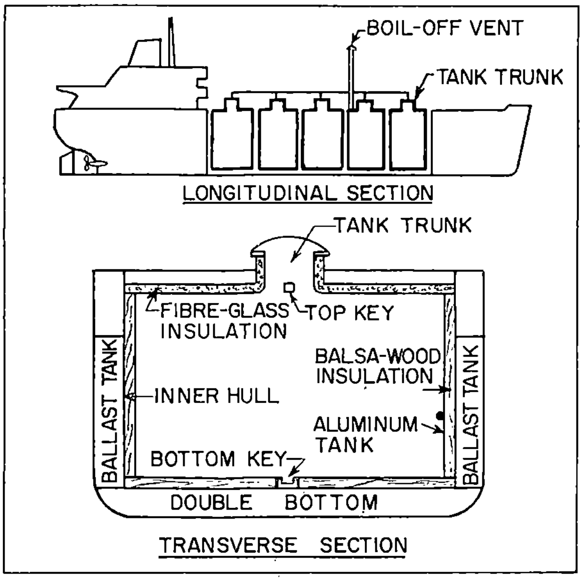


FIG. 3. Cross sections of vessel showing prismatical tanks

prismatical tanks (rectangular parallelopipeds) are used. Unfortunately, prismatical tanks introduce two severe design problems:

(1) High stresses in the flat walls caused by cyclical, dynamic loads from rolling, pitching and heaving of the ship.
(2) Thermal stresses in the walls caused by sharp, vertical temperature gradients when the tank is only partially filled.

Obviously, one large tank filling the entire cargo space would be cheaper than several small ones. However, regulatory bodies for ships have limits on the size of individual compartments with respect to dynamic loadings, free-surface liquid effects, and safety under collision conditions.

Working within these shape and size limitations, the next step is to select the material of construction. Although all materials, metals, plastics, concrete, wood, etc., show an increase in strength with de-

creasing temperature, most of them become brittle at low temperatures. For this application, the choice narrows to aluminum (5000 series alloy), stainless steel, or 9% nickel steel [23]. Aluminum is selected on the basis of economics.

As was mentioned before, the prismatical tanks are expensive so it was necessary to optimize the design. In order to do so, a method for

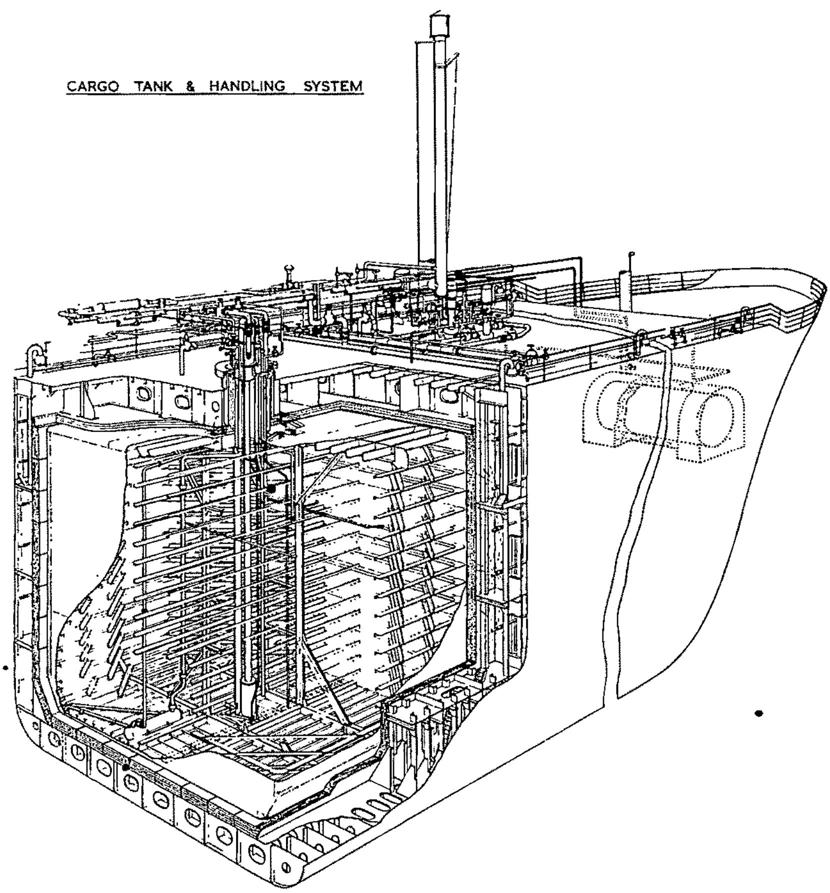


FIG. 4. Cutaway of cargo tank showing internal bracing

making stress analyses had to be developed [22]. The general configuration of the tanks and relative location in the ship are shown in Figures 3 and 4.

In Figure 3, it can be noted that the aluminum tanks are surrounded with insulation, which had to meet the following requirements:

(1) The insulation must maintain the ship's structure near ambient temperatures. Since the steel used in the hull cannot withstand LNG

temperatures without becoming brittle, the insulation must also perform as a secondary, liquid-tight barrier in the event one of the aluminum tanks springs a leak.

(2) The bottom insulation must be capable of withstanding the enormous stresses (due to the ship's motion) generated at the bottom key of the tank.

(3) The insulation must provide a prescribed rate of boil-off of LNG if the vapors are to be used to generate power for propelling the ship.

(4) Since the insulation has to be attached to the ship's inner hull, it must be able to withstand the severe thermal stresses (−250°F on one face and ambient on the other) without yielding.

(5) In the event of fire on board ship, the insulation must be able to maintain structural integrity for at least four hours when its outer face is exposed to a temperature of 1200°F.

TABLE 7

RATIO OF YIELD STRESS TO THERMAL STRESS FOR COMMON MATERIALS

Material	$(S_{YP}/\alpha E(\Delta T)^1{}_{max}$
Wood	2.2–9.2
Cast iron	2.3
9% nickel steel	1.2
Foamglas	0.45
Concrete	0.3
Stainless steel (304 annealed)	0.28
Aluminum alloy (5000 series, annealed)	0.22

[1] S_{YP} = Yield point in compression.
α = Coefficient of thermal expansion.
E = Modulus of elasticity.
ΔT = Temperature differential, 70°F to −320°F = 390°F.

To satisfy these particular requirements, balsa wood was the only insulating material that was adequate in all respects. In general, any material, whose ratio of yield stress to thermal stress is greater than one, can be subjected to cryogenic temperatures in a fully restrained condition so that it is not free to contract. Table 7 compares this ratio for several common materials of construction. Although cast iron has a ratio of 2.3 it is not suitable for cryogenic service because it becomes brittle even at 0°F. Nine per cent nickel steel is not recommended for use below liquid nitrogen temperatures. With its high ratio, 2.2–9.2, wood is one of the best materials for cryogenic service.* Its chief disadvantage lies in the difficulty to predict stresses. Being an anisotropic material, one must consider three ultimate strengths and 9 Poisson ratios in analyses.

* Wood is even finding use at high temperatures, such as in spacecraft nose cones [24].

The final design problem involves the insulation and cargo tanks together. Economics might indicate a thickness of insulation which results in prohibitive thermal stresses being generated in the tank walls, since the thermal stresses increase with decreasing insulation thickness as shown in Figure 5.

The installation of the aluminum tanks is shown in Figure 6. Figure 6A shows the hold space lined with balsa panels which are so laminated as to give identical physical properties in two dimensions. The panels

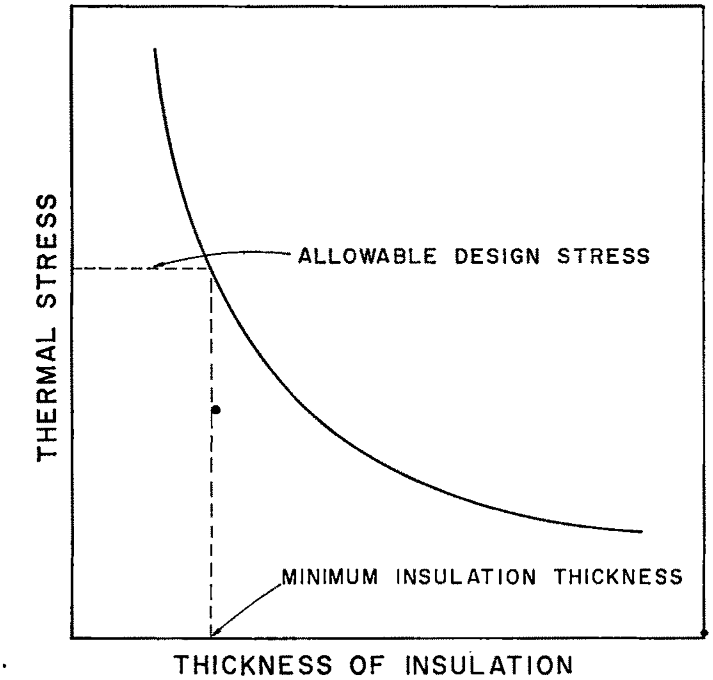
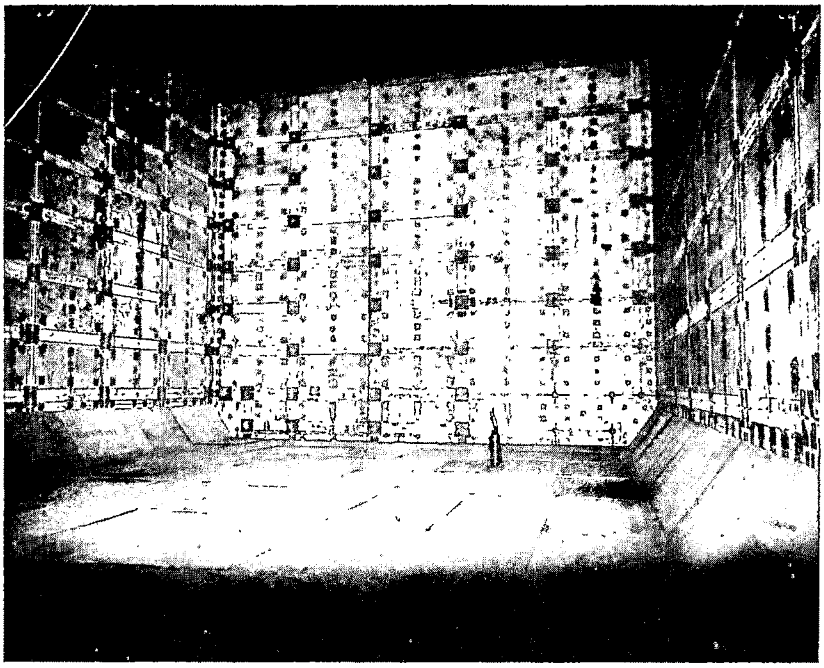
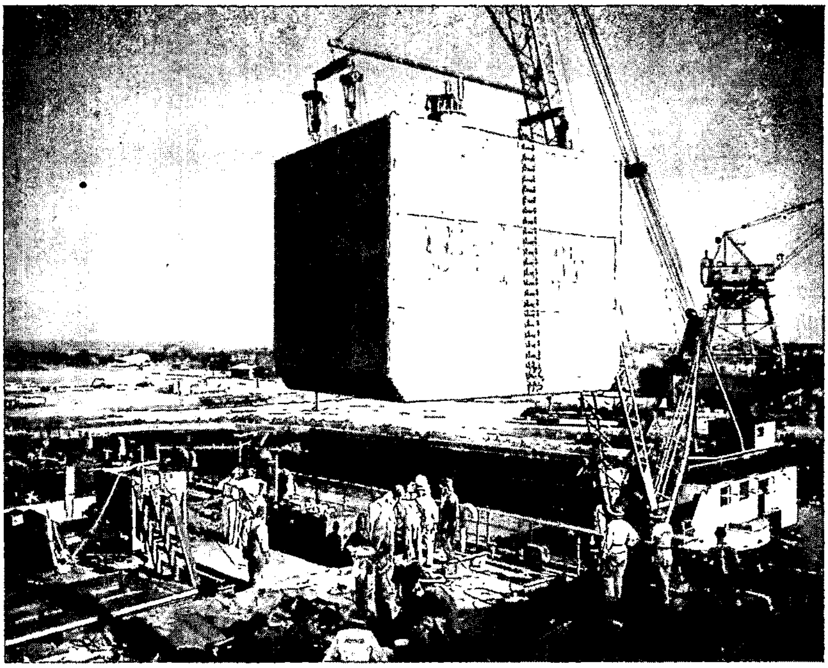


FIG. 5. Variation of thermal stresses in tanks with thickness of insulation

are faced with ⅛ inch plywood. They are 4 × 8 × 1 ft thick and are joined together with plywood scabs and fiber glass rosettes. Figure 6B shows one of the aluminum tanks, measuring 29 × 40 and 32 ft high, having a capacity of about 280,000 gal being lowered into the hold. Figure 6C shows the first tank in place, the clearance between the sidewalls of the tank and insulation averages about 1 inch in order to maximize on cubic carrying capacity. Four more similar tanks, when installed, bring the total capacity to over 1.40 million gallons (equivalent to 115 million cubic feet of gas).

Figure 7A shows the appearance of a Cl-M-AVl dry cargo ship before conversion, and Figure 7B, the Methane Pioneer after conversion. The





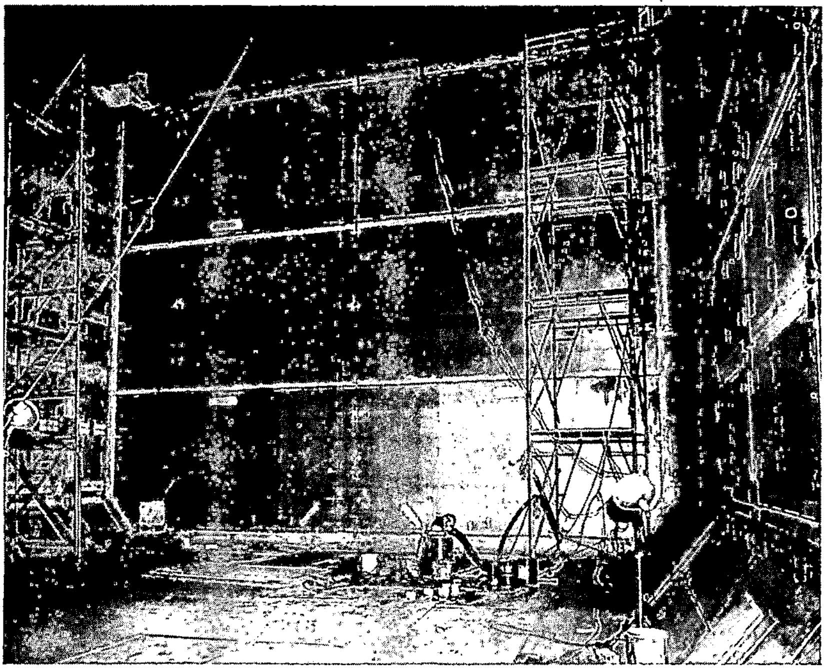


FIG. 6. Installation of aluminum tank in methane pioneer
A. Insulated hold space
B. Lifting the tank
C. Installed tank

ship is over 350 ft long, 50 ft wide, and 40 ft deep, a healthy-sized pilot ship but tiny compared to present day tankers.

The Methane Pioneer took on its first load of LNG, equivalent to 115 million cubic feet of gas, and departed from Lake Charles on its historic voyage, January 28, 1959. It arrived at Canvey Island on February 20, 1959, and discharged its cargo into 2 insulated, land storage tanks, each having a capacity of 670,000 gal (equivalent to 55 million cubic feet of gas).

Over the next year, ending in March 1960, the Methane Pioneer made six more trial shipments. The ship performed admirably, even in very heavy seas where rolls exceeding 45° were recorded [19, 20, 25]. Having proved that LNG could be transported overseas by tanker, LNG shipments from Lake Charles were terminated because the Methane Pioneer was only a pilot ship and had less than one-fifth of the minimum carrying capacity for economical operation. Since then, the Methane Pioneer has continued in service as a refrigerated butadiene carrier between the Gulf Coast and Holland.

An idea which was conceived nine years earlier by W. W. Prince to solve a local power company's problem in Chicago had become an inter-

national enterprise. The world awaited with great interest, particularly the skeptics—and there were many—for the maiden voyage of the Methane Princess. Hardly had she docked at Canvey Island before many others were dashing madly to get into the LNG business and reap the benefits at the expense of Constock's pioneering leadership. Constock, after nine years and some $15 million of investment had proved not only to herself but—with full knowledge aforethought—for all her potential competitors that the job could be done. Although Constock had developed a protective wall of several hundred international patents, these alone were not enough to discourage competition.

Commercial Ventures

Early in 1960, Royal Dutch/Shell joined forces with Constock, and a new company, Conch International Methane Ltd., was formed. Royal Dutch/Shell and Continental Oil Company each acquired a 40% interest and Union Stock Yards and Transit Company retained the balance of 20%. (The name Conch is derived from CON = Continental, CH = Chicago Stock Yards, and CONCH = (sea) Shell.) The headquarters were moved to London to initiate the first commercial venture for hauling LNG from North Africa to England. The only activities that remained in the States were the pilot plant projects on gas purification, submerged pumping, in-ground storage, and fire tests, at Lake Charles; insulation development at Gamble Brothers in Louisville, and basic research at Continental Oil Company in Ponca City, Oklahoma.

The London headquarters were rapidly expanded to provide for research, development, engineering, marketing, and patent services. Their first step was to initiate construction of a liquefaction plant and larger tankers modeled after the plans developed by Constock. Shortly thereafter, in the spring of 1960, Conch entered into an agreement with the French government to purchase gas* from the big Sahara Hassi R'Mel gas field and to build a liquefaction plant near Arzew, Algeria, on the North African Mediterranean Coast. The liquefaction plant was to be financed and built by a new company, CAMEL (Cie Algierienne du Methane Liquid) owned jointly by Conch and French interests.

Although Britain presumably was to provide the initial market for this plant, and construction of the plant was undertaken, more than 18 months elapsed before the U. K. government announced its approval in January 1962 [26]. The delay was due to political angles involved: coal-industry opposition and the Algerian turmoil following its liberation, which raised questions regarding the stability of the Saharan gas as a source for imports. However, in the final analysis, the

* Contrary to popular belief, the contract price for the gas was around 25 cents per 1000 cu ft which is no better than current gas prices on our Gulf Coast.

British Gas Council decided that the Sahara gas was as reliable as the British sources for oil which—in the minds of many Europeans since the Suez crisis—are not too reliable [27].

FIG. 7. A. Cl-M-AV1 before conversion
B. Methane pioneer after conversion

After Algeria gained its independence, it demanded and obtained additional participation in the LNG venture. The final lineup of operating companies is as follows [28]:

1. Liquefaction Plant by CAMEL (40% Conch, 40% French interests, and 20% Algerian Government).
2. Gas Production at Hassi R'Mel Gas Field by Ste. d'Exploitation des Hydrocarbures d'Hassi R'Mel.
3. Pipeline* from gas field to Arzew by SOTHRA (combination of French and Algerian interests).
4. Transportation by British Methane Ltd. (50% Conch and 50% British Gas Council) which buys LNG, ships to England, and sells to British Gas Council.
5. Transportation by Gaz de France, the French counterpart of the British Gas Council, which buys LNG and ships to France [29].

With the decision by the French to participate in the initial venture by contracting for 50 million cubic feet of gas daily, the capacity of the liquefaction plant was raised from 100 million to 150 million cubic feet per day, with Britain under a 15-year contract to take 100 million cubic feet per day. The liquefaction cycle selected was based on a cascade cycle developed by Constock. The terminology "cascade" derives its usage from the fact that the gas is progressively cooled in a series of

* Three hundred miles of 24-inch pipeline.

steps as shown schematically in Figure 8. The second law of thermodynamics provides that, as the difference in temperature between the refrigerating medium and the medium being cooled is diminished, the more efficient the process becomes. In the limit, the most efficient process would be one in which the temperature difference was zero. However, practical, cost limitations force a compromise to a finite temperature difference, which as noted in Figure 8 amounts to 5°F in each step except one of the ethylene stages (8°F).

TABLE 8

COMPARISON OF LNG LIQUEFACTION CYCLES

Cycle	*Inlet Gas Pressure, psi*	*Horsepower Per Million Cu Ft Gas/Day*	*Per Cent of Total Feed Used as Fuel*
Ideal	500	185[1]	
Practical limit	500	400[2]	
Cleveland cascade	615	662[1]	15
Russian cascade	725	660[3]	
Constock barge expander	1000	1000[1]	30
Arzew cascade	465	469[1]	8
Conch expander	465	540[1]	10
Transco cascade peak shaving	315–490	716[4]	

[1] C. L. Ritter, *Chem. Engr. Progr.*, *58*, 61–69 1962.
[2] Estimated.
[3] P. S. Parker and R. H. Armston, *Gas Age*, March 11, 1954.
[4] R. Martin, *Petrol. Management*, Dec. 1964, 84–89 (Actual Horsepower Required is 624).

A comparison of the power and fuel consumptions for the various LNG liquefaction plants that have been built to date is presented in Table 8. Although the theoretical, ideal power requirement is only 185 horsepower per million cubic feet of gas per day, the practical fact that no machinery, such as compressors and expansion engines, have been built, as yet, which are 100% efficient, and all economical heat exchangers must operate with finite temperature differences, raises the theoretical limit from 185 horsepower to a so-called practical limit (by today's standards) of 400 horsepower. The Arzew plant is not far away from this limit with 469 horsepower.

The storage facilities for liquefied natural gas at Arzew amount to 17 million gallons of liquid or equivalent to 1-billion cubic feet of gas [28]. There are 3 double-walled, above-ground storage tanks, each having a capacity of over 2.5 million gallons. These tanks are modeled after the Lake Charles tank with a scaleup factor of about two. Two of these tanks have an inner shell of aluminum and the third a 9% nickel steel inner tank. All three have 3 ft of perlite insulation between these inner shells and an outer shell of conventional steel.

The rest of the storage, which amounts to 55% of the total, or 9 million gallons, will be in a frozen cavity 122 ft in diameter and 114 ft into the ground. This in-ground storage principle was developed by

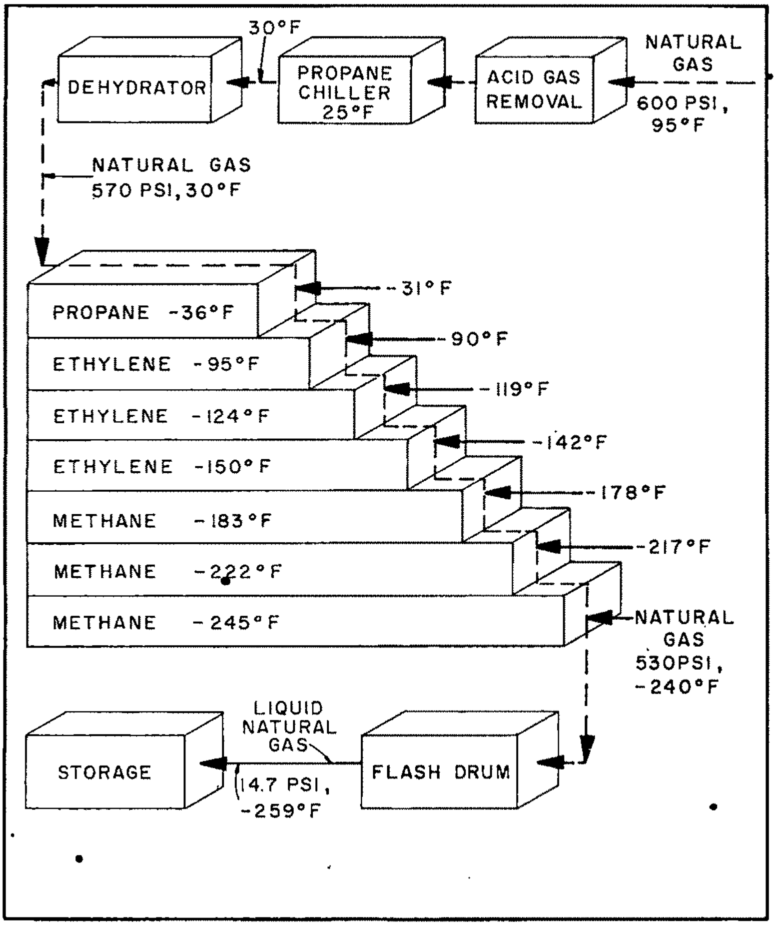


FIG. 8. Schematic of liquefaction of natural gas by means of cascade cycle in Algeria (data from *Chemical Engineering*, October 12, 1964, 182–184).

Conch [30] and is based on the principle that the moisture in the ground freezes to form a frozen cavity, which serves as an ice barrier that prevents leakage and at the same time supplies adequate insulation. The use of this in-ground storage results in a saving over conventional above-ground storage in the neighborhood of about 5 cents per gallon or, in this case, about $450,000.

The storage facilities will supply three tankers, each of them making about 30 round-trips per year, and delivering about 7 million gallons of

liquid (equivalent to over 500 million cubic feet of gas) per trip. Two of the tankers, the Methane Princess and the Methane Progress were built in British and Irish shipyards respectively. These tankers are essentially replicas of the Methane Pioneer in having prismatical aluminum tanks and balsa wood insulation. However, the French-built tanker, appropriately christened the Jules Verne,* has seven cylindrical tanks of 9% nickel steel insulated with 17 inches of Klegecell[1] on the bottom (conical shaped, rather than keyed). The vertical walls and roof consist of 2½ inches of Klegecell covered with a liquid-tight sealant to act as a secondary barrier. The walls and roof are further insulated with perlite [31]. Apparently, the French were able to realize sufficient economies in going to the cylindrical, rather than prismatical tanks, despite the 20%

TABLE 9

APPROXIMATE COSTS FOR LIQUEFACTION AND OVERSEAS TRANSPORTATION

1. Capital investment			
a. Liquefaction plant 100 million cu ft/day		$\$40 \times 10^6$	
b. 2 Tankers @ $\$13 \times 10^6$			
Basic ship	50%		
Cargo handling	20		
Insulation	12		
Alum. tanks	18	26×10^6	
			$\$66 \times 10^6$
2. Annual operating costs (source to market about 1500 miles)			$\$13 \times 10^6$
3. Direct cost liquid natural gas delivered: *Before* taxes, administrative, research, and sales expense and *excluding cost of* gas at source		$0.36/1000 cu ft	

loss in cubic capacity. It has been reported [31] that the French ship cost $10 million to build against the $13 million for its British counterpart; the $3 million savings could have tipped the balance for cylindrical tanks, not to mention a possibility that the Conch-patented ship designs might have been evaded.

The terminal facilities on Canvey Island consist of seven conventional above ground storage tanks, including the two that were used on the trial shipments with the Methane Pioneer, for a total capacity of 140 million gallons or a gas equivalent of over 1.1 billion cubic feet. The liquid is vaporized from storage in a novel scheme of heat exchange with propane and sea water which results in a net production of about 1.7 megawatts of power as a bonus. The vaporized gas then goes into an

* French science fiction writer who half a century ago referred to natural gas as a major energy source.

[1] Polyvinylchloride.

18 inch pipeline extending some 300 miles northwest for delivery to 8 local Gas Boards; who will either blend the natural gas with manufactured gas or reform it to the lower calorific value needed in domestic burners [32].

The investment costs for an operation liquefying and transporting LNG are enormous; an order of magnitude estimate is given in Table 9. To these figures must be added the cost of the unloading terminal facilities, which are comparable in cost to the liquefaction plant if gas reforming and pipeline distribution facilities are included.

The costs given in Table 9 are to be viewed with reservation since they can vary widely depending on the locality. For example, one reference [33] quotes that the Arzew liquefaction plant cost $60 million. Even correcting the figure of $40 million given in Table 9 for a 100- to 150-million cubic feet per day plant, the corresponding figure would be around $50 million. A later reference [32] pegs the total costs in Algeria at $70 million; another reports $86.8 million [28]. From these figures, it is possible to arrive at an estimated total investment in this first LNG project as summarized in Table 10 in round figures.

Whatever the final investment figures become, Conch is under a 15-year contract to supply Britain with 100 million cubic feet of gas per day at a price of 88 cents per thousand cubic feet, which indicates a slow payout. The corresponding costs of manufactured gas in England from both oil and coal range from $1.07 to $1.75 per thousand cubic feet depending on the process used [27].

Future of LNG *Transportation*

The first commercial shipment of LNG from Algeria arrived in London on the *Methane Princess*, October 12, 1964. Since then, the Methane Progress and the Jules Verne have been brought into service. Much of the excitement regarding the potential markets for LNG in Western Europe was chilled to sub-zero proportions when gas was discovered on July 29, 1959 [34] by NAM (Nederlandse Aardolie Maatschapij—jointly owned by Royal Dutch/Shell and Standard Oil of New Jersey). The extent of the reserves was not publicized until around the middle of 1962, when it became rather evident that the field was the largest in Europe [35]. Up to this time, the Po Valley in northern Italy and Lacq in southeastern France provided the bulk of the natural-gas supply in Europe. By 1964, it became clear that the Dutch field was the third largest in the world, ranking behind only the Texas Panhandle and the Sahara field. Although the extent of the Dutch discovery was not known prior to 1963, Royal Dutch/Shell as a partner to this discovery surely must have been aware of the ultimate impact of Dutch gas on LNG when they joined forces with Constock in 1960.

It is certain that the Dutch gas will cut deeply into the formerly,

TABLE 10

REVISED ESTIMATE OF TOTAL INVESTMENT IN FIRST LNG VENTURE

		Millions of Dollars Invested
Gas pipeline (300 miles of 24 inch line from Sahara gas fields to Arzew)		\$ 35.0
Liquefaction plant, Arzew		85.0
Two tankers		25.0
Unloading and distribution in London		
Canvey Island facilities	= \$ 8.0	
Reforming plant	= 21.0	
Distribution line	= 21.0	50.0
Total		\$195.0[1]

[1]This total does not include the French tanker and the unloading and distribution facilities in France. However, the gas pipeline in Africa was constructed apart from this particular project. Its cost probably balances the French investment in a tanker and unloading terminal. Therefore, the total investment resulting directly from the gas liquefaction is in the neighborhood of \$200 million.

potential LNG markets in Holland, Belgium, Sweden, northern France, and Germany. The Dutch discovery has kicked-off an exploration panic across the North Sea and extending inland into northern England. Engineering feasibility studies on laying a gas pipeline across the English Channel are already in the advanced planning stages.

France is attacking the natural gas supply problem on all fronts. The Jules Verne had not even made its first shipment when France announced plans to build another tanker 4 times as large [31]. For several years, they had studied and experimented on a pipeline under the Mediterranean to bring Algerian gas to Spain and on into France [36, 37] but recently admitted that the project was shelved due to Franco-Algerian political problems [31]. However, at the same time, Ben Bella revealed his plans for a Mediterranean gas pipeline to Europe with a second gas liquefaction plant in Eastern Algeria [38].

Even with the Dutch finally announcing that they were pricing their gas competitively with other fuels, as low as 35 cents per 1000 cubic feet while continuing with expansion of distribution lines, Jersey Standard, the other half of NAM (discoverers of, and partners in producing Holland gas) gave their answer to the North Sea successes by releasing plans for shipping Libyan gas to Italy and Spain. The liquefaction plant, which will be located on the Mediterranean Coast, 100 miles from the Essos Zelten area fields, will have a capacity of 300 million cubic feet of gas per day—twice the size of the Arzew plant. Tankers will start shuttling to Italy and Barcelona, with the former taking 80% of the cargo, by late 1967. Total investment will be around \$200 million [39].

While others are looking at Middle East and West Pakistan gas for transporting to Japan, Australia and South Africa, Polar LMG Corp., a subsidiary of Union Oil and Marathon Oil Co., is negotiating a contract with Tokyo Gas. Co. to deliver 35 million cubic feet of gas per day from either Alaska or British Columbia [40, 41]. Originally, the timetable looked for 1965 as a shipping date, but it was delayed to 1967 after natural gas was discovered in Niigata, Japan [42].

Scandinavian oil industry leaders are talking about a joint venture to import Sahara gas, despite the proximity of the Holland and North Sea fields [42].

It is no secret that Conch, Union Oil, Ohio Oil, and others are looking at both the U.S. East and the West Coasts with an expectative eye. California's demand for gas is expected to reach 8 billion cubic feet of gas per day by 1982, at which time it is estimated that they will be producing less than 1 billion cubic feet [43, 44].

Australia, which is still considered a good market, is continuing to show substantial increases in their own gas fields [45].

Venezuela flares more gas by a factor of almost two than is currently consumed in all of Western Europe. They missed their chance of becoming the first source for LNG when they could not come to an agreement with Constock on gas price back in 1958 [44], but they very likely will enter the picture soon.

Looking to the year 2000, when the world's energy consumption will climb to five times the current rate [1] it is reasonable to conclude that transportation of LNG will continue to prosper. Even though natural gas continues to be discovered all over the world, there is an interesting, opposing force which will act to increase the cost burdens on distribution by pipeline. As population density increases, right-of-way cost for pipelines rises drastically. Holland, with a population density of 910 per square mile, coupled with the need for myriads of underground road and water crossings is already faced with pipeline construction costs that are two to three times higher than the average elsewhere [34]. Already, they are resigned to the fact that at least half of their production will have to be exported.

Competition from other fuels will always be a problem, but the petroleum and gas industries thus far have demonstrated a unique—so far as energy sources are concerned—adaptive, pricing-control characteristic which keeps its products moving on the open market. Continued research and development will lower the cost of producing LNG. Modest improvements can be expected in the liquefaction and storage aspects; however, the next big jump will occur when ship builders abandon the notion that an LNG carrier must look and act exactly like a conventional oil tanker and start building a carrier to conform to the needs of the cargo. Some trends have already been noted, ranging

from modest [46, 47, 48, 49, 50] to others more daring [51, 52, 53].

Concluding Note

In retrospect, if the writer has failed to convey the significance of Constock's successful pioneering effort, then let it be remembered as an outstanding, technological achievement. In this era of daily breakthroughs, however, it is appropriate to quote Dr. Harvey Brooks, Dean of Engineering and Applied Physics at Harvard University:

"Breakthroughs are mainly figments of publicity agents' imagination. They come slowly, a series of steps, advancing the art. There's more prior art than we're led to believe. It's a matter of sudden realization."

Acknowledgment

The author appreciates the permission granted by Conch International Methane Limited, Constock-Pritchard Corporation, and Continental Oil Company to use liquefied natural gas as the subject for the Sigma-Xi-RESA National Lectureship in April 1962.

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AMERICAN SCIENTIST, *53*, 1965

PERSPECTIVES

TWO ESSAY REVIEWS

I. Life in Space and Humanity on the Earth

By SU-SHU HUANG

The View from a Distant Star (Man's Future in the Universe) by H. SHAPLEY; 212 pages; $1.75 paper; A Delta Book of Dell Publishing Co., 1964. *Of Stars and Men* (The Human Response to an Expanding Universe) by H. SHAPLEY; Designed and illustrated by R. C. BARTLETT; 1958 edition revised; 134 pages; $12. boxed; Beacon Press, 1964. *Worlds Without End* by N. J. BERRILL; 240 pages; $5.95; The Macmillan Co., 1964. *Interstellar Communication*, edited by A. G. W. CAMERON; 320 pages; $8.50; W. A. Benjamin, Inc., 1963. *We Are Not Alone* (The Search for Intelligent Life on Other Worlds) by W. SULLIVAN; 325 pages; $6.95; McGraw-Hill Book Co., 1964.

The Astronomer's Contribution to Humanity

BUDDHA was enlightened by revelation under the Bohdi tree. He gained all the wisdom and truth about life through meditation in seven times seven days. The scientist's effort in trying to understand life has been by collecting facts. It is a tedious and painstaking process which may take millenniums. But, in the end, it appears that among their divergent ways of teaching and wisdom, the two cover some common ground and that is the virtue of humility.

When Charles Darwin proposed his theory of evolution that implied the humble origin of the human race, it was a severe blow to the human ego. Even after the lapse of a hundred years, there are still many who find it difficult to accept. However, Darwin's theory and its consequent developments are not the topic of the present review as their impact and significance have been fully discussed. What I would like to call attention to concerns another development in science which equally dampens the human ego. The topic is the problem of extra-terrestrial life.

The problem of extra-terrestrial life is closely related to how man looks at his own position in the universe. Thus, before the heliocentric theory of Copernicus, man regarded the earth as the center of the world and himself as the pinnacle of creation. This anthropocentric conception was prevalent in many philosophies and religions. Then Copernicus degraded the earth into a planet and implicitly raised the question of life in other planets. When it was further recognized that the sun is nothing more than just another star, the question was asked, as did the Dutch physicist Christian Huygens, "why may not every one of these stars or suns have as great a retinue as our sun, of planets, with their moons, to wait upon them?" It takes only a little more imagination to suggest

that the planets "must have their plants and animals, nay and their rational creatures too, and those as great admirers, and as diligent observers of heavens as ourselves. . . ." Huygens' view represented an early anticipation of the now accepted view, but it had to wait three hundred years before astronomers could support it with observations. In this respect, the situation is not unlike what has been encountered in the development of the theory of biological evolution. Long before Darwin, evolution had often been discussed, but it took Darwin many years of field work in collecting facts before this concept was developed into a scientific theory.

How have the astronomers' observations contributed to the understanding of life in the universe? We may mention two aspects: First, he has put the problem in the perspective of vast space and time. After Copernicus, it was discovered that the sun with its planetary system is only a tiny speck in the huge stellar system that is the Milky Way. Furthermore, the Milky Way system again is found to be only one of numerous galaxies that populate the universe. The successful entanglement by observations of the hierarchic structure of celestial systems culminated in Hubble's expanding universe. In doing so, the astronomer puts the earth in an inconspicuous position. Or, more generally, we may say that there is no privileged position in space with respect to the distribution of matter in the universe.

The second part of his contribution has been to verify that physical laws that were discovered on the earth or in the solar system can be applied to the remote regions of the universe. The first success in this direction was, of course, the application of Newton's law of gravitation to binary stars, making the law truly universal. But the most significant finding with respect to the problem of life elsewhere has been the discovery of the uniformity of matter. Celestial bodies are found to be composed of the same kind of chemical elements as those present on the earth. The atoms and molecules in stars emit or absorb radiation in the same spectral lines and bands as we find them in the laboratory on the earth. In short, the law of physics, formulated from our experience on the earth, does not depend upon the position in space. There is no privileged position in space with respect to physical laws. Consequently, if life has appeared on the earth and moreover has developed a high intelligence, there is no reason why similar phenomena cannot happen elsewhere. Under such circumstances one can only conclude by borrowing from Legge's translation [1] of Chuangtse: "when life comes, it is because it is time for it to do so. When life goes, this is the natural sequence of events." This concept of free but universal occurrence of life, I believe, is what the astronomer has truly contributed to the understanding of the meaning of life. Its socio-philosophical implication is evident since it teaches the basic principle that nature is impartial and discriminates in favor of no one.

At this point I cannot help but point out a pure coincidence in the world affairs (on the earth) and scientific thinking. If we look back on the history of the past twenty years and compare it with that before the war, there is a definite improvement in the mutual understanding between men. We have not been enlightened overnight, but we are going in this direction. Interestingly, this transition coincides roughly with the period when astronomers are abandoning the collision theory for the formation of the solar system and reviving the nebula theory which predicts the formation of the planetary system that is necessary for life emergence as a universal phenomenon.

H. Shapley, by observing the distribution of globular clusters, first discovered, in 1918, that the solar system should be located near the rim, instead of at the center of the Milky Way system. This important discovery in astronomy, I believe, must have had a tremendous feedback effect on his thinking, because he has become a passionate crusader against the narrow "anthropocentric religions and philosophies." However, in his recent book, "The View from a Distant Star," he did not make a systematic exploration of this theme. Rather, the book is a collection of his reminiscences and outlook about galaxies, stars, educational systems, international cooperation and other subjects in which he has found an interest. Indeed, many of the chapters in the book are based on lectures and on articles that the author has written from time to time. This explains the wealth of subjects that have beeen discussed in this relatively small volume. In each subject, the author has some interesting points of view. He tells them with wit and originality of style very much like a wise grandfather would to his grandchildren on long winter nights, in order to instruct his youngsters as well as to delight them.

As an outstanding scientist with a keen interest in social and international problems he has much to say on many topics in, as well as under, the sky. In general, he emphasizes the overall similarity between physical phenomena and human events. For example, in the chapter on "Stars, Ethics, and Co-existence" he first describes, literally, "the common breath of humanity." Since there are a great many molecules in one deep breath, these molecules will in the course of time spread by wind over the land and sea and enter the breath of every man, woman and child in the entire world. He then goes on to talk about physical events such as the propagation of waves that are necessarily international and, finally, leads to a discussion of coexistence between this country and USSR. His wisdom and his love for mankind can be seen everywhere in the book. It will certainly nourish the mind of young intellectuals whom, I believe, the author has set himself to address, and provide them food for thought.

The title of the book, "The View from a Distant Star" is a happy one

because it is otherwise difficult to describe this all-inclusive and broad-scoped booklet that is a collection of reflections and wisdom of an eminent scientist of this century. The subtitle, "Men's Future in the Universe," is, however, slightly misleading as only a small part of the book deals with this problem.

Shapley's other book, "Of Stars and Men," published for the first time in 1958, and revised in 1964, is a more systematic treatment of the author's stand than "The View from the Distant Star." It starts with a general introduction of cosmography—a term used by the author to describe "the field of study that has the same relation to the cosmos that geography has to the earth"—which provides a good introduction for acquainting the reader with the universe we live in. It goes on to describe cosmic chemistry and the possibility that life may appear elsewhere because of the large number of stars that are present in the universe. Finally, the origin of life on the earth, the sense receptors in the animal world and the grim future hazards of the human race are discussed to conclude the book. The central theme of the book concerns the position of mankind with respect to nature.

The troubles we encounter in our world may have too many causes to be enumerated, but not the least are human conceit and bias. Shapley's book is a good antidote for this human weakness. For this reason, the book should be recommended and on the list of required reading not only in science, but also in sociology courses in every high school in the world. For the record, Shapley has not discussed sociological problems in this book. But any one who has read it will be taught a good lesson of open-mindedness, and an open-mindedness in dealing with our fellow men perhaps may ease many thorny problems that now face mankind.

From its attractive format, the revised edition of this book may indeed be issued with the purpose of presenting it to student readers. It is now beautifully illustrated and clearly printed on paper of good quality—a far cry from its unpretentious first edition. On the other hand, there are no major changes in the text from the first edition, apart from an addition of a short index and a splitting of one chapter into two.

Emergence of Intelligence—A Biological Problem

If we take a look at living beings around us, we must be impressed by their great varieties. Actually, they represent only a fraction of what has existed on the earth. If we further remember that all the living things that have ever appeared on the surface of the earth might have descended from the same unicellular organism that emerged by chance in the early phase of the earth's existence, we cannot avoid the conclusion that, even if the basic chemistry of life elsewhere is due to the same carbon reactivity that is well known to us, the actual forms of life in other worlds and their evolutionary sequences must have an extremely wide range of

possibilities. Thus, the appearance and evolution of living organisms should behave like the chess game or more properly the Chinese game of "GO" [2] (Wei-ch'i in Chinese) which is now more popular in Japan than in China.

In this game of GO, two players start with an empty board on which there are 19 × 19 = 361 vacant spots at the intersections of two sets of mutually perpendicular lines and take turns in placing their stones, one at a time, in any of the vacant spots on the board. From time to time one or more stones can be captured by the opponent and removed from the board. The spots where stones have been removed become once again vacant and ready for further placement by both sides. Hence, without understanding the rules of the game, one can immediately realize that the chance of playing two games in an identical way is vanishingly small.

The living organisms of course, face more varieties of choices in each step in their evolutionary sequence than the GO players do. Perhaps it is due to this reasoning that Shapley is not an enthusiast for interstellar communication in spite of his keen interest in the general problem of life in the universe. He said, in *The View from a Distant Star*, "Perhaps we should first attempt reciprocated communication with non-human organisms here on the earth—say with a vegetable or a scarab beetle, or a termite queen-mother who represents the highest natural societal organization known on this planet. Foolish suggestions, yes, but they suggest the difficulty and probable impossibility of interplanetary communication."

Shapley's idea is shared by others like G. G. Simpson (Paleontologist) and H. F. Blum (Biologist). They argue that the chance of taking the direction towards intelligence in biological evolution is small and question the wisdom of their fellow scientists for talking about interstellar communication or even exobiology.

On the other hand, we may also argue that although there are a large number of ways that biological evolution may actually proceed, a large fraction of them may tend toward high intelligence. For, after all, evolution is not a blind process; it must be guided by natural selection. Just like the game of GO, the better player always ends up by winning the game; so in the game of biological evolution. Living organisms that can adapt to their natural environment always have a high chance of emergence and survival.

In a recent article in this journal, R. Bieri [3], a biologist, gives a convincing argument in favor of the inevitable emergence of intelligence in the course of biological evolution. He concluded: "Given the ninety-two known, naturally occurring elements, the forms of energy available, and limited time, the number of alternative solutions to the major steps leading to a conceptual organism are strictly limited. The phe-

nomenon of convergent evolution is so widespread in both the plant and animal kingdoms that it needs no special elucidation here. Suffice it to say that the evidence shows that, again and again, animals and plants have independently evolved not only similar structures but also similar biochemical systems and similar behavioral patterns as solutions to the same fundamental problems."

In presenting these two opposing views, I do not pretend to know which one has a stronger argument than the other. But it clearly shows that the basic uncertainty concerning life in the other worlds lies in the domain of biological science. For this reason, I had great expectations for the book, "Worlds Without End" subtitled "A Reflection on Planets, Life and Time" by N. J. Berrill because the author is a zoologist. However, after I have read through the book, I find myself in a most disappointed mood. Actually, one does not have to go very far in the book before he realizes that the author not only has nothing new to say on this subject but also is ill-prepared for what is already known. In the first two chapters of 31 pages, the author quoted long passages of science fiction in five places with a total length of eight pages and mentions Jules Verne four times. It gives the impression that only writers of science fiction know anything about Venus and the Moon, for these are two subjects discussed in the first two chapters. Not a single name among many modern astronomers who actually observe Venus and the Moon is mentioned, let alone quoted. The trend of quoting science fiction at length is not limited to the first two chapters but is a conspicuous feature of the entire book.

Although the book covers astronomical problems in about two-thirds of the volume, it is reasonable to suspect that the author has received little formal training in astronomy. This suspicion may be seen from the dubious and ambiguous statements the author made in the book. For example, we find on p. 70, "According to Kepler's rule of location of planets, a planet should exist between Mars and Jupiter, . . ." Again on p. 182, "After all, Kepler found the rule for spacing and left room for the undiscovered asteroids in an empty orbit between Jupiter and Mars." It is evident that the author is confused between Kepler's law of motion of planets and Bode's law of planet spacings—an obvious mistake that no one who has ever taken a course on elementary astronomy would easily make.

Interstellar Communication

The debate on the emergence of intelligence from the biological point of view has been overshadowed by the more exciting and fascinating discussions of interstellar communication, and even of interstellar travel, from the technological point of view. "Interstellar Communication" edited by A. G. W. Cameron and "We Are Not Alone" written by

W. Sullivan are two representatives of this tendency. They describe the outlook of life in other worlds and the consequent possibilities as seen, mainly, by physical scientists and engineers. The fact that both books treat the problem in this way is not because the books are intentionally biased in favor of the technological point of view but because little has been written from the biological point of view. From what we have already said in the previous section, the relative reluctance of biological scientists in dealing with the problem at hand is understandable. However, this leaves the discussion of this important problem incomplete. The situation may best be illustrated by the construction of a building. Astronomers, by their finding of the universality of matter and physical laws in the universe, have built the foundation which made a scientific discussion of life in the universe possible. Now physical scientists in other fields have found means for interstellar communication. In our example, it means that a magnificent roof of the building has been built. But between the foundation and the roof little has been constructed, leaving the roof somewhat dangling in the air without support. To fill up the gap, discussions by biologists, such as the one by Bieri [3] quoted before, are urgently needed.

Sullivan, science editor for *The New York Times*, keeps up the tradition of this newspaper by reporting every idea and every speculation advanced in the past several years that he has either found in the literature or heard in meetings or interviews. In many cases, he traces the history of the topic to the very beginning and provides the reader with a necessary background in every instance whether in astronomy, in biology, or in whatever field he is treating. It is apparent that, before he wrote this book, he had done a thorough research himself. Indeed, his thoroughness in reporting the life problem outside the earth can be attested to by the fact that, for the last several pages of the book, he has summarized some discussions, by both clergy and laity, on the spiritual significance of life in outer space.

The book is easy to read, and both scientists and laymen will find it interesting. If any one has a desire to learn something about the present state of our knowledge about extra-terrestrial life, this is the book to begin with. If he becomes fascinated with any particular problem, the author provides, at the end, an extensive bibliography for his further study.

What I myself find most interesting is the chapter on "Wax and Wigglers" which treats the question of whether the carbonaceous materials of some kind of meteorites (Orgueil) have their origin in extra-terrestrial life. Sullivan describes vividly the arguments—scientific as well as emotional—used by both sides in this great debate and finally concludes by observing "The Orgueil debate is a classic example of a scientific discussion become personal, emotional and enmeshed with professional

pride. The talents and ingenuity of participants have been directed toward proving their case, rather than seeking out the truth. They have thus demonstrated that they are human, but the wonderful self-discipline and objectivity that we call pure science has suffered." Actually, in the entire book this may perhaps be the only place that the author injects his personal opinion. Elsewhere he has kept his function purely as an objective reporter—a not surprising fact if we remember his background as science editor with *The New York Times.*

"Interstellar Communication" is a scientific anthology that includes 22 reprints of papers published originally in the period 1959–1962 and 10 articles written specially for this book. Some of the latter are written by the editor himself and provide a general astronomical background of the subject. This collection reflects the state of mind of some scientists (mostly physical scientists) as regards the emergence of life in the universe and its technological implications, such as interstellar communication and travel, at the dawn of the space age. Therefore, the collection serves three purposes, for popular reading, for inspiring further studies and as a record of the time. The value of popularization of this collection is somewhat reduced, a little more than one year after its publication, by the appearance of Sullivan's book mentioned previously because the latter discusses, among other things, the essential points contained in nearly all the articles in Cameron's collection. Of course, papers in Cameron's collection are most original and go into more technical details than can be obtained from the book by Sullivan whose purpose is solely for popular information. Hence, for references of scientists themselves and a record of their prophets in the early stage of a new branch of science, Cameron's collection cannot be surpassed.

Ours is the age of technology. No one can deny its powerfulness and influence if he just takes a look at the progress it has made in the past generation. The atoms have been tamed so that they can be split or fused according to our wish. The gravity of the earth has been overcome so that we are now ready to go to the moon and eventually to other planets. Now we begin to see even the means for communicating with technological societies in the other parts of this vast galaxy. Ironically, however, we have not yet discovered a sure way to communicate with each other with understanding between beings on this little earth.

Super-civilization and the Problem of Man's Future

A discussion of interstellar communication necessarily leads to the problem of "other" civilization or civilizations we can communicate with. Since we are only on the threshold of such a venture, it immediately follows that a civilization that can be contacted must be more advanced than ours. Consequently, a great deal has been said of the so-called "super-civilization." Three scientists boldly and drastically have sug-

gested three possibilities that beings in these supercivilized societies might do with their super-technology.

In 1960, F. J. Dyson (theoretical physicist) of the Institute for Advanced Study, Princeton, N.J., argued, in the words of Sullivan, that "the population growth in such worlds would have continued to press the limits of available sustenance, as set forth in the theory of Thomas Robert Malthus at the end of the eighteenth century. The limiting factors in such a situation would be the available material and the available energy. Both of these shortages could be met by dismembering one or more planets of that solar system and using the material to build a shell completely enclosing the parent star. The entire energy radiated by the star would then become available—40,000 billion times that which falls on the earth."

R. N. Bracewell (radio astronomer) of Stanford University has another idea for what super-civilizations would do. He argued that, instead of scanning the candidate stars with the radio telescope for the telltale voice of other communities in the galaxy, a super-civilization would send automated messengers to orbit each candidate star in order to find out whether a technological civilization has reached maturity there. According to him, such a messenger would be powered by light from the candidate star and therefore resemble some of our own space probes except in different scales in time and space. He further points out that such a messenger "may be here now, in our solar system, trying to make its presence known to us" and mentions the strange radio echoes which he thinks have never been satisfactorily explained.

Finally, C. Sagan (astrophysicist) of Harvard University has an even more ambitious scheme for super-civilizations. He sees travels in interstellar space as a real possibility, perhaps even a necessity. For he said: "It reopens the arena of action for civilizations where local exploration has been completed; it provides access beyond the planetary frontiers, where the opportunities are limitless." Consequently "a central galactic information repository" may be established in order to assemble the knowledge of galactic communities.

I cite these three examples all of which are included in Sullivan's book (two of the original papers are included in Cameron's anthology, while Sagan's work came out after its publication) not only because they are extraordinary but also because they induce me to ask the question: will our own technological civilization reach this super-state?

The three scientists just mentioned did not touch on these questions. However, by assuming the appearance of super-civilizations in the galaxy, they tacitly imply a great future for the technological civilizations of our own. Such an inference, however, is at variance with the conclusion derived some years ago by two other scientists. Each of them wrote a book that discusses the future of man and his civilization. I

refer to "The Next Million Years" by Charles G. Darwin [4], grandson of the author of the "Origin of Species" and a well-known physicist himself, and "The Challenge of Man's Future" by H. Brown (geophysicist) of California Institute of Technology [5]. These two authors give us a bleak prediction for our future.

Darwin argues that any nation faces a dilemma in adopting a policy for its own population. In the first place, if one nation limits its population, it faces the danger of being crowded out by other nations that do not follow such a policy. He cites also the other damaging consequences of such a policy. On the other hand, without suitable restraint on the population in the world, the future of man can only be involved in a bitter struggle for food and survival. Therefore, Darwin concludes that the civilization of man on the earth is at its peak now and we can look for nothing in the future but its decline.

While Darwin discusses in his book the long range future of man from certain general principles that he assumes, Brown's book, which Einstein called an objective book of high value, concerns our immediate future by presenting first the statistical data about the world population, resources, and the rate of consumption of these resources. From these data and the present world situations with respect also to the population, resource, and industrialization, Brown sees three possible patterns of life "The first and by far the most likely pattern is a reversion to agrarian existence . . . as the second most likely possibility, (it is) the completely controlled, collectivized industrial society. The third possibility confronting mankind is that of the world-wide free industrial society in which human beings can live in reasonable harmony with their environment." Brown does not think the third possibility can ever exist for long but still gives a high hope that it will if we prepare ourselves in time. In any case, both Darwin and Brown do not have such high expectations for the advanced technology that are implicitly contained in suggestions by Bracewell, Dyson and Sagan.

If we now examine the background of these five scientists, we find that they are all outstanding in their respective fields of physical science. But the future of the technological civilizations they foresaw are completely different. Such diagonally different expectations of our future reflect the difficulties and uncertainties that are involved in the human relationship that defies a rigorous analysis such as we find in physical science.

My point in presenting this case is not to express my own view on this problem. Honestly, I am greatly impressed by the arguments of Brown and Darwin on the one hand and still maintain a great expectation for our own technological civilization as envisaged by Bracewell, *et al.*, on the other. The point I want to make is more earthly and of a much shorter time scale than they discuss. It concerns the state of mind of

man, in a time scale in terms of a generation instead of a millennium.

Physical scientists and the great majority of the general public alike are now elated by their past successes and become exceedingly confident in their ability for molding a glorious future for mankind. Progress appears to them to be without limit and the great expectations reflect this high mood of euphoria. This mood bears some resemblance to the way a child entertains himself in the evening before he is taken to a carnival that he has never attended before. For this reason, we may take these great expectations of technological possibilities as romantic lyrics that pay homage to the mighty of technology as well as express the joy of us scientists in this generation that follows the opening of the space age. Since it is this state of mind that actually helps us make progress, as recently amplified by Schwarzschild [6] in his statement before the Committee on Aeronautical and Space Sciences of the United States Senate, we should cherish it.

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AMERICAN SCIENTIST, *53*, 1965

II, The Problem of Human Thought

By S. GLUCKSBERG

Thinking: From Association to Gestalt by J. M. MANDLER & G. MANDLER; 300 pages; \$4.95 cloth; \$2.95 paper; John Wiley & Sons, 1964. *The Act of Creation* by A. KOESTLER; 751 pages; \$8.95; The Macmillan Co., 1964. *Structure & Direction in Thinking* by D. E. BERLYNE; 378 pages; \$8.95; John Wiley & Sons, 1965.

THESE THREE books provide an intriguing set of contrasts to the reader interested in the problem of human thought. The Mandlers' volume is a carefully selected collection of writings on thinking, spanning the history of the problem from Aristotle to Duncker's 1935 classic monograph on problem-solving. Included are several heretofore untranslated works, notably the selections from the works of Otto Selz, G. E. Müller, and Oswald Külpe. The editors offer a lucid introduction explaining their choice of material. They chose to follow a particular line of development in the history of the problem of thinking, a line that traces the rise of British associationism through the Wurzburg psychologists, culminating in the Gestalt tradition in America. While this decision precludes a complete history of the study of thinking, it does make possible the presentation of a unified, systematic series of vignettes, connected by perceptive commentary by the editors. This is not simply a grabbag collection of celebrated essays and articles, but a thoughtfully selected and arranged sequence of writings, set in context by the editors' running commentary. Anyone interested in a brief and delightful introduction to the history of the problem of thinking will profit from this book.

The Mandlers explicitly ignore the development of behaviorism from Pavlov to the present on the grounds that any consideration of this development should be a part of a study of contemporary work in thinking. Koestler, in his monumental volume, *The Act of Creation*, disagrees. Behaviorism, Koestler contends, is dead and buried *circa* 1930, and deserves to be resurrected only as a convenient whipping boy. Koestler's work is at the same time fascinating and, if taken as a serious scientific contribution, infuriating. Perhaps the most cogent description of his book might be impressed upon the cover in the form of a heraldic device: brilliant analogy rampant on a field of ingenious speculation.

The first half of Koestler's work is intended for the interested, intelligent layman, and considers all possible acts of creation, from humor through art to science. The second half, intended as a serious technical contribution, reaches the same conclusion as the first half after consideration of a wealth of material from biology, philosophy, physics, chemistry and, in general, the history of science. This conclusion is that the creative

act, from banal pun to sublime discovery, occurs unconsciously via the bisociation of two matrices. Bisociation differs from association by virtue of the prior separation of the two elements involved in terms of matrix membership. If idea A and idea B occur together for the first time, then the coincidence is judged as creative or noncreative on the basis of whether or not ideas A and B were, prior to their coincidence, members of the same matrix. If idea A and idea B were members of the same matrix, then the act is not creative. If they were each members of separate matrices, then the act was creative.

This formulation is, of course, an elegant and literate restatement of a time-honored notion, occurring throughout the writings of the British associationists. It occurs as well in the work of contemporary American and Russian writers, including those who are working within the stimulus-response framework so heartily despised by Koestler. Unfortunately, Koestler suggests no techniques for classifying events, acts, processes, or anything else as matrices. Further, bisociation and association are distinguished only by fiat, and not by specification of discriminative characteristics other than that the former is creative, the latter not so. In short, this can in no sense be considered a serious scientific contribution. It is vastly entertaining, even brilliant, but Koestler is essentially anti-scientific. He displays extraordinary gullibility when reporting phenomena which fit his ideas (e.g., magpies are described as singing antiphonally), and an equally extraordinary capacity to ignore or distort vast amounts of material that are not congruent with his thinking (e.g., "...natural law (sic) is replaced by Pavlov's law..." [p. 565]). The book is recommended as an encyclopedic essay that can be profitably mined for nuggets of cocktail hour conversation and anecdotal gems suitable for embellishing undergraduate lectures.

Berlyne's book, *Structure and Direction in Thinking*, could well have been written as a direct rebuttal to Koestler's position. As opposed to Koestler's paleoassociationism (Koestler's term), Berlyne offers a neoassociationistic approach to directed thinking. Demonstrating considerable epistemological sophistication, Berlyne first presents an outline of an associationistic formulation which integrates current English-, Russian-, and French-language psychology.

Berlyne then deals with the problem of differentiating between (a) autistic, reverie-like thinking, and (b) directed thinking, by postulating two kinds of thoughts. The first, situational thoughts, represent the external world or specific states of the external world. The second, transformational thoughts, constitute the mental operations which change the original situational thought into a different one. For example, think of the number three. If we are instructed to find the fourth power of that number in our heads, we might first think of $3^2 = 9$. The operation of squaring, or performing the operation 3×3, constitutes a transforma-

tional thought which changes the original situational thought, 3, to the new situational thought, 9. Continuing until we reach 3,[4] we have a train of directed thought consisting of alternating situational and transformational thoughts. Free association, lacking transformational thoughts, constitutes undirected or autistic thinking.

This formulation permits Berlyne, in the second third of the book, to translate much of Piaget's formulations of the development of thinking into a mathematical formalization employing concepts and principles deriving from the vast body of Russian and American literature on the formation and strengthening of associations. For example, transformational thoughts, or responses, are shown to be capable of structural arrangements in compound habit-family hierarchies. Habit-families, in turn, are further shown to correspond, in special cases, to characteristics of groups (in the mathematical sense). Those formal properties of groups which are required to account for such behavioral phenomena as quantitative-invariance judgments are then specified.

In its present stage, the translation of Piaget into associationistic terms is only programmatic, as was Dollard and Miller's effort to reproduce psychoanalytic theory in terms of behavior theory. Berlyne is well aware of the preliminary aspects of this work: "It is evident that a great deal more spade work will have to be done before rigorous and fruitful hypotheses about transformational thoughts can be laid down and predictions deduced from them can be tested" (p. 123). Bringing Piaget's ideas to English-speaking psychologists in terms of familiar concepts would be contribution enough. Berlyne's own formalization promises to be fruitful in its own right.

The final third of the book concerns motivational aspects of directed thinking, and includes a consideration of the relations between motivation and the formal mechanisms proposed earlier.

In summary, each of the three books is highly recommended. The Mandlers' collection provides an excellent historical review of one line of development in the history of the study of thought. Berlyne presents a tightly organized formulation of directed thinking, with a broad review of motivational factors involved in thinking. Both are to be regarded as serious contributions to our understanding of the thought process. Koestler, whose treatment is certainly the broadest of the three books considered, is fascinating, sparkling, and unconvincing. Nevertheless, it is an intellectual tour de force well worth dipping into.

Photograph courtesy of Mr. Bern Dibner, The Burndy Library, Norwalk, Conn.

Salute to

GREGOR JOHANN MENDEL

who in February and March 1865 communicated to the Society of Naturalists in the town of Brno (Brünn), Czechoslovakia, the results of his experiments on hybridization of varieties of common garden peas, grown in his monastery garden.

Kodak advertises:

high-priced drudgery diminished... a new plate that will go far

Thin-layer chromatography caught on about four years ago. Now anybody who claims knowledge of how to identify or synthesize substances and finds himself vague about TLC should worry a little. He has been washed up from the mainstream and had better take measures. He will not read far or listen long before the thought strikes that he should learn the technique for coating slurries of adsorbents like silica gel on glass plates. At chemical and biological labs the world around, a goodly chunk of the working time is now devoted to this art. Many fine tricks influence the homogeneity and isotropy of the coating and the level of activation imparted to it. No sooner having learned of them, he can now forget them.

He is just as well off as the eager beavers who couldn't wait until the messiness was eliminated by us, who got our start 85 years ago in relieving photographers of the need to coat their own plates.

Now we have a mighty force of chemists and respected technicians of our own. During recent months doubts have been deftly planted in their minds about the wisdom of drawing pay for such essentially mindless tasks as coating their own chromatoplates, except where some special technique still demands glass or an adsorbent other than silica gel bound with polyvinyl alcohol. They have been persuaded to weigh the importance of these special techniques against costs of preparing glass chromatoplates, of documenting the results shown by the chromatogram, of storing the bulky things for reference, of recovering the expensive edged glass for reuse.

For general work they have begun to standardize on a poly(ethylene terephthalate) sheet on which 100μ of fluorescent (or non-fluorescent), PVA-bound silica gel of our own preparation and control has been coated, not manually but by a manufacturing organization that owes its robust health largely to its precision in depositing thin layers of one thing on another.

As EASTMAN CHROMAGRAM *Sheet, this new polyester TLC medium—scissors-prone, conveniently flexible but not limp, unbreakable, sending up no clouds of siliceous dust to breathe—can now be obtained from a nearby lab supply house. If the price they quote for a box of twenty 20cm x 20cm sheets does not bring pangs of guilt about continuing to coat by hand, your problem is obviously one of excess staffing. If you hesitate only because you want to try it first, request a sample of* EASTMAN CHROMAGRAM *Sheet from Distillation Products Industries, Rochester, N. Y. 14603 (Division of Eastman Kodak Company).*

The trouble with astronomers

Astronomy has probably done more for us than we have done for astronomy.

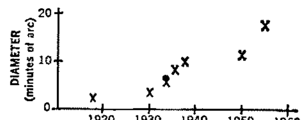
Some of the deepest chemical and physical underpinnings that support us as we cheerfully manufacture $n $\times 10^8$ worth of photographic materials this year were driven down to bedrock because astronomers were analytical-minded users of photographic materials, though unable to match boardwalk tintype artists for volume of consumption.

The trouble with astronomers is still their paucity. Yet here we go, proudly announcing the KODAK Special Plate, Type 080-01, that halves exposure time for the faintest of stars, nebulae, and their spectra, or alternatively, establishes new extremes in faintness. Since, in the class of observations where it is not extraneous light that imposes the limit, penetration is bought with exposure time and focal length, and since the world supply of telescope time on serious focal lengths defines a rather cozy little customer-community of astronomical frontiersmen, questions of commercial sanity might well be raised.

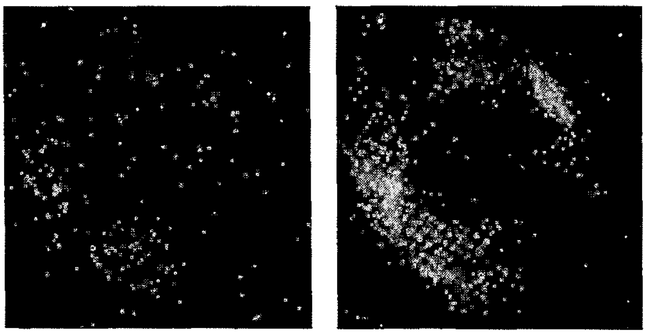
No, the explanation is not so simple as that this emulsion research will soon pay off in shooting cowboys on dimly lit saloon sets for TV. A deeper understanding now reveals weakness in conclusions drawn from studies of fast films designed for other purposes than to distinguish threshold light signals from darkness. That the earthlier applications of photography use fractional-second exposures instead of hours is only part of the difference.

With rival claims now reassessed, photographic detection has been restored to its former eminence in the astronomical observatory. How can we now possibly stand it to refrain from jumping in? To whom shall the astronomers turn if not to us?

Not many men pull on their shoes in the morning with the thought, "Today I'll ship something that will probably expand the visible universe." It's fun to make a galaxy like Messier 32 grow:



This business, yielding a revenue inverse to its cosmic scope, is conducted by Special Products Sales Division, Eastman Kodak Company, Rochester, N. Y. 14650.



Kitt Peak National Observatory recorded the very large and faint planetary nebula NGC 7293 in Hα light with (left) the former best choice, KODAK Spectroscopic Plate, Type 103aE, and (right) the new KODAK Special Plate, Type 081-01, an "E"-sensitized version of our new breed.

THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

Internal Factors in Evolution by L. L. WHYTE; 128 pages; $4.00; George Braziller, Inc., 1965.

This short book is remarkable in many ways. Lancelot Law Whyte is not a biologist yet he has, in this crystal clear essay, put his finger on a problem that is central to biology. In some ways, the very clarity of the writing will be a disadvantage to him for it is easier to attack a man if one understands exactly what he says. However, I do not think one can disagree with his main theme; the only differences that might arise are on the use of words and the originality of the basic idea. These are relatively trivial points and they should not keep the reader from the opportunity to do some new and careful thinking about rather basic biological problems.

The idea (which unfortunately Mr. Whyte refers to as a "surprise") is simply that there are two kinds of selection; an external Darwinian one and an internal one which is independent of the adaptability of the organism to a particular environment. The internal selection takes place by the machinery of the organism passing upon whether or not a particular mutation can survive considering the nature of internal *milieu*.

This point leads him to stress the importance of the development of the individual in evolutionary process, for each variation or mutation must go through the two screening processes of the internal as well as the external one. He says, "Consider, for instance, the school of population statistics, which starts with the excellent aim of fully objective numerical observations, but sometimes slips into asserting that population figures provide the *only* basis for a theory of evolution. This is wrong. The statistical theory of populations must be complemented by a structural theory of individual ontogenesis and of its influence on phylogeny. This may reinstate a theory of the typology of phenotypes based on a typology of genetic systems and of proteins, etc."

For some time I have been interested in the notion that one of the reasons for the present day schism between molecular and evolutionary biology is that the relation between ontogenetic processes and evolution is unclear and has not been well formulated, a conclusion that certainly fits in with these views of Mr. Whyte. The only place, on this issue, where we part company is his insistence that development is a period of preparation for a stable adult. I would prefer to think of organisms as life cycles, not adults, and then the understanding of the relation of internal and the external factors will be easier and more meaningful.

For some years I have had a problem with one of Mr. Whyte's basic notions concerning the internal structure which performs the selection. However, this is a word problem and not a serious one. The ideas of *form* and *organization* or the new term he uses in this book, *coordinative conditions*, always seem to me misleading. By the very elegance of the words, they seem to imply that more is known of how the inside of an organism is put together than is in fact the case. There would appear to be a human foible which consists of obtaining comfort from grand words; their very grandeur implies a greater understanding than is justified. *Entelechy* apparently gave Dreisch this comfort, and even though a favorite sport for the last fifty years has been castigating him for this concept, I often wonder whether, in less bold ways, we do not all seek the same kind of comfort in particular words. So many times I have heard level-headed experimental biologists say that the great unsolved

problem is that of biological organization, and I immediately worry whether or not by saying this we are subconsciously putting the problem on a level where it cannot be attacked by science or logic. Rather it seems to me that the man who firmly declares that there is no such thing as a problem of biological organization, and spends his time studying organisms with an open mind, will more likely make both scientific as well as philosophical advances.

To return to the concept of internal selection, the great difficulty, from the point of view of the professional biologist, is that he will be inclined to dismiss all of this with the remark that it is obvious. Mr. Whyte recognizes this difficulty, and furthermore discusses at some length the work of others who have examined the issue in the past. He does create the impression that everyone else has said something slightly different, but I fear the only reason that they have not said precisely the same thing is that it has been assumed and understood from the beginning. For instance, this is the whole basis of the lethal mutation which has been studied in such detail by E. Hadorn. Also, the whole question of the restriction of mutation, which raises doubts about the wisdom of the expression "non-random," has been discussed from many points of view. H. F. Blum has stressed the physico-chemical restrictions and C. H. Waddington has specifically stressed the internal developmental factors to an extent not obvious from the author's historical review of the subject.

The point was, in fact, first recognized when it was realized that natural selection involves reproductive success; not just over-population and the survival of the fittest. In discussing this Mr. Whyte says, "The crucial characteristic of the synthetic theory is *a net reproductive differential of genetic variants, regarded as due to differences of adaptive fitness which are thus causes of phylogenetic change*. This may be taken as a definition of Darwinian adaptive, or *external selection*."

But, by defining reproductive success in terms of adaptive fitness, he purposely eliminates the value of the notion of differential reproduction. He does this because he feels that, instead of taking cognizance of internal selection, the use of relative reproductive success hides the whole concept. Its original purpose, however, was not to obscure, but to recognize the undoubted significance of this inner selection. It is a factor which had to be included and it is as natural as any of the other forms of selection.

Again, however, we are fluttering around in word problems. The fact that the standard biologist might object to some of the author's new terms is of no consequence. There is an internal selection; it is an interesting fact that has significance both to the development of individuals as well as the process of evolution. Mr. Whyte has written about it in an unconventional, but exceedingly clear fashion. It will be worth our while to think about what he says, and suppress the desire to feel superior about the way he says it. Sometimes people who ignore the club rules have a way of prodding one into a bit of new thinking, and in this Mr. Whyte has succeeded very well.—*J. T. Bonner*

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Genetics & Man by C. D. Darlington; 382 pages; $7.50; The Macmillan Co., 1964

It is certainly true that mankind is a biological species, and just as certainly false that mankind is nothing but a biological species. The "nothing but" fallacy has, time and again, stultified otherwise legitimate attempts of some biologists to use their specialized competence for elucidation of human problems. Understandably, these attempts have evoked an equally fallacious reaction on the part of some social scientists, who wish to believe that man has escaped from the clutches of biology when he became human. As so often happens, the truth lies somewhere between the extremes; man is an animal, but a very extraordinary one. Biological evolution has transcended itself when it produced man. Mankind is involved not in one but in two evolutions, the biological and the cultural. These evolutions are not independent but interdependent, and connected by feedback

relationships.

C. D. Darlington, an eminent biologist and the professor of botany at Oxford University, is a highly articulate and sophisticated exponent of views rather close to the biological "nothing but" end of the spectrum. The book under review is an abridged edition of his "The Facts of Life," first published in 1953. Brief descriptions of some recent findings in general and in human genetics have been introduced (the genetic code, relations of lung cancer and smoking, chromosomal aberrations in man); the two concluding chapters and some comments elsewhere have been deleted; the body of the book, as well as its "spirit," are unchanged. The author is convinced that what a man is or could become is determined by his heredity, and that the great environmental plasticity of the human developmental pattern is an insidious illusion. The assertions that "The structure of a society rests on the stuff in the chromosomes and on the changes it undergoes," and that "The materials of heredity contained in the chromosomes are the solid stuff which ultimately determines the course of history," are not found in the present edition. However, Darlington maintains that the environments in which people live have been chosen by themselves, and that the choices are determined by the genes. Thus, "The crudely physical and physiological properties of the body combine with all its other genetically determined properties to decide the modes of life we choose. Our choice therefore, in the whole sum of our activities forestalls the reaction of heredity and environment. That is the genetic view." To some extent this is, of course, true; an animal can move from a less suitable to a more suitable environment, and what is or is not suitable may be genetically conditioned. But as an explanation of the human diversity this is completely unrealistic. We discover in our childhood, and confirm this discovery daily as long as we live, that some environments are inaccessible, despite being attractive. The first edition of this book, despite the brilliance of its style and the great erudition of its author, has made few converts among scientists; the new version is not likely to have any greater success.—*Theodosius Dobzhansky*

Of Men and Galaxies by F. HOYLE; 73 pages; $2.95; University of Washington Press, 1964.

The 1964 Danz lectures at the University of Washington, *Of Men and Galaxies* is written in the same appealing platform style which was one of the charms of his earlier collection of lectures, *The Nature of the Universe.* This time, however, Hoyle is discussing extraterrestrial life rather than cosmology. He takes up the now-current theme that extraterrestrial intelligent life no doubt exists, and examines the challenge of establishing communication with it. The result is a fascinating little volume.

There are three separate lectures, the first of which examines the relationship of the creative intellect (such as Hoyle's) to what has come to be known as "Big Science." The second considers the relationship of creative intellect to society as a whole, and to extraterrestrial civilization. Pursuing the thought expressed so dramatically in his science-fiction work, *The Black Cloud,* he proposes a massive effort to effect communication with life beyond the earth as soon as possible.

The third lecture argues that each extraterrestrial civilization, like our own, has only one chance to survive the revolutionary impact of technology. If self-destruction becomes rampant, the chance afforded to perfect civilization will have been lost forever, because energy supplies will have been exhausted in the earlier phases of civilization. Hoyle believes that the social and technical evolution of intelligent life is beyond our control. Instead, the composition of an infinite variety of infinitesimal influences predetermines the course of human affairs with great certainty. He even goes on to make the "religious" hypothesis that the emergence and development of intelligent life has not been a meaningless accident. However, he does not suggest a meaning. Rather, he urges humanity to crack the

extraterrestrial communications problem—to "get our name in the galactic directory" as he puts it. In this endeavor, Hoyle holds out the hope for an extraordinary breakthrough in human development. At last, we would be in contact with civilizations which, by definition, would have succeeded in advancing to a high level without destroying themselves.

Hoyle's views on human affairs are every bit as challenging as those on cosmology. Even if the steady-state theory expounded in the earlier lectures has not proven completely successful, it has profoundly influenced developments in the field. The same will be true of the lectures in this little book.—*George B. Field*

The Universe & Its Origin, edited by H. MESSEL & S. T. BUTLER; 147 pages; $3.75; St. Martin's Press, 1964; *The Universe of Time & Space*, edited by S. T. BUTLER & H. MESSEL; 291 pages; $2.95 paper; The Macmillan Co., Pergamon, 1963; *Space Physics & Radio Astronomy*, edited by H. MESSEL & S. T. BUTLER; 174 pages; $4.25; St. Martin's Press, 1964.

The Universe and Its Origin is a very readable book covering the evolution of the solar system, stars, and galaxies. It is written at the general level and will therefore be of interest to high school and college students, teachers of general science, and the amateur or armchair astronomer.

The authors are well chosen for the particular fields that they cover. In particular, Prof. Bart J. Bok in chapters 6, 7, and 8, gives an excellent account of the observational evidence for the evolution of stars individually and in clusters, with examples chosen from the northern and southern hemisphere; Prof. Thomas Gold in Chapters 9–13 describes with admirable lack of bias the several theories for the origin of the planets and satellites.

To complete the all-embracing subject matter of the title, G. Gamow gives an account of various cosmological theories, the evolutionary, steady state, and static models. C. B. A. McCusker describes the origin of cosmic radiation and its relation to the sun, novae, and the universe as a whole. Quasars, the enigmatic high energy radio sources, were discovered too late for inclusion in this book.

The Universe of Time and Space covers mainly topics in physics with a section on astronomy. The standard is somewhat higher than *The Universe and Its Origin* and I would place it at the sophomore physics level. Relativity is covered in considerable mathematical detail, though the discussion is held at the non-calculus level. Twelve pages are devoted to a table of the elements and isotopes within a chapter on atoms and nuclei in the section written by the editors. There is a discussion of atomic models and electromagnetic radiation.

R. Hanbury Brown has a single chapter on the stellar interferometer at Narrabi, Australia, where he explains its purpose and mode of operation. At the time of writing, the interferometer had not been put into operation and so results have not been included in the chapter.

The astronomy section has the title "The moon, planets, and comets in the expanding universe." The author, Prof. Lyttleton of Cambridge University, is noted for his research on comets, and the account of the solar system is good, though the level is more general than the physics contents of the book. He continues in the popular usage of "proving" that the universe is expanding by means of the overworked Olbers paradox—"the whole sky should appear brilliantly luminous" but it does not because "owing to relative motion the brightness of a galaxy falls off more quickly than the simple distance effect would imply." Actually, the redshift accounts for the darkness of the night sky whether it is caused by a doppler shift or whether it is caused by a gravitational or some other effect in a static universe.

Prof. T. Gold completes this volume with "The first five years of space research," a clear, stimulating summary, mainly of the U. S. Program. He writes: "I will not say just how many millions of dollars I would think justifiable to

spend for this glimpse of knowledge. It seems to me that it is worth a major effort from all of humanity. The techniques of space research have come to stay and are already contributing greatly to the knowledge of our surroundings. Those who, five years ago, when the first sputnik was launched, doubted the future of space research, have a place reserved for themselves in the history books together with the gentlemen who were sure that no passenger could survive travelling in a railway train at more than five miles per hour."

Space Physics and Radio Astronomy is the third book which shows the more unfortunate aspects of the series—the fact that they are based on the collected lectures of summer schools for high school students and teachers. It is difficult to justify the inclusion of mechanical flight through the Earth's atmosphere, winds in the upper atmosphere, and a discussion of optical stellar interferometers under the book's title. Yet these topics are well-written and undoubtedly formed a successful part in the summer school lecture series.—*Gerald S. Hawkins*

David Rittenhouse by B. HINDLE; 394 pages; $8.50; Princeton University Press, 1964.

David Rittenhouse (1732–1796) of Pennsylvania—clockmaker, instrument-maker, builder of orreries, surveyor, astronomer, contributor to mathematics and physics, political figure, and public servant—represented the best of science in the British colonies of North America. Contemporaries hailed him as "one of the luminaries of the eighteenth century," and his career demonstrated, even more clearly than did those of Franklin and Jefferson (whose roles in science Brooke Hindle observes with a fresh eye) what it meant to be a scientist in the generation that made the American revolution. The draftsman's skill, which Rittenhouse developed in making clocks, was the base from which he moved into science as a builder of those models of the solar system which demonstrated so much of the eighteenth century view of the universe, the orreries which are still prized possessions of Princeton and the University of Pennsylvania. The observations of the transit of Venus in 1769 from an observatory on his own farm at Norriton, Pennsylvania, stocked by instruments of his own construction, brought Rittenhouse international notice and local fame as an astronomer and led to his permanent residence in the intellectual metropolis of the British colonies—Philadelphia.

Anyone who attempts to evaluate the career of an American scientist must take some posture toward the question whether the American cultural environment is an asset to science because of political freedom and social mobility, or whether it is deadweight which swamps the rare creative individual in both mediocrity and distraction. Hindle makes a major contribution to the discussion of this question not by succumbing to either myth but by presenting in an interesting and well-written biography a figure who represents both the glory and misery of the American scientist's plight. Hindle does ample justice to the several facets of Rittenhouse's work in basic science, which he distinguishes clearly from even more varied and time-consuming activities in the applications of science, some of them tasks forced on Rittenhouse more because of his general reputation than any special knowledge. Scientists who read this book should not be disappointed when it turns into a first-rate analysis of the financial history of Pennsylvania during the Revolution, because Rittenhouse as Treasurer of the new State and a leader of the more radical of the nascent political parties was as surely expressing the Enlightenment philosophy which motivated him as when he was tracking a comet. By placing David Rittenhouse in his full scientific, intellectual, social, and political setting Hindle has made a distinguished addition to the short list of American scientists who have been favored by a biography adequate to their own stature.—*A. Hunter Dupree*

Reaction Heats & Bond Strengths by C. T. MORTIMER; 230 pages; $5; Addison-Wesley Publishing Co., 1963.

In his concise, but nevertheless comprehensive, book Dr. Mortimer describes the present state of thermochemistry and outlines the impact which the precise thermochemical data have on many fields of chemistry. He clearly defines the meaning of the fundamental concepts of thermochemistry such as heat of reaction, of formation, of atomization, etc., and shows their interrelation. The concepts of bond energies and bond dissociation energies are lucidly discussed and the ambiguities existing in the term "bond energy" are well recognized.

Nevertheless, Dr. Mortimer successfully explains the benefits derived from the concept of bond energy and illustrates this point on examples leading to the concept of strain energies, stabilization energies, resonance energies, etc. The last term—resonance energy—is well elaborated and consequently many misconceptions created by this concept are dispelled.

The treatment of strain energies is well illustrated by numerous examples, and so is the treatment of stabilization energy. Discussion of molecular addition compounds, metal-carbon and metal-halogen bonds, ionization energies in aqueous solutions, and bond strength in silicon, phosphorus, and sulfur compounds made the book interesting for many chemists. The organic, inorganic, or physical chemist finds intriguing observations and remarks.

Finally, many data given in tabulated form make this small volume valuable as a reference book, and hence it deserves its place in private collections as well as in libraries.—*Michael Szwarc*

Essays 1958–1962 on Atomic Physics & Human Knowledge by N. BOHR; 100 pages; $5; John Wiley & Sons, 1963.

This slim volume of essays, written by Niels Bohr during the last years of his long, rich life, contains variations on many of the themes that were central in his thinking. Bohr never stopped reflecting on the implications that the development of quantum physics might hold for the rest of the world of the intellect. He always stressed the concept of complementarity which, in his view, made possible the creation of "a frame wide enough to embrace the account of fundamental regularities of nature which cannot be comprehended within a single picture." The first two essays try to suggest the general importance of this new approach in atomic physics to "the development of methods for ordering and surveying human experience." The second pair of essays, including a tantalizingly incomplete lecture given a few months before his death, set forth some of Bohr's ideas on the relationships between physics and biology, always a subject of real interest to him.

By far the longest and most fully developed article is Bohr's Rutherford Memorial Lecture, which occupies close to half the volume. In it, Bohr traces his relationship to "the founder of nuclear science," paying particular attention to the years in Manchester just before the First World War, when the young Danish physicist first saw how to tackle Rutherford's nuclear atom by using the new quantum theory and began to create a real theory of the physical and chemical properties of matter. This essay, together with Bohr's reminiscences of the Solvay Congresses and his brief discussion of Heisenberg's early work in quantum mechanics, belongs with Bohr's earlier account of his extended dialogue with Einstein on the foundations of quantum theory; all are important documents in the history of modern physics.

Bohr is never easy reading. He revised and rewrote endlessly, often changing a paper completely in each of the many proofs that he required, always trying to express precisely all the subtleties and interrelationships that he perceived, and never willing to be content with even the appearance of oversimplification. As Pauli once wrote, "He knew well what he wished *not* to say when he strove in long sentences to express himself in his scientific papers."—*Martin J. Klein*

Chelating Agents & Metal Chelates, edited by F. P. DWYER & D. P. MELLOR; 530 pages; $17; Academic Press, 1964.

This interesting volume was written almost entirely by authors from "down under." Glancing at the slightly redundant title one expects to find an authoritative and extensive review of the literature in this burgeoning field. Instead, the reader is presented with a series of chapters on special topics which, while for the most part are quite interesting, represent far from exhaustive treatments of the various subjects. The chapters tend, understandably enough, to dwell at length upon work done in the authors' own laboratories at the expense of equally significant work done elsewhere. Of the ten chapters, at least two are rather specialized in nature: Chapter 7—Metal chelates of ethylenediaminetetraacetic acid and related substances and Chapter 10—Physical and coordination chemistry of the tetrapyrrole pigments; and are perhaps out of place in a volume of general nature. Chapter 2—The nature of the metal–ligand bond, is interesting and quite refreshing to read. Several new correlations are presented here which tend to increase one's understanding of a complicated subject. Comparing the table of contents with that of a similar volume: "Chemistry of Metal Chelate Compounds" by A. E. Martell and M. Calvin published over a decade ago, one finds a considerable increase in space devoted to topics of interest to biochemists in the present volume compared with the older book. The chapter on optical activity in metal chelates included in the present volume is timely and well written. Many inorganic and biological chemists will want to own this book, but its excessive purchase price may give them pause.—*W. D. Horrocks, Jr.*

Alienation & Freedom, The Factory Worker & His Industry by R. BLAUNER; 222 pages; $7.50; The University of Chicago Press, 1964.

By utilizing a national Roper job-attitude survey and by interviewing 78 workers, Blauner explores the degree of alienation of the American worker. Blauner differentiates four aspects of alienation: (*a*) powerlessness, the lack of control over the conditions of employment and control over the immediate work process; (*b*) meaninglessness, the inability to use one's capacities while at work; (*c*) social alienation, the lack of integration in a social unit; and (*d*) self-estrangement, the separation of the inner self in the activity of work.

Each of these dimensions varies in form and intensity depending upon the technology required by the nature of the product (which is Blauner's primary focus), specific economic conditions (growth rate, competition, etc.), and the social setting in which the industry is found.

The impact of technology is greatest with respect to powerlessness. Craft (typography) automated industries cause less powerlessness than either the machine-tending (textiles) or assembly-line (automobiles). In addition, the craft industry provides the worker with a sense of occupational identity whereas the automated industry (refinery) gives him a sense of responsibility and organizational identity.

Blauner concludes with an interesting historical perspective on alienation. In the early period, dominated by craft industry, alienation is at its lowest and freedom at a maximum. Alienation moves upward with the period of machine industry and peaks with the assembly-line industries of the twentieth century. Automated industry becomes a counter trend. Automation increases the worker's control over his work process as well as his feelings of responsibility for the total product.—*Chris Argyris*

Studies in Large Plastic Flow & Fracture with Special Emphasis on the Effects of Hydrostatic Pressure by P. W. BRIDGMAN; 362 pages; $9.75; Harvard University Press, 1964.

This book is not a text in plasticity, but rather it is a classic reference on the response of real materials. Most of the experimental data are more than twenty-years old, but have not been

superseded or appreciably altered. The genius of Professor Bridgman was in conceiving of direct experiments in which the phenomena were important and where elegant instrumentation was less so.

Professor Bridgman makes it perfectly clear that the behavior of real materials is very complex. In the development of the mathematical theory of plasticity, these complications are not admitted to exist. Thus, the unloading behavior (p. 207) is not a smooth functional relation between stress and strain. Most theories of plasticity would prohibit such a response.

One of the very admirable points of the research of Professor Bridgman is the use of the accepted equations of plasticity to guide his investigation. Another equally admirable trait is that he did *not* force his experimental results to fit his previously accepted equations. A large number of plasticity experiments are today considered unacceptable until they fit a theory, no matter how inappropriate that theory is in other situations.

Among the minor errors in the book is a statement (p. 346) that the Karman theory of plastic wave propagation is based on the "*statically* measured stress-strain curve." This is not so. It is based only on *some* relation existing between stress and strain. It happens that the static one works very well. One could also criticize the small number of repeat tests used to study a particular point. The use of the term *velocities* in plasticity has come to mean something different from that which Professor Bridgman uses in the last chapter.

One thought which went through the reviewer's mind while reading this book was: "How many of the recognized Professors of Physics in the United States are today vitally interested in the response of materials in the sense of Bridgman?" Certainly they will not win a Nobel Prize for their efforts; and if my experience is typical they will soon be extinct. Does this mean that materials are now well understood? They certainly are not. Who then is to make the basic macroscopic investigations and *interpret* them?

This book should be on every reference shelf concerned with plasticity. It should also be read by the modern practitioners of continuum mechanics to see just how complex the response of real materials is.—*O. W. Dillon, Jr.*

Principles of Angiosperm Taxonomy by P. H. Davis & V. H. Heywood; 556 pages; $15; D. Van Nostrand Co., 1963.

Of the several plant taxonomy texts published in recent years this one is the best. It presents the most lucid account of modern taxonomic methodology and emphasizes the importance of plant taxonomists as gatherers and organizers of biological information. Such information from a wide variety of sources is obviously of use in determining relationships among plants and constructing taxonomic systems, but it has also provided the stimulus for numerous investigations of various problems pursued by workers with little direct interest in taxonomy. Much of the contemporary research in molecular biology—particularly on "lower" plants—is based upon leads provided by more or less traditional taxonomists.

The first portions of the book are devoted to discussions of the historical development of taxonomy, its principles, and the problems associated with determining and using taxonomic characters. Subsequent chapters deal with the sources of taxonomic evidence, ranging from traditional ones such as floral morphology to the more recently developed fields such as phytochemistry. Two chapters are concerned with field, herbarium, and library methods, and with the assembling, organizing, and presentation of data. The latter part of the book deals with phenotypic and genotypic variation, evolutionary differentiation, and hybridization. These chapters are particularly valuable in their emphasis of variation patterns and how they may be handled taxonomically. The authors have only a few axes to grind. They present a wide-ranging, well integrated, and remarkably balanced account of modern plant taxonomy.—*Robert Ornduff*

The Precession Method by M. J. BUERGER; 276 pages; $13.50; John Wiley & Sons, 1964.

The Buerger precession, X-ray diffraction method is nowadays the most efficient and fastest technique for the routine determination of the unit cell and symmetry of single crystals. For example, the reviewer has charge of a laboratory where eight of the instruments are in constant use by six investigators. Since no really thorough and up-to-date treatment of this rather specialized method has been available since the monograph appeared in 1944 in which Prof Buerger first described his invention, this book is most welcome.

The book jacket says that the book is "complete and concise"; it is more than complete, but is in no sense concise. Every possible aspect of the subject that has occurred to the author and his co-workers is included here, often expounded at great length. The practical user may at times grow a little impatient with the prolixity of the treatment (for example, a whole page is taken up to describe how the screws on a goniometer head should be turned to center a crystal) and wish for more conciseness. Nevertheless, nearly everything he requires is there, while the book is much more than a manual but gives a thorough analysis of the principles and theory of the method. For example, an exhaustive treatment of cone-axis photography is given, although such photographs are of little practical importance in routine work, being used only occasionally to check the periodicity of the net plane stack above the zero net.

A few minor points of omission come to my mind. For example: no mention is made of the complementary relation between the Buerger precession camera and the Weissenberg camera which makes it possible to record all three axial planes of a crystal without remounting it; some discussion of the use of the method to study twin relationships, for which it is particularly powerful, would have been most useful; no discussion is given about the application of the Buerger precession camera to crystals with very large unit cells, such as in proteins where the method has proved to be indispensable. But every investigator develops his own personal techniques to increase the efficiency of his work, which are different from every other investigator's. Certainly, no user of the Buerger precession camera can afford to be without this book, in spite of its price.—*Howard T. Evans, Jr.*

Gravitation & Relativity, edited by H.-Y. CHIU & W. F. HOFFMANN; 353 pages; $15.75; W. A. Benjamin, Inc. 1964.

This volume is primarily the outgrowth of seminar lectures delivered in 1961 and 1962 at the NASA Goddard Institute for Space Studies at New York. The lecturers were J. L. Anderson (Stevens), R. H. Dicke, R. F. Marzke, and J. A. Wheeler (Princeton), V. W. Hughes (Yale), and J. Weber (Maryland). The topics range from fundamental and formal discussions of general relativity and modifications of that theory to relatively specific problems in astrophysics. Experimental investigations dealing with the properties of mass and charge, and with gravitational waves, are included.

Perhaps fortunately, the editors have not attempted to organize the materials available to them on tapes and in manuscript form into a unified whole. With a number of senior investigators with divers point of view contributing, such a unification would have resulted, in all likelihood, in a loss of freshness and immediacy, and in considerable delay in the appearance of this volume. In a rapidly unfolding area of investigation these considerations are much more important than formal or substantive unity of the book. Accordingly, the reader should not look for either a textbook of current theories of gravitation, or for a comprehensive review of current research. Rather, he will find a set of highly personal, but in all cases lucid and persuasive, accounts of the approaches of the individual authors. Each chapter carries its own list of references to the literature.

It should perhaps be mentioned that the editors have done a painstaking job of eliminating errors of transcription so frequently met with in material based on tape recordings. No doubt, the authors have cooperated, but the whole book reads well, and several of the articles are provided with tables and figures. All in all, the book is to be highly commended as a valuable contribution to current literature on gravitational research in the widest sense.—*Peter G. Bergmann*

The Future of Man by P. TEILHARD DE CHARDIN; 319 pages; $5; Harper & Row, 1964.

Like its famous predecessor, *The Phenomenon of Man*, this collection of twenty-two essays by Teilhard defies pigeon-holing as a strictly scientific, philosophic, speculative, or visionary work. Written between 1920 and his death in 1955, these essays touch many areas of scientific endeavor and concern. While fourteen of them have already appeared in scattered scientific journals, seven were unavailable until their appearance in this fifth volume of the collected works which, in French, now comprise eighteen volumes of essays and letters.

Despite some outdated scientific data, the provocative and often prophetic insights of Teilhard make this both a timely and valuable collection. More readable and down-to-earth than the *Phenomenon*, it should stir interest and debate among the specialists in psychology, education, sociology, anthropology, political science, and the whole range of the biological sciences. It should also appeal to the physical scientist who seeks a more integrated and unifying approach to his vision of the world.

Of special interest are the evidence and arguments presented for the unique convergent nature of the human zoological group where the subhuman pattern of divergence and speciation is not found; a fact often ignored by the sociologist.

Teilhard's biological interpretation of human history—particularly the role of war in forcing human convergence—as the extension and continuation of organic evolution into the psychic level in the socialization and planetization of mankind, along with his consideration of education as an "essential and natural form of biological additivity" should clarify the "directional", "orthogenetic," or "Lamarckian" interpretation of the evolutionary process which many objected to in the *Phenomenon of Man*. His discussions of the influence of the evolutionary perspective in modern life and thought, of forced and free human convergence with its economic, intellectual, and social implications, as well as other rich insights from the Teilhardian synthesis, make this a book worth exploring for any scientist or thinker. Debatable, but stimulating.—*Robert T. Francoeur*

Plant Virology, edited by M. K. CORBETT & H. D. SISLER; 527 pages; $12.50; University of Florida Press, 1964.

Text books written by an assemblage of experts are becoming necessary and fashionable. The contributors to this volume do not all qualify as experts in being a long way from home, as only 4 of the 22 are from outside the U.S., but they include leading virologists from the U.S. and England. This group, organized by M. K. Corbett, assembled over a period of several weeks to give a course in plant virology at the University of Maryland. Fortunately or unfortunately, they include several who are primarily or exclusively animal virologists. The section on tumors is about 97% devoted to animal tumors. The section on diseases of arthropods is about 93% devoted to viruses which have no known invasiveness in plants, and Suchov's 1940 evidence of a plant virus changing its insect vector is not referred to. The subject is organized into introduction, symptoms, transmission, identification, assay, strains and interference, insect vectors, purification, serology, electron microscopy, structure, biochemistry, infection, control, taxonomy, form and function, diseases of arthropods, tumor viruses, speculation on the origin of viruses, subject index, and author index. With the exception of one chapter, all information is well-documented. The chapter

on symptoms, by Holmes, is hardly documented at all, but this may be excusable, in that to have documented this extensive coverage would likely have taken as much space as the text. Of an estimated 12,000 papers on plant virology since 1900, an estimated 2000 are cited here. In addition, there are several hundred references to bacterial, insect, and mammal viruses, and the similarity and possible interrelation of viruses attacking the four major groups of hosts are set forth. Starting points as well as current information are usually given. It may not be as well written as Bawden's 1964 edition of *Plant Viruses*, but it is more comprehensive. The only error noted was in the mislabeling of the tubes in Fig. 10-4 of one of Steere's excellent diagrams of purification procedures. This book could serve well as a beginning book for a student and as a reference book for a professional.—*C. E. Yarwood*

Sampling Systems Theory & Its Applications by YA. Z. TSYPKIN; Two Volumes, 742 pg.; $30.00; The Macmillan Co., Pergamon, 1964.

This very extensive and expensive set by a leading Russian engineer is not likely to find its way to the shelves of many workers simply because of its price. This is too bad because the book has several attractive features which would prove useful to many. There are six chapters each approximately one hundred pages long. Their titles are: Pulse Systems and Their Applications, Fundamentals of the Discrete LaPlace Transformation and Difference Equations, Fundamentals of the Theory of Open-Loop Pulse Systems, Investigation and Analysis of Open-Loop Pulse Systems, Fundamentals of Closed Pulse Systems, and Investigation and Computation of Closed Pulse Systems.

The author places considerable emphasis on the practical application of pulsed systems and includes much material on such topics as multi-color printing presses, radar systems, digital computer circuitry, wide band video amplifier, communication system using prediction, automatic frequency control system, automatic gain control system, and many others. In a typical discussion of one of these topics, the author exhibits a block diagram of the system with a description of the operation of the physical system operation. The assumptions of the analysis are then spelled out and an analysis of the (usually simplified) system is undertaken. It is shown how certain system parameters affect system operation and some ideas about improving performance are given.

There are several drawbacks to the books other than price. The index is quite inadequate. The quality of the paper and binding is poor even though the type setting is very satisfactory. Many references are to Russian language sources which limit their value to Western readers. This is bound to happen in such a book. The translation has been well edited but, since the original was written in 1958, the approach taken in certain sections does not reflect the current methods of analysis. Little discussion of non-linear pulse systems is to be found in the volumes. Because of an emphasis on application to physical systems, the author has chosen to exclude such topics as approximation techniques for non-linear systems.

Generally, the book is of value to people with particular interest in the application of pulse systems to certain industrial processes. Because of its age, the material is of limited value to current research efforts in pulsed system theory.—*Stephen J. Kahne*

The Genetics of the Silkworm by Y. TAZIMA; 253 pages; $9.95; Logos Press, London; Prentice-Hall, 1964.

The silkworm *Bombyx mori* is one of the genetically best known organisms. Workers in different countries have contributed to its analysis but Japanese geneticists have made the most important and most numerous contributions. Much of their work has been difficult of access to students outside of Japan and even the valuable reviews by Tanaka and Kikkawa in *Advances in Genetics* (1953) could not

give adequate summaries. Such a summary is now available in Tazima's book. It is detailed without being encyclopedic. In about 200 pages of text, illustrated with 58 figures or graphs, a clear, penetrating, and critical account is provided. The genetics of Bombyx began in terms of transmission genetics and, as early as half a century ago, reached a high level of achievement. It branched into biochemical genetics, especially of pigment determination which, in the thirties, had been inaugurated for the eye pigments of Drosophila. It had furnished one of the first examples of maternal determination of phenotypes and led to biochemical interpretations of this phenomenon. Beginning primarily with the forties, it added fundamentally to knowledge of sex determination demonstrating that the W (=Y) chromosome contains a male determining region and that ploidy of the autosomes does not shift the type of sex: "... whenever a W chromosome is present the individual is invariably a female, without it it is always a male." Unlike Drosophila or Lymantria, no intersexes seem to occur in Bombyx. Tazima himself has contributed decisively in this area, as well as in that of mutagenesis. The silkworm is suitable for very large-scale experiments concerning mutations at specific loci and the book contains important summaries of work involving spontaneous as well as radiation-induced mutations. Other topics covered relate to the remarkable E group mutants which change the development of whole segments, similar to Lewis' bithorax mutants in Drosophila, genetic control of hormonal mechanisms, mosaicism, parthenogenesis, and polyploidy. Tazima's book is much more than a record of past work. It points to new areas of studies and will be productive of new insights.—*Curt Stern*

Solar System Astrophysics by J. C. BRANDT & P. HODGE; 457 pages; $12.50; McGraw-Hill Book Co., 1964.

The authors set out to write a survey of the solar system, including the Sun, planets, and interplanetary space, which will be useful both as an advanced text and as an introduction to the field for space scientists and engineers. Considering the great variety of topics covered (from hydromagnetic waves to atmospheric dust), I feel they have succeeded remarkably well. The plan is to describe the phenomena involved in physical terms, to sketch their mathematical treatment, and to give references to more detailed accounts.

This approach requires considerable self-discipline in order to restrict the treatment of each phenomenon to the few pages allotted to it. By so doing, the authors are able to consider in succession the deep solar interior, its surface layers, interplanetary space, and finally the atmospheres and interior of the planets, all in some detail. The mathematical training required varies rather widely, from the vector analysis of hydromagnetism, to mere graph-reading in the chapter on meteorites. Perhaps this reflects the propensities of the two authors involved. In any case, the statement in the introduction that elementary and atomic physics, as well as calculus are necessary to understand everything, is perhaps an understatement. This suggests that, if the book is to be used as a text, it will be useful in junior and senior astronomy courses primarily.

The style is concise and attractive, the illustrations numerous and helpful, and the bibliographical notes are extended and useful. My only negative findings were a few minor errors: the density of Mercury is correctly given as 5.3 g/cm^3 on p. 321, and quoted three times with a decimal point error on p. 329. Newer results on the optical polarization of Jupiter (Gehrels) are ignored, giving a false impression of the atmosphere of that planet.

In summary, I believe this book will meet the needs mentioned earlier, and meet them well. Presumably the high price is partly due to the use of several dozen photographic reproductions. It is questionable whether their inclusion was worth the loss of a rather large market of bright young students who can't afford the book.—*George B. Field*

Electron Paramagnetic Resonance by S. A. AL'TSHULER & B. M. KOZYREV; 374 pages; $13.50; Academic Press, 1964.

This volume is published in 1964 as 'a comprehensive treatise on the field of electron paramagnetic resonance, covering both the theoretical background and the results of experiment." In general, the authors have succeeded in producing a book of definite value to those engaged in electron paramagnetic resonance (EPR) research. It is important to note, however, that this is a translation of a text originally published in 1961 in Russian, and that its "comprehensive" data and references are limited to the period prior to 1959. The absence of the extensive theoretical and experimental results in EPR research of the past six years constitutes a regrettable deficiency in a book of this type.

The first two chapters of this book present a brief review of resonance theory and experimental techniques. The conciseness of these chapters is offset by the clarity of the authors' description of basic concepts and devices. Here, as throughout the book, the references are the most complete available, and very well organized.

Chapters III to V discuss ionic crystals. These chapters constitute the major part of the book as well as reflect the primary interest of the authors. The theory of the EPR spectra of ionic crystals is presented in Chapter III by Al'tshuler. In Chapter IV Kozyrev presents a comprehensive review of the experimental data obtained from these spectra. This compilation of data on the ionic crystals utilizes and complements the well-known Bleaney-Stevens and Bowers-Owen articles from the *Reports on Progress in Physics*. The data are presented in the form of tables, and the many references are conveniently cited.

The final three chapters review most of the remaining phases of EPR research. Here, however, there is not the detailed development accorded the ionic crystals. Metals, semiconductors, color centers, liquids, free radicals,

and double resonance experiments are included in the discussion. The sections on the various methods of dynamic nuclear polarization and on maser research especially suffer from the absence of the data and developments of the past six years.

Finally, the quality of the translation and typography is to be commended. The inadequacy of the subject index is distressing, but this is a minor deficiency in view of the overall excellence of this book.—*Robert F. O'Brien*

Analysis of Ancient Metals by E. R. CALEY; 173 pages; $6.; The Macmillian Co., Pergamon, 1964.

The present monograph presents reasoned conclusions based on a very considerable effort over a period of years by Dr. Caley and his associates.

The problem of selecting a representative sample for chemical analysis, particularly for corroded objects that may have considerable historical and artistic value, is discussed thoroughly at the outset.

The selection of a chemical analytical method, or other means of determining composition, depends upon preliminary tests and observations. These problems are discussed in the second chapter.

The various methods of gaining analytical information are discussed in separate chapters for each of gold, silver, copper, and their alloys. A chapter is devoted to other metals, primarily lead, tin, and their alloys. The determination of the composition of iron and steel objects comprises a separate chapter. A final chapter deals with the important problem of presenting the results in the form of reports which give the maximum information to scholars who may have no training in chemical principles and in the science and art of determining chemical composition. Many tables of analytical data for various classes of materials and other numerical data are given, as well as a subject index.

The chemical analytical methods are in the main of the simple and "tried and true" variety. The author gives careful consideration to so-called non-destructive methods, e.g., specific gravity, X-ray fluorescence, and to photometric techniques, neutron activation, and others. Some of these methods are inapplicable because they cannot be applied without cutting the specimen or because of corrosion layers.

This thorough and accurate treatise gives an excellent selection of literature references in each of the principal chapters. As a whole, the monograph should be of interest to archaeologists, museum workers, and to all who have interest in, or are confronted with, the problem of determining the composition of ancient metallic objects.

The author is well known for his contributions to the development of new analytical methods, and for his numerous studies, extending over three and a half decades, on the use of chemistry, particularly quantitative chemical analysis, as a contributing means for solving archaeological problems. During the first decade and a half of this period it was the reviewer's privilege to observe his meticulous care and the excellent scholarly approach brought to both areas. During this period he established by chemical analysis a time-scale of the bronze coins of Asia Minor based on systematic variations of the lead-tin ratio through the centuries.—*N. H. Furman*

Physics of Magnetism by S. CHIKAZUMI (English edition prepared with the assistance of S. H. CHARAP); 554 pages; $15.75; John Wiley & Sons, 1964.

This book is an expansion of the 1959 Japanese edition, but except for a brief slip here and there, it reads as if it had originally been written in English, so that both the author and Dr. Charap can take pride in the presentation. The subject matter is of correspondingly high order and the main complaint which one could make concerns the statement on the jacket to the effect that no previous knowledge of magnetism is assumed. This book overlaps heavily with the two widely-used elementary treatments: Dekker's *Solid State Physics* or Kittel's *Intro-*

duction to Solid State Physics—and the present volume will be rough going for anyone without this kind of background. The elementary material is reviewed very briefly and then there is a thorough discussion of each of the topics which Dekker or Kittel cover lightly. The level is fairly nonmathematical, and many of the formulas are derived from plausibility arguments. The core of this book is the treatment of domain theory (about 9 chapters) from both a phenomenological and a quantitative basis. It is a pity, however, that nothing was included on the topic of micromagnetics. It has been pointed out a number of times by W. F. Brown, Jr., that domain theory is logically inconsistent when the size of the domain is reduced down to that of the domain wall (as in magnetic thin films). Although micromagnetics does not give the answers which domain theory fails to furnish—because of the difficulty of obtaining rigorous solutions—a book which is supposed to be an up-to-date review of the field of magnetism should at least mention this development. Otherwise, this text can be highly recommended.—*Allen Nussbaum*

Electronic Spectra & Quantum Chemistry by C. SANDORFY; 385 pages; $14.95; Prentice-Hall, Inc., 1964.

This book fills a definite need for many quantum chemists. Professor Sandorfy's book is a good attempt at bridging the gap between formal textbooks and published works at a more advanced level. It will be helpful to many students who know some fundamentals of quantum mechanics but want to introduce themselves into the actual research and calculations of electronic spectra especially for π-electron systems.

The book begins with the usual preliminaries of wave mechanics but quickly moves onto the molecular orbital and valence bond methods. The chapter on the valence bond method is long and well written. This is something on which very few textbooks spend any time nowadays. The

book is arranged so that the chapter may be omitted. Its inclusion is worthwhile not only because it explains very clearly how one goes about using the valence bond method on large molecules, but also this method is useful in general quantum chemical thinking.

The book then moves onto a clear presentation of the methods of electronic spectra; e.g., symmetry of molecules, selection rules, calculation of spectral quantities, and the SCF method. One attractive feature is the detailed numerical examples; e.g., on the intensities of spectral transitions both by the molecular orbital and valence bond methods. Finally, it includes some semi-empirical methods and the free electron model. The chapter "Summing up. What next?" is indicative of the tone of the book. The bibliography is up-to-date and complete.—*Vincent McKoy*

Experimental Endocrinology: A Sourcebook of Basic Techniques by M. X. ZARROW *et al.;* 519 pages; $15.; Academic Press, 1964.

A compilation of the basic techniques used in endocrinology has long been needed. Thus, the above book becomes a basic necessity for the library of all interested in the study of hormones whether vertebrate or invertebrate. Invaluable as this book might be, it seems necessary to indicate this reviewer's opinion of certain apparent shortcomings of the authors.

The authors have attempted to prepare not just a laboratory manual but a textbook as well. This dual task is too much to ask of 500 pages of text. It is inevitable that, as a text, some areas of the subject matter should be slighted or in some cases only one view of the picture presented.

As a laboratory manual, it is hard to see how this book could be used by students or investigators in an independent manner, unless they were already well versed in animal techniques, since such basic procedures as how to hold an animal and make various types of injections are never discussed. Furthermore, instructions for autopsy, including the proper removal and preparing of endocrine tissues or other organs for weighing appears to be absent.

In spite of this lack, or the scantiness of detail regarding certain features of experimental procedure, this book provides an invaluable function in its compilation, in one place, of many of the procedures widely used for the study of the ductless glands in all types of organisms. Many of these procedures will continue to serve a basic function in the science of endocrinology for a long time to come. Thus, for all who wish to understand the fundamental procedures used in endocrinology, as well as for any who contemplate an experimental attack, a basic source book is now available.—*Robert D. Lisk*

The Mitochondrion (Molecular Basis of Structure & Function) by A. L. LEHNINGER; 262 pages; $9.; W. A. Benjamin, Inc., 1964.

In about 250 pages, Lehninger presents a distillate of the massive and widely scattered volume of literature on the development of our current knowledge of the structure and function of the mitochondrion. Unlike many contemporary compendia on the subject, his account is carefully stripped of much detail of technique and experimental data. Consequently, the result reads—in very lucid language—like an exciting, action-packed drama, and it relates one of the most fascinating and sophisticated stories of modern biology.

The book is divided into eleven chapters of about 20 pages each. The references at the end of each chapter are in most cases carefully chosen, and the review articles and the research papers have been helpfully separated into two categories.

In general, those facets of the mitochondrial structure and function which have been the subject of study in the author's own laboratory have been better presented. By contrast, electron transport, which by definition should be the central theme of any treatise con-

cerning the mitochondrion, has not been given the attention and the space it deserves. The E_0' of coenzyme Q, the molecular weight of NADH dehydrogenase and the nature of the prosthetic group of this enzyme should all be revised. There are at least three species of nonheme iron in the electron transport system which have not been adequately discussed, and the possible role of nonheme iron—and in many instances that of coenzyme Q—has been omitted from discussions of electron transport (see Figures 3–4, 6–1 and 6–2 and the pertaining discussions in the text). In addition, the nature and the composition of lipids, which are the second major constituents of mitochondria, have not been reviewed.

However, these shortcomings detract very little from the value of the book. Definitely, the author should be congratulated for his successful effort and he certainly deserves a wide audience for his fascinating and instructive account of the mitochondrion.—*Youssef Hatefi*

Organic Syntheses (An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals), Vol. 44, 1964, W E. Parham, Editor-in-Chief; 131 pages; $4.95; John Wiley & Sons, 1964.

This is the 44th volume in one of the oldest, most respected, and undoubtedly most useful series in chemistry. The necessity of having available thoroughly worked-out and independently tested procedures for the preparation of organic compounds, with explicit directions and honest yields, does not seem to diminish in spite of the increasing emphasis on instrumentation in chemistry.

Since the series is well known to its users, it is hardly necessary to say more than that the newest volume maintains the same level of accuracy, detail, and interest as its predecessors. Of the 31 compounds whose preparation is described here, several may be noted as of more than usual interest: Indoleacetic acid, the plant growth hormone; 7-t-butoxynorbornadiene, an entry to 7-substituted bicycloheptanes; 3-quinucli-

done, a bridged amino ketone; t-butylazidoformate, a useful reagent in peptide synthesis; and trimethylpyrylium perchlorate, one of the few high explosives to be listed in *Organic Syntheses*. Perhaps more generally useful will be some of the methods of wider scope: a new method of converting Grignard reagents to aldehydes, a simple and practical synthesis of azulenes, and a useful modification of the Koch-Haaf reaction which allows the carboxylation of branched saturated hydrocarbons. Detailed directions are given for two useful syntheses of amines, the Ritter preparation of t-carbinyl amines, and the Decker-Becker monoalkylation of primary amines. Hauser's practical modification of the Reformatsky reaction, using lithium salts, is a welcome addition. The index covers volumes 40–44.—*Richard K. Hill*

The Life of the Cell (Its Nature, Origin, & Development) by J. A. V. Butler; 167 pages; $4.50; Basic Books, 1965.

The title and contents of this book taken together, demonstrate forcibily what has happened to biology in the last couple of decades. A title such as "the life of the cell" conjures up a vision of classic biology, but the contents are almost purely molecular biology.

The book is intended for the lay reader who has little knowledge of the life sciences save what he may remember of general biology courses and what he has picked up since from newspapers. In concise, directly-written chapters, it begins with the simplest constituents of living tissue and then mounts through grades of complexity to proteins and nucleic acids. It reaches the level of life, with chapters on viruses and bacteria, then takes up more complex organisms. Final chapters deal with tissue components of multicellular creatures, including one on the brain, and the last chapter expands the view to the universe as a whole and its possible life-content.

The key to the entire book is, as one might expect, that fashionable set of initials, DNA. It arrives first in the discussion of mitosis and a description of replication. The role of DNA in the synthesis of protein surrounds the discussion of viruses, and its part in mutation lends interest to the material on bacteria.

Multicellular organisms raise the question at once as to how tissues can be specialized and yet bear identical sets of DNA molecules. (Surely this is the most tantalizing mystery in molecular biology at this moment.) The workings of antibiotics are interpreted in terms of their interference with DNA-supervised protein synthesis. The connection of DNA with cancer and with the problem of the origin of life is obvious.

The book is a pleasant and clearly-written guide, for the layman, through the molecular biological wilderness, with DNA as a unifying thread.—*Isaac Asimov*

Collected Scientific Papers by Wolfgang Pauli, edited by R. Kronig & V.F. Weisskopf; Vol. 1, 1133 pages; Vol. 2, 1408 pages; $70; John Wiley & Sons, Interscience, 1964.

These two large and weighty volumes contain the complete scientific writings of Wolfgang Pauli—some twenty five hundred pages of his books, review articles, occasional works, and, of course, his scientific papers. Pauli was born in 1900, with the twentieth century and the quantum theory, and his scientific work spans the years from 1919 (!) to his premature death in 1958. The scope and character of his work may be suggested by remarking that a thorough study of these two volumes would come close to providing a complete, and profound, education in modern theoretical physics—in its content, its methods, and especially its spirit.

Pauli's first major work was done while he was still a graduate student: at the request of his teacher, Arnold Sommerfeld, he wrote a comprehensive article on relativity for the Mathematical Encyclopedia. It was greeted by Einstein in words that characterize all Pauli's work. "No one studying this mature, grandly conceived work would believe that the author is a man

of twenty-one. One wonders what to admire most, the psychological understanding for the development of ideas, the sureness of mathematical deduction the profound physical insight, the capacity for lucid, systematic presentation, the knowledge of the literature, the complete treatment of the subject matter, or the sureness of critical appraisal."

These volumes contain such famous masterworks as the paper introducing the exclusion principle, the solution of the hydrogen atom by matrix mechanics, the incorporation of the spin into wave mechanics, the papers with Heisenberg on field quantization, the suggestion of the neutrino at the Sixth Solvay Congress, the many papers on spin and statistics, and the Handbuch article on wave mechanics. But there are also lesser known gems, like the theory of paramagnetism for free electrons (to which Pauli referred in his remark, "I don't like this solid state physics—I started it, though"). There are Pauli's answers to Ehrenfest's probing questions on quantum mechanics; (Pauli inherited Ehrenfest's title as "the conscience of physics," and wrote a moving and perceptive obituary, included here, of his friendly antagonist of many years).

Even some of Pauli's unique personality appears here—in his letter to Weisskopf after the overthrow of parity conservation, in his "Remarks on the History of the Exclusion Principle," and in some of his reviews. Who else would have greeted an article on a new unified field theory of Einstein's in 1932 by remarking on the almost regular rate with which such theories appeared, each referred to as "the definitive solution" of the problem, and by suggesting that one should announce annually: "Einstein's new field theory is dead. Long live Einstein's new field theory!"

It is a pity that the price and, unfortunately, the fact that the bulk of the material is in German will keep this set out of the hands of the graduate students who would profit so much from it. Let us hope they will find their way to it in the libraries!—*Martin J. Klein*

Recent Progress in Surface Science. Vol. II, edited by J. F. DANIELLI, *et al*; 541 pages; $18.00; Academic Press, 1964.

The British team which six years ago edited a collection of papers on surfaces as a token of homage to N. K. Adam, now works on a more permanent basis. The second volume of the series contains 5 physico-chemical and 5 biological papers. "Physical Adsorption at the Gas-Solid Interface" by Dubinin, Bering, and Serpinskii, "Heterogeneous Catalysis" by Brennan, "Contact Angles" by Elliott and Riddiford, "Emulsions" by Davies, "Flotation" by Jay and Robinson, "The Genetic Control of Cell Surfaces" by Beale, "The Physiology of Pinocytosis" by Rustad, "Plastron Respiration" by Crisp, "Preparation and Properties of Isolated Cell Surface Membranes" by O'Neill, and "The Cell Membrane: Image and Interpretation" by Elbers.

The coverage is, perhaps, too broad; not many people would be interested in both halves of the book, and either the chemical or the physiological part will remain unappreciated. Another drawback is common to all collections of this type: a contribution to a "legitimate" scientific journal is written when the author has something new to say, while a contribution to a "Progress in..." series is prepared on the invitation of an editor and may, to a considerable extent, repeat earlier papers or books by the same author.

The articles are supposed to report on the six years, 1956 to 1961, but, naturally, many earlier references also are given. The first review is particularly impartial; my only objection is that it, as usual, concentrates on the changes produced by the adsorption in the gas while the simultaneous (and equally important) changes in the solid are not emphasized. The authors of the third article disagree with the reviewer so that he must disagree with them. However, all chemical reviews are well done and deserve careful reading; and physiologists should judge the rest.—*J. J. Bikerman*

Biographical Memories of Fellows of The Royal Society, 1964, Vol. 10; 388 pages; $6.; The Royal Society, London, W.1.

Three of the twenty-one memoirs in this volume are of especial interest to North Americans; they are the memorials to Herbert Spencer Gasser, Edgar William Richard Steacie, and Otto Struve. They share a common feature in their lives, high scientific achievement and, at the same time, superb administrative skill. Lord Adrian records of Gasser that "he became one of the foremost physiologists of his generation and remained so in spite of his wider responsibilities as Director of the Rockefeller Institute." Steacie, born in Montreal, studied at McGill, and, after staff appointments there, proceeded to the National Research Council at Ottawa, first as Director of the Division of Chemistry, then Vice-President and President of the whole Council. "It is difficult," writes his biographer, "to consider the development of science in Canada since the last World War without at the same time thinking of the late E. W. R. Steacie, and of the impact that he had on its development." At the same time, "Steacie's contribution to the advancement of free radical chemistry was a monumental one." Otto Struve came to the U.S.A. from Kharkov in Russia. A Ph.D. in astronomy at Chicago was followed by Directorships which took him to Yerkes, McDonald, and to the head of the Astronomy Department at Berkeley. His researches brought him international honors. In 1960 he was selected to become Director of the National Radio Observatory, at Green Bank, West Virginia, where he ushered in the era of radio-astronomy in the U.S.A.

Human Engineering Guide for Equipment Designers, Second Edition, by W. E. WOODSON & D. W. CONOVER; 473 pages; $10.00; University of California Press, 1964.

This book contains a large amount of material, both qualitative and quantitative, relating to the interaction between

man and his environment. It is pertinent to the design of all sorts of mechanical equipment in which humans are involved. How much room is needed for a seated person to work comfortably? How far can he reach for a control knob? What should be the size and shape of the knob, and how much torque can he exert? What kind of indicator is best to display what has been accomplished? How should the work area be lighted and ventilated? How are these matters modified in a stationary location, in a jet airplane, or in a space vehicle? Some conclusions are trivial: "Seat heights are expected to be at a certain level when a person sits down." Others are more subtle: "Empty space myopia is a condition in which the eyes tend to accommodate for a distance of about 6 meters in front of the observer. Objects beyond this distance are consequently out of focus and may not be seen. This can be a serious problem if the astronaut is being depended upon to detect other objects

or vehicles." This reviewer was interested in comparing the dimensions of the driver's seat of his automobile with those proposed for an optimum seat for the driver who must remain alert and comfortable for long periods of time. His conclusion was that his trips should be limited to only short duration.—*W. J. Cunningham*

The Groups of Order $2^n (N \leq 6)$ by M. HALL, JR. & J. K. SENIOR; 225 pages; $15.00; The Macmillan Company, 1964.

In a difficult subject like finite group theory it is of great importance to have examples to test theories. However, examples are not readily available. E.g., the problem of finding the exact number of (non isomorphic) groups of a given order is forbiddingly difficult. Everything is easy up to order 8 where there are three abelian and two non-abelian groups, the quaterion group and the dihedral group. The next complicated order is 12, then comes 16. There are 14 groups of order 16 and 51 of order 32.

This book gives a table of the 267 groups of order 64. Even the number was not known previously. Each group is presented (*i*) by generators and defining relations; (*ii*) by generating permutations; (*iii*) by a diagram of normal subgroups, the normal subgroup and factor group being identified in each case. Further, each group is given three identification marks, the order, its "family" and its "place" in the family. Some classification principles devised by P. Hall and published elsewhere are used.

The prime number 2 plays a special role in finite group theory in many connections and the problem of this book is one of them. Groups of order p^r, p an odd prime, behave differently in certain respects from groups of order 2^r. E.g., there is no non-abelian group of order 2^r all of whose elements ($\neq 1$) have order 2. There is, however, a group of order 27 all of whose elements ($\neq 1$) have order 3.

The work was originally started separately by Senior (a chemist) and P. Hall. Their later collaboration was interrupted by the war. When P. Hall found himself unable to return to the plan, M. Hall, Jr. used his wide experience in group theory to fill his place and to explain the theoretical background of the construction.

All the work was done by hand; since electronic computers are now being employed for algebraic and combinatorial problems it is possible that this enormous task could have been eased. Patience and time were needed for this work, but the reader will soon notice that far more than that was involved.—*Olga Taussky Todd*

Physical Acoustics Principles & Methods, Vol. 1, Part A: *Methods & Devices*, edited by W. P. MASON; 515 pages; $18; Academic Press, 1964.

The past ten years have witnessed an enormous activity in the area of physical acoustics as regards research in gases, liquids, and solids and applications in such diverse fields as delay lines for information storage, mechanical and electromagnetic filters for communication channel separation, ultrasonic cleaning, testing, inspection, measuring, machining, welding, soldering, polymerizing, homogenizing, medical diagnosis, surgery, and therapy.

The book under review constitutes one-half of the first volume of a rather ambitious undertaking, viz., the production of an integrated treatment of the techniques, applications and analytical results obtainable by the methods of physical acoustics. The rate of growth and development in this field is ever increasing and the proposed series under the editorship of Dr. Mason, himself a most prolific contributor to physical acoustics, is most welcome. Since all applications and analytical uses depend upon the tools and techniques of generation, detection, and measurement of mechanical disturbances in material media, it is appropriate that the first volume of the series deal with these topics. It is stated that the subsequent volumes will apply the principles and methods of Volume 1 in dealing with the effects and analysis of wave propagation

in gases, liquids, solutions and polymers (Vol. II); with the effects of point dislocation and grain boundary imperfections on acoustic velocity and attenuation in polycrystal and single crystal metals and in insulating crystals, with lattice dynamics and with loss mechanisms in the earth (Vol. III); and acoustic topics which contribute to an understanding of solid state physics (Vol. IV). An obvious important omission is that dealing with the topics associated with the interaction of ultrasound and biological media.

The seven chapters of Part A of Volume I deal with wave propagation in fluids and normal solids, guided wave propagation in elongated cylinders and plates, piezoelectric and piezomagnetic materials and their function in transducers, ultrasonic methods for measuring the properties of liquids and solids, the use of piezoelectric crystals and mechanical resonators in filters and oscillators, and guided wave and multiple reflection ultrasonic delay lines. Although the emphasis is on physical aspects, rather than engineering details, the references provided are adequate for the reader to obtain the necessary information for practical applications. All authors are acknowledged leaders in their respective fields and the treatments are prepared at the level of the advanced graduate student.—*Floyd Dunn*

Arachnida by T. Savory; 291 pages; \$9.50; Academic Press, Berkeley Square, London W. 1, 1964.

Books on spiders are few and still fewer consider also the harvestmen, scorpions, and other arachnids. The only sources of information on these groups are out-of-date or rather technical. Savory has written numerous books and popular articles on spiders; the present one replaces *The Arachnida,* published thirty years ago.

The book deals mainly with morphology and systematics. It is commendable that Mr. Savory has adopted the nomenclature of the German and French literature rather than the profusion of English names. On the other hand, the book impresses the reader as rather superficial. For instance, phenomena are named rather than interpreted in view of the possible adaptative significance. The volume is not up-to-date in all fields. Biochemistry, physiology, behavior, and ecology are barely mentioned. The discussion of spider silk chemistry refers to the classical work of Emil Fischer (1907), but makes no reference to more recent work using modern analytical methods. There is less than a page on chitin, and no indication that it is just one of the polymers present in the exoskeleton. One is bothered, moreover, by the quaintness of such statements as "Not the least of the intentions of this book has been to display Arachnology as an autonomous, independent science, a branch of knowledge as clearly defined as conchology. . ."; in a discussion of the arachnid origin of the vertebrates, ". . .the origin of the Vertebrata is to be found among the Echinodermata and the arachnid theory is left as no more than an interesting episode in the history of arachnology"; by the chapter on "Arachnophobia"; and by one on historical arachnology, listing the dates of various important publications interspersed with the dates of demise of the better known students of arachnids.

Savory's volume, however, presents such useful sections as a detailed chapter on classification including fossil groups, a table giving the comparative nomenclature of orders, a useful list of books and sources on arachnids, and much information that is not otherwise easy to find, some of which is anecdotal and some of great interest. *Arachnida,* despite its minor shortcomings, deserves a place on the shelf of students of invertebrate zoology.

The would-be purchaser might be interested to know that the price is 3 £ (\$8.40) if bought from an English bookstore, while it is \$9.50 if bought from the American subsidiary of Academic Press.—*Herbert W. Levi*

Advances in Photochemistry, Vol. 2, eidted by W. A. Noyes *et al.;* 453 pages; \$16.50; John Wiley & Sons, Interscience, 1964.

In the rapidly unfolding vistas of

organic chemistry, photochemistry is almost in the forefront. The volume under review takes stock of the field as it stands now and ponders over the many questions of theoretical interest that have eluded solution thus far. Nine different chapters cover the progress in the investigations relating to: free radical and molecular reactions in the gas phase, elementary gas phase reactions involving hydroxyl and oxygen atoms, photochemical reactions of sulfur and nitrogen heteroaromatic systems, photochemical processes in halogenated compounds, preparation, properties, and reactivity of methylene, photochemistry of organic nitrites and hypohalites, phosphorescence and delayed fluorescence from solutions, photoionization and photodissociation of aromatic molecules by vacuum ultraviolet radiation.

The treatment in the different chapters varies from highly theoretical and quantitative discussions (Benson; Kevan and Libby) to practical utilitarian aspects of organic synthesis (Mustafa; Akhtar). Such diversity offers ample evidence of the scope for potential investigations. Mustafa's chapter, in particular, covers virtually every known type of chemical transformation possible when an organic molecule absorbs radiation in the visible and ultraviolet range. The chapter by Kevan and Libby is one of the most lucidly presented discussions of the chemistry of ionic states, a large part of it dealing with reactions in the solid state. Professor Benson's comments on "Snap-out reactions" or "methylene insertions" provide stimulating and provocative propositions which indeed are "intuitively reasonable." The expression "ionicity" (page 22) seems to be unusual.

The book is well arranged and printed. The reviewer would only wish to make the remark, that numbering the chemical formulae with Arabic numerals (Akhtar's chapter) seems to make for easier reading than the use of Roman numerals (Mustafa's chapter) especially when the number gets beyond 200!

The editors richly deserve to be congratulated on bringing together such excellent and topical reviews in the

fascinating field of photochemistry.—*B. S. Thyagarajan*

The Transition from Childhood to Adolescence (Cross-Cultural Studies of Initiation Ceremonies, Legal Systems & Incest Taboos) by Y. A. COHEN; 254 pages; $5.75; Aldine Publishing Company, 1964.

In this provocative study of the means by which children learn to define and locate their places in society, Cohen reveals a broadly eclectic perspective inspired by the findings of psychoanalysis, clinical psychology, physiology, primate ethology, animal ecology, genetics, and comparative ethnology. The child's location in the family and extended consanguineal kin groups is the theme which ties together three cultural explicanda whose origins and functionings have intrigued anthropological theorists for a hundred years: the ritualization of puberty; primitive legal systems and the notions of individual and collective responsibility; and the universal prohibitions of sexual intimacy among specified categories of kinsmen.

Sharing the renewed interest in the relationship between human biology and culture, Cohen notes that learning is most likely to be effective during those ontogenetic epochs when, because of physical changes which he cannot understand and may not even fully sense, the child is psychologically "vulnerable." The institutionalized behaviors associated with the transition to adolescence may be several, corresponding with two stages of biological puberty. The second are those rites, often accompanied by ordeals and the abrupt segregation of the initiates, which occur around the time of the appearance of the overt secondary sexual traits. But the first is the more profound. Occurring several years earlier, at the "first stage of puberty" (marked by biochemical changes not superficially visible), it is often accompanied by the extrusion of the male child from his household and the avoidance of further familiar contact between brother and sister. The behaviors at both stages function to break down the prior tight identification with the nuclear family and force the child into other sets of relationships. Basic identification within the nuclear family correlates with the principle of personal liability in law, and an emphasis on the extended kin group correlates with collective responsibility.

Recent explanations of incest taboos have noted the economic advantages of exogamy, demographic factors in prehuman societies, deleterious effects of inbreeding, etc. Cohen suggests that there is a universal psychobiological need for privacy which is protected by channeling ego's sexual activities away from members of those groups who claim his primary identificative energies, towards kinsmen or strangers whose "socio-emotional anchorages" are to other groups.

Cohen's evidence is a statistical manipulation of data from 65 societies, ranging from primitive foragers to modern townsmen and villagers. The cross-cultural method has inherent difficulties and in a concluding chapter he notes his awareness of some of them. The major problems are the selection of a sample from a universe of unknown dimensions and the determination of comparable units. His decision to select those societies for which he found pertinent ethnographic data sidesteps, perhaps reasonably, the methodological issue. But it leaves the reader less confident in results expressed as $p < .01$ or even 0.001.—*Michael M. Horowitz*

Optoelectronic Devices & Circuits by S. WEBER; 360 pages; $15; McGraw-Hill Book Co., 1964

The development of the laser has stimulated increased interest in the utilization of optical devices as integral parts of electronic systems for signal processing. Progress in this area has been quite rapid, making it difficult for the experimental research worker and the engineer to keep abreast of developments. The book under review may alleviate some of these difficulties. It comprises one very concise descriptive article on optical principles, which must be considered refresher material due to

its brevity, followed by reprints of 97 articles on optical-electronic devices, circuits and systems, arranged in 12 chapters, which have appeared in *Electronics* magazine in recent years. The article groupings deal, respectively, with: fiber optics and laser principles and design considerations; generation, modulation, and detection of light for communication purposes; military and space applications; applications of infrared; display systems utilizing electroluminescence; pattern recognition; applications to the computer functions of data storage, switching and counting; control of industrial processes; instrumentation for measurement purposes; and applications in television.

In general, the articles are very brief, many being sufficient for the expression of a single idea, and some are pure speculation. They are of the *how-it-was-done* variety rather than the *how-to-do-it* kind. However, many of the techniques described are very clever and individuals engaged in these specific areas should find useful ideas applicable to their own problems.

The dates of publication of the original articles are nowhere recorded in the book, which makes placing them in proper historic period difficult. This is mentioned since some of the ideas presented have become outmoded since publication. A greater than necessary number of typographical errors appear, but these should not cause the reader undue problems.—*Floyd Dunn*

Newcastle Disease Virus, An Evolving Pathogen, edited by R. P. HANSON; 352 pages; $7.50; The University of Wisconsin Press, 1964.

Newcastle Disease is an ailment of domestic fowl which is caused by a virus. The disease was first described in 1926 and since then has spread throughout the world. Because of its economic importance it has received widespread attention from veterinarians and others who are primarily interested in preventing outbreaks in commercial flocks. The virus is also widely studied because of its many interesting properties. The aim of the symposium and the book

was to look at a new disease in its entirety and to write the history of its evolution.

As in any compendium, the writing is very uneven and this is especially true in this case because the writers come from very different disciplines. There is a great deal of repetition and overlapping between chapters, not all of which is bad, if, for example, one were interested in getting extensive and conflicting views on the efficiency of vaccination. An attempt was made to cover the history of the disease from every possible angle. There is a chapter on the development of the modern chicken and one on the changing environment of the chicken which is followed by four papers on the history of the origin and spread of the disease. About 100 pages are devoted to the approach of the animal husbandman.

The central chapters of the book are devoted to descriptions of the virus itself and its behavior under laboratory conditions and this is followed by a section on the pathology of the disease; there are several papers on other myxoviruses. Fenner gave an excellent summation of the proceedings in a final chapter and compared the knowledge of the evolution of Newcastle Disease with the evolution of myxomatosis where it was experimentally introduced in wild populations.—*George K. Hirst*

Motivation: Theory & Research by C. N. Cofer and M. H. Appley; 958 pages; $12.50; John Wiley & Sons, 1964.

This book provides the serious student of behavior with a compendium of the facts and fictions about motivation. Quantitatively, the bibliography contains well over 2000 titles; qualitatively, the work is outstanding. These authors have read with understanding and written with clarity and insight.

Because motivation is not a content topic whose boundaries can yet be clearly delineated, the book includes essentially everything somehow motivational. . .drive and incentive, learned and unlearned, human and animal, conscious and unconscious, individual and social. It may seem strange to find topics such as hoarding, fatigue, and sex bedded down in a single chapter, but the authors have provided sub-summaries of each such topic and larger summaries justifying the structure they have imposed upon this heterogeneous field.

By way of conceptual integration, Cofer and Appley adopt an equilibration (homeostatic) model because they find it more general than a simple need-reduction model. Then, in place of a single goading mechanism based on drive stimuli, they propose two arousal-type mechanisms based on the interaction of organism and environment. Stimuli appropriate to a state of disequilibrium activate a sensitization-invigoration mechanism, and stimuli regularly antedating consummatory behavior activate an anticipation-invigoration mechanism. These are similar to mechanisms advanced by other contemporary theorists, but, in this book, they are suggestively related to the broad range of motivational topics considered. Overall, a very significant contribution.—*Frank A. Logan*

The Science of Smell by R. H. Wright; 164 pages; $4.95; Basic Books, Inc., 1964.

This is an interesting book on a subject of great importance in physiology. The author has brought together a wide variety of information on chemoreception in insects, fish, and mammals. There is some discussion of the importance in the life history of the organism, but largely it is concerned with problems of physiological mechanism. For instance, he examines the extreme sensitivity in many of the examples, the nature of the chemicals involved, the relation of the concentration to the response, and finally there is a good discussion of various theories of odor discrimination.

Although this book has a clear style, it suffers from one drawback. One is never certain for whom the book is written: parts of it are so elementary that even a freshman might be of-

fended, while other parts are rigorous going. The presentation would have been more successful if the author had set his sights on the level of the intelligent graduate student, for the many sections at this level are excellent.—*J. T. Bonner*.

Thermal Degradation of Organic Polymers by S. L. MADORSKY; 315 pages; $12.50; John Wiley & Sons, Interscience, 1964.

Although important papers appeared earlier, the start of systematic measurements here and abroad in the area of polymer pyrolysis may be traced back to about twenty years ago. The results, combined with the existing knowledge of hydrocarbon decomposition mechanisms, led to the formulation of a quantitative kinetic chain theory which accounts for the different degradation patterns observed in the low temperature region (200–500°C) for different polymer structures. One of the prominent centers of this activity has been the National Bureau of Standards, with which Dr. Madorsky continues to be affiliated after his retirement as a staff member.

The volume deals entirely with experimental aspects and results. After a consideration of various degradation stills and procedures for the measurement of weight loss as a function of time, the discussion turns to the results for specific polymer types. The information desired, but not always available, is the rate of volatilization and its temperature dependence, the chemical composition of the volatiles, and the rate of decrease of molecular weight. Experimental results are presented for polystyrene and substituted styrenes, polyolefins, halogenated systems, dienes and their copolymers, polyaromatics and cellulosic materials. Results and mechanisms are reviewed for both low and high temperature degradation, and form the basis for suggested structural correlations with thermal stability. However, no attempt is made to relate quantitatively experiment to theory.

Contrary to current restrictive trends in such monographs, the author includes an adequate and critical account of re-

search pursued elsewhere, besides presenting his own contributions. The literature references extend to 1963.

This volume has much to offer as a review of experimental literature to the kineticist who is concerned with chain and molecular decomposition mechanisms; to the investigator of synthetic methods for thermally stable structures; to the technologist in search of materials for specific applications; and finally, to the student desiring general information.—*Robert Simha*

The Matter of Mendelian Heredity by K. R. LEWIS & B. JOHN; 269 pages; $10.; Little, Brown & Co., 1964.

This book is for students having little prior aquaintance with genetics, but it is *not* a conventional text. It opens with a section entitled "Prelude" which summarizes what the authors consider important for an understanding of heredity. Two quotations from this section will serve to illustrate their opinions. First, the authors put the discussion of DNA and the chemical basis of heredity at the end of the book because, "just as we prefer a student who knows the relationship between meiosis and Mendelism to one who knows the cytological details of a division and the chemical structure of the spindle, let us say, so do we prefer one who appreciates the biological significance of the Mendelian discovery to one who 'knows' the detailed structure of a molecule—even the DNA molecule." Second, they caution the teacher. "Let him teach the older discoveries in the light of subsequent ones-not in their shadow." "That they are old does not make them fundamental, but they are fundamental even though they are old."

The book holds closely to the concepts set forth in the Prelude. Chapter one discusses the behavior of genes and chromosomes in meiosis. Mendel's results are considered in detail. Chapter two considers elementary statistics and probability; Chapter three, interaction between genes; Chapter four, breeding systems; and Chapter five, mutation, selection, and evolution. Chapter six is a curious interlude which deals with making chromosome preparations and the details of meiosis. Chapter seven considers the chemical basis of heredity and is not particularly enlightening.

Generally speaking, the book represents a thought-provoking and refreshing departure from the molecularly-oriented texts which are now in vogue and it should serve as a counterbalance to them.—*Nicholas W. Gillham*

Space Physics, edited by D. P. LEGALLEY & A. ROSEN; 752 pages; $25.00, John Wiley & Sons, 1964.

This unique book is a monumental compilation of scientific data accumulated by some 150 satellites and space probes launched by the United States prior to mid-1963. As such, it comprises an indispensable handbook for the specialists in many aspects of space exploration. The utility of the book extends considerably beyond this primary purpose, however. By imbedding the presentation of the data in a matrix of pleasant tutorial discussion concerning the means of acquisition of the data, their interpretation and significance, and their correlation with previous knowledge and with data from other space experiments, the authors have also provided a useful graduate text, and an informative, circumspect exposition for the non-specialist.

Specificially, the book consists of four major sections. The first describes the purposes and techniques of the space experiments, including some description of the individual space vehicles which transport them. The second section deals with the physical properties of the sun, the planets, and their atmospheres, including a detailed chapter on the upper atmosphere of the earth. The third section discusses the distribution and intensities of the interplanetary magnetic fields and solar plasmas, and the fourth section is reserved exclusively for the critical problem of high energy radiation patterns in space.

In general, the book is attractively presented. The figures and graphs are clearly displayed, as are most of the tables, although a few of the latter tend

to be somewhat ponderous. The Table of Contents is particularly clumsy and overbearing. The references are collected at the end of each chapter. and in most cases are quite complete.

One might logically question the exposition of this subject matter at a time when space research is just emerging from its infancy into a period of systematic study. A steadily growing number of unmanned probes and satellites are daily accumulating new data, some of which must supersede those presented in this book. As the most perspective of all diagnostic devices-man-injects himself more regularly into the space domain, the rate of data acquisition will grow exponentially. Nevertheless, this present book will serve well as the primary reference, on which subsequent, more technically detailed, but less tutorial volumes may be based.—*Robert G. Jahn*

Single-Crystal Films, edited by M. H. FRANCOMBE & H. SATO; 420 pages; $14.50; The Macmillan Co., Pergamon, 1964.

This book collects the papers given at an International Conference at the Philco Laboratories, Blue Bell, Pennsylvania, in May 1963. The conference was intended to stress studies on films in an optimal state of crystalline perfection, i.e., single crystals as free as possible of impurities, point defects, and dislocations. Since perfection of any kind is easier to specify than to achieve, most of the work actually reported dealt with experimental studies of epitaxial growth of films of metals, silicon and germanium, with assessment of imperfections by means of X-ray and electron diffraction and electron microscopy. This work should be of great interest to chemists and physicists who may wish to use these epitaxial films in their own work. One thinks immediately, for example, of the value of such materials in studies of surface reactions, contact catalysis, and electrochemical kinetics.

Several interesting theoretical developments are described in some detail. Van der Merwe gives an excellent account of the "surface dislocations" at the interface of a bicrystal, and

Mathews provides electron microscope views of these interesting imperfections in thin films of gold-silver alloys. Hirth, Hruske, and Pound review nucleation theory to reach the conclusion that atoms striking a substrate adsorb and thermally equilibrate before they move over the surface to join an embryo or nucleus.

The publisher has produced this book with unwonted elegance, so that the many beautiful electron micrographs retain their full clarity. In many of these we discover new evidence that Nature is one of the leading artists of the abstract impressionist school. The pictures obtained by Sella and Trillat of cleavage faces of rock salt decorated by metal evaporation are especially striking—whirls and arabesques with germanium nuclei, flowers in gold films viewed by transmission. Other fantastic forms are the "oriented wrinkles" observed by Yelon and Voegeli in annealed films of nickel on rock salt. One might conclude that not only every scientific library but also every library of fine arts should have a copy of this remarkable book. But the persons who need it most are those toilers in the field of surface chemistry who are not yet convinced that they must define precisely the surfaces on which they are working.—*Walter J. Moore*

Methods in Carbohydrate Chemistry, Volume IV: *Starch,* edited by R. L. Whistler; 335 pages; $13.50; Academic Press, 1964.

Sixty-one contributors have collaborated in preparing 75 articles on the chemical technology of starch as Volume IV of the excellent series "Methods in Carbohydrate Chemistry." Thus, the starch chemists now find themselves in the same favorable position as the monosaccharide chemists (Volume I and II) and their configurational counterparts, the cellulose chemists (Volume III).

The current volume is divided into six sections which fall into a logical pattern. Section I deals with the preparation of starch and starch fractions, and it would appear that the best methods are in fact presented for the isolation of starch from corn, wheat and potato sources. The section on chemical analyses is quite complete, dealing with analyses of whole and modified starches, starch fractions and starch hydrolyzates. Section III on physical analyses follows the same outline as the one on chemical analyses and deals with such physical characteristics as density, viscosity, mechanical properties, optical rotatory dispersion, diffusion, osmometry, and sedimentation. The next section on microscopy is quite short but includes both optical and electron techniques. Section V deals with the degradation of starch. Attention is given primarily to acid hydrolysis and enzymatic degradation; however, an article each on the acetolysis of amylopectin and the methanolysis of starch is given. It seems unfortunate that a discussion of alkaline degradation is not given here. The last section discusses the derivatives of starch. After some preliminary articles on reactivity, which are primarily concerned with various pretreatments to achieve greater reactivity, the remainder of the section deals with the formation of esters and ethers. Two articles are devoted to the oxidation of starch, including the preparation of the industrially intriguing dialdehyde starch.

As was the case with the previous volumes in this series on the entire field of carbohydrate chemistry, Volume IV on starch is a well-prepared and authoritative addition to the technological literature. All chemists dealing with starch will wish to add this book to their collection.—*Ludwig Rebenfeld*

Nutritional Factors in Virus Formation by K. Yamafuji; 128 pages; $7; J. B. Lippincott Co., 1964.

In this short book Professor Yamafuji (Kyushu University, Japan) attempts to summarize his hypotheses on nutritional relationships and virus formation. Observations made in higher animals are discussed and supplemented with extrapolations from insect and plant virus studies as well as 'phage systems. The book is divided into three main sections: "Chemistry of Virus Parti-

cles"; "Virus Formation in Relation to Cellular Metabolisms and Nutritional Factors"; and "Mechanism of Virus Induction." Each of these sections is ubdivided into a number of independent discussions. For example, one of the topics in the second section is entitled "Factors Relating to Nucleic Acid Metabolism" and subdivided into: (1) Deoxyribonucleic and ribonucleic acids (*a*) Animal Viruses (*b*) Bacterial Viruses; (2) Purines and Related Compounds (*a*) Effects on Virus Multiplication (*b*) Incorporation into Nucleic acids; (3) Pyrimidines and Related Compounds (*a*) Effects on Virus Multiplication (*b*) Incorporation into Nucleic acids; (4) Nitrite and Hydroxylamine (*a*) Virogenic action (*b*) Mutagenic action. 53 illustrations and chemical formulas are used together with 24 tables to provide substance for the author's conclusions. Included in the 283 references are 78 papers by Professor Yamafuji and associates (dealing with silkworm virus problems).

It is obvious that the original text was not written in English, and it is unfortunate that the translation was not carefully edited by someone familar with the subject matter. Professor Yamafuji has been able to carry out in his studies on silkworm viruses a number of experiments resembling those carried out using animal viruses and bacteriophage systems. He concludes that viral diseases can be regarded as the consequences of specific nutritional or metabolic disturbances, and maintains that the discovery that normal cellular metabolism produces intermediates with the ability to induce viral polyhedrosis by activating a chromosomal genome may afford promise of solving the important but difficult problem of the virosis-nutrition relationship.

Professor Yamafuji's book is one of the series "International Monographs. Aspects of Animal and Human Nutrition." The chief concern of this series is to study and show the nutritional environments best suited to a perfectly normal metabolism of the organism. Disease is considered as a consequence and not a cause of this disturbed metabolism.—*D. Perlman*

Differentiation & Development. Proceedings of a Symposium sponsored by the New York Heart Association; 172 pages; \$5.50; Little, Brown & Co., 1964.

This is an exceptionally good symposium on topics of current interest in development. It has the consistently high caliber of some of the early growth symposia and deserves widespread attention. The papers are by D. M. Bonner, P. R. Gross, W. Beerman, H. O. Halvorson, B. Mintz, D. D. Brown, R. L. DeHaan, and N. K. Wessells, with introductory remarks for the various sections by E. L. Tatum, A. E. Mirsky, C. L. Markert, and C. Grobstein.—*J. T. Bonner*

Unified Theory of Nuclear Models by G. E. BROWN; 178 pages; \$7.25; John Wiley & Sons, 1964.

The central idea of this book is that many of the models invented to explain quite different aspects of nuclear spectra can be usefully regarded as manifestations of the Hartree-Fock theory of self-consistent fields. Since all the nucleons contribute to the self-consistent field, collective nuclear motions will exhibit themselves especially vividly as a corresponding motion of the field. For example, Brown discusses rotational states of nuclei in terms of the theorem of Thouless and Valatin [1] that if the self-consistent field is non-spherical, the Hartree-Fock equations have solutions corresponding to slow rotation of this field. The various types of nuclear motion leading to vibrating self-consistent fields are discussed at length. This is reasonable, as Brown himself made some of the most important contributions to this subject. He first presents his and Bolsterli's [2] simple but strikingly successful model of the giant dipole excitation. Then he discusses vibrations in terms of the mathematical scheme alternatively called "time-dependent Hartree-Fock theory," "linearization of the equations of motion," or "randomphase approximation." The theory of pairing correlations is presented according to the Copenhagen prescription [3], in which the quasiparticle transformation is introduced in an attempt to generalize the seniority concept to a situation in which the nucleons move in non-degenerate single-particle levels. The last section of the book treats the optical model, especially the picture of Lane, Thomas and Wigner [4]. The treatment here follows Brown's *Revs. Mod. Phys.* article [5] on the same subject.

So much physics can be put into so few pages (172), because of the way the book is built around the single idea of the self-consistent field. This should also make it useful for the part of a graduate nuclear physics course dealing with nuclear models. However, although Brown has taken great pains to emphasize the physical ideas, the conscientious reader or student must be prepared to fill in many of the details for himself.—*B. F. Bayman*

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The Physical Chemistry & Mineralogy of Soils, Vol. I: Soil Materials by C. EDMUND MARSHALL; 388 pages; \$12. John Wiley & Sons, 1964.

A valuable contribution has been made to the soils literature with the publication of Marshall's book. Before the appearance of this volume the literature pertaining to soil chemistry and mineralogy consisted largely of a group of diversified publications in various journals and languages without a common denominator. Thanks to the efforts of Professor Marshall, this information is now drawn together in a concise,

scholarly fashion. Topics and references are judiciously chosen. The author's publications, together with those of Mattson, Wiegner, and others, are extensively quoted.

The first chapter deals with the general physical chemistry of the soil system. The next chapter, dealing with the mineralogy and chemistry of sand and silt, is a review of structures of various minerals and experimental findings under controlled conditions. After reading this chapter one may question whether the author actually got around to discussing the topic implied in the title. Following Chapter 2, Marshall really gets to the center of the problems and here he is at his best with chapters covering the physical chemistry of colloidal systems, adsorption, electrochemical properties of clays, ion exchange, physico-chemical fixation, and electrokinetics. These are considered the prime chapters and, in this connection, they stand out as probably the most important and authoritative work on the subject that is available today. Chapter 4, dealing with properties of organic matter, while perhaps meeting the intended needs of the text, does not give complete treatment to the subject. Of the 49 references in Chapter 4, only 1 postdates 1953. This suggests that Marshall had either written the chapter 10–12 years ago or research activities in this field have been at somewhat of a standstill.

Thanks to modern instrumentation and the efforts of many investigators, chemical and physical reactions of the soil are now understood a little better. The major hurdle of translating laboratory investigations to the natural soil system, however, still remains to be bridged.

The volume is an advanced and theoretical treatment of the subject. It is not a book on methodology and the author makes no claim to this effect. It will serve as a valuable source of information for advanced students, teachers, and researchers in soils, geology, mineralogy, and chemistry (P. 154, ref. 64, high field should read Highfield.)—*J. C. F. Tedrow*

Fabric & Mineral Analysis of Soils by R. BREWER; 470 pages; $15; John Wiley & Sons, 1964.

This book brings together and organizes information and concepts about micromorphology of soils, particularly as seen in thin section with the aid of a petrographic microscope. Here is an important textbook and manual on pedography which parallels books treating the older discipline of petrography. Procedures given in the appendix include those for hardening undisturbed soil samples so that they can be cut and ground like rock specimens. Most of the 133 figures are microphotographs taken in plain or polarized light of soil constituents and features in their natural arrangement.

Brewer proposes observation of soils at a number of levels of magnification using a hand lens in the field, a stereomicroscope with undisturbed monoliths, and a petrographic microscope with thin sections and grain mounts. The book builds an important bridge between macroscopic field description of soil profiles and the common laboratory analyses of mixed and sieved samples representing entire soil horizons. This bridge was well begun by Kubiena twenty years ago. Brewer has mastered the subject and enlarged it enormously. He reports the results of over a decade of research, much of which was done in the Division of Soils, Commonwealth Scientific and Industrial Research Organization, Australia. He presents a thorough summary of work of other investigators, as reported in the pedologic literature, and as reviewed by him with many workers in the course of his extensive travels.

The bulk of the book is devoted to the considerable task of naming and defining what one sees through the microscope in thin sections of soils. The observed features include such items as a clay-rich "s-matrix," a manganiferous concretion ("glaebule"), and a clay coating ("argillan") on the surface of a void where fine plant roots may be concentrated. An ambitious system of classification of attributes and features of soil structure is presented and succinctly related to some theories of their genesis. Laboratory experiments to produce artificial replicas of pedologic features are discussed briefly. Interesting research possibilities in the expanding field of pedography are indicated.

One could wish that in the next edition of the book all technical terms, many of which are new, were clearly defined and related. The proposal that geographic names be used in labeling variants of soil fabrics and s-matrixes might well be deleted.—*Francis D. Hole*

Advances in Child Development & Behavior, Vol I, edited by L. P. LIPSITT & C. C. SPIKER; 387 pages; $12.00; Academic Press, 1964.

Developmental psychology encompasses research on a wide variety of psychological and growth phenomena. In addition, it is concerned with methodological problems of developmental research as well as with the more mechanical aspects of obtaining and handling infants and children for use as subjects.

Because of the amorphous nature of the field and the wide variety of researchers who may at times be interested in working with children, the publication of handbooks has been relatively frequent. Handbooks, however, are terribly ponderous in style as well as mass, and, more importantly, the information they contain is often quite dated by the time it reaches the experimenter. Also, requirements for completeness typically force the contributors to cover classic chapter heading topics rather than more specific research or methodological issues.

A series such as *Advances in Child Development and Behavior*, by presenting in successive volumes critical syntheses of currently prominent research areas, can avoid many of the faults of the handbook and make a valuable contribution. The first volume has nine chapters, six of which were written by staff or former students from a single university. This limits the scope of the book to some extent. There are six chapters on various aspects of learning: three of these are on discrimination learning; one is on word

associations; one is on learning in infancy; and one is on the functional analysis of behavior. The other three chapters are concerned with physical growth, the development of perceptual constancy, and children's responses to novelty and complexity.

There is also considerable variability in breadth of coverage of the various chapters. Some are thorough and excellent reviews, others report primarily the author's research. Most of the chapters might have been presented, with only slight modification, as articles in existing journals. With the book selling for $12.00, many a reader might have preferred such an arrangement.—*W. E. Jeffrey*

Introduction to Properties of Materials by D. ROSENTHAL; 359 pages; $7.95; D. Van Nostrand Co., 1964.

The repeated demands upon engineers to produce systems to function reliably in environments previously unexplored by man makes the nature of Engineering Education an important factor in determining the useful life of an engineer. It is no longer adequate to base this education on an understanding of modern technology without the support of a knowledge of the fundamental atomic mechanisms underlying the properties of materials. The increasingly shorter time between the advent of a new concept in the fundamental research laboratories and its application in engineering products requires the engineer continuously to update his approach to problems. Certainly, this is made an easier task if he has been trained to reason from first principles. Professor Rosenthal's book provides an introduction to the properties of materials written with these problems in mind. The text proceeds through an introduction to atoms and the processes of crystal growth to the macroscopic, mechanical, electrical, and thermal properties of real materials. The dependence of these bulk properties on the atomic nature of the materials is examined and correlations between properties such as thermal and electrical conductivity pointed out. The text has a sufficient number of references to enable the

interested student to acquire the habit of independent search for understanding and contains good diagrams and photographs. The quantity and detail of the material covered should make the text suitable for a one semester course for engineering students in their Junior year.—*B. S. H. Royce*

The Conduct of Inquiry, Methodology for Behavioral Science by A. KAPLAN; 428 pages; $8.00; Chandler Publishing Company, 1964

Formal accounts of scientific methods, rendered by logician or experimental craftsman, often are light years away from the concerns and problems of day to day scientific practice. Large segments of the scientific community thus tend to view new methodological studies with cold indifference or overt hostility. Valuable methodological work, as a result, may be rejected even without benefit of cursory examination. Kaplan's account of scientific methods is one such work that behavioral scientists with responsible attitudes toward training can scarcely afford to ignore.

The range of the book is enormous. Scientific concepts, laws, measurements and statistics, theory and models are among the topics analyzed in depth. Yet, each is related to contemporary behavioral science with admirable skill. The author has succeeded where so many have failed for a variety of reasons. Among the most important is his insight that verification in science, as well as discovery, is not an entirely logical process. At the outset, he sharply distinguishes between actual scientific activity and idealizations or logical reconstructions of such. The author's constant emphasis on this distinction, although often subtle, clarifies much that is confused in current behavioral inquiry.—*Roy Lachman*

An Introduction to Radiation Chemistry by J. W. T. SPINKS & R. J. WOODS; 477 pages; $12.75; John Wiley & Sons, 1964.

The authors of this excellent introduction to a fast growing and important field of chemical research have succeeded in bridging the gap between the uninitiated scientist, engineer, or layman and the practicing radiation chemist. Both types of individuals will be interested in this book.

The specialist in this field will want this book on his own private bookshelf, for it serves as a quick and comprehensive reference to the important literature in the field up to late 1963. He will find more modern interpretations put on older data in the literature, as well as dispassionate, balanced treatments of areas of radiation chemistry where much controversy has existed. He also will find the book to be an excellent text, with problems, for a senior-graduate type course.

The layman will find many interesting features in this book, whether he is merely interested in "browsing" or trying to catch up with recent technology in radiation processing. If he is not caught up in the comprehensive chapters dealing with the fundamental aspects of the interaction of radiation with matter, ions and excited molecules, and free radicals he may examine what differences exist in the radiation chemistry of gases, solids, and liquids. Or he may wish to peruse the chapter on polymerization induced by ionizing radiation. The chapters on sources of radiation, interaction of radiation with matter, and radiation dosimetry are recommended reading for the individual who wishes to be able to cope with and understand more thoroughly the ionizing radiations which play an increasingly important role in his civilization.

Due to the rapid progress made recently in certain areas (e.g., pulse radiolysis), certain sections of the book are now out of date. Nevertheless, in the opinion of this reviewer, this volume remains the best single comprehensive treatment of radiation chemistry available today.—*C. N. Trumbore*

William Herschel & the Construction of the Heavens by M. A. HOSKIN; 199 pages; $6.00; W. W. Norton, Inc., 1964.

William Herschel himself is the guiding spirit of this volume. At least two-

thirds of the text is Herschel's and is comprised of excerpts from various papers which appeared from 1783 to 1817. Michael Hoskin has devoted his own commentary to analyzing Herschel's contributions in the light of the evolution of astronomy during that period. He has also enlisted the aid of Dr. D. W. Dewhirst of the Cambridge Observatory, who has gone through the laborious and necessary task of identifying most of the stellar objects described by Herschel, and of giving their present designations.

Herschel's abilities, idiosyncrasies, and wide range of thought have long been known. The present work nicely shows his intellectual development with respect to his theories of the stellar system. Most impressive is the record of Herschel's struggle to be an empiricist, to refrain from hastily leaping to conclusions on the basis of insufficient data. In spite of his professed caution, Herschel simply did not operate that way. Mr. Hoskin shows how Herschel's grand hypotheses not only directed his investigations, but also often caused him to close his mind to "inconvenient facts," even when he was the first to notice the pesky things. The deliberate selection of observations in order to shore up his theories was a mark of Herschel's genius, which should be another warning to anybody that might still think of science as a well-ordered development according to fixed rules of method. Indeed, at one point Mr. Hoskin declares that Herschel's achievement "was more brilliant in conception than in execution," which is certainly not the usual comment on the great astronomer.

Covered sequentially are Herschel's work on double stars and the motion of the sun, the construction of the heavens (first synthesis), the construction of the heavens (second synthesis), and the last effort of 1817 to view the heavens in three dimensions. Mr. Hoskin has produced an excellent monograph—the kind that the historian finds so useful and so necessary.—*J. Morton Briggs, Jr.*

Fracture Processes in Polymeric Solids (Phenomena & Theory), edited by B.

ROSEN; 835 pages; $27.50; John Wiley & Sons, Interscience, 1964.

The editor and authors have made a valuable contribution in preparing a volume providing a much needed exposition of present knowledge of fracture processes in polymeric solids. The aim of the book is twofold. "(1) to bring the investigation of fracture processes in other materials into contact with the concepts, ideas and results of the work of polymer scientists whose main interests are the relations between chemical structure and mechanical behavior, and (2) to familiarize the polymer scientists with the thinking, theories, and results of physicists and engineers who approach fracture as a feature of the solid state or as a limit to material performance."

In order to accomplish this aim, the editor had to solve a formidable problem in selecting the topics relating to various aspects of fracture processes. The broad scope of the book is indicated by chapter headings: Polymeric Solids, Brittle-Like Fracture, Time-Dependent Fracture Processes, Failure Processes, and Supplementary Contributions. The first chapter offers a clear exposition of basic concepts of polymer morphology, physics and mechanics. It is followed by a review of Griffith's theory of strength of brittle solids. The subsequent presentation of experimental data concerns the effects of temperature, molecular size, and orientation. Although these variables have an important role in fracture process, a theoretical treatment relating them to the observed effects had not been proposed up to the time this volume was compiled.

Glasses and amorphous unfilled polymers are the subject of discussion in the chapter on time-dependent fracture processes. Progressive weakening of glasses is treated primarily from the phenomenological standpoint. The treatment is therefore concentrated on crazing and related effects. Viscoelasticity and time-temperature superposition are emphasized in treatments of fracture of rubber-like solids. Special topics such as cutting, effect of molecular orientation, morphological aspects of deformation, role of chemical changes in fracture, etc., are discussed in Chapter IV. "Supplementary Contributions" include an interesting theoretical derivation of a distribution function for fragmental size which is based on Griffith's model of strength in conjunction with a theory of probability. It is followed by a review of experimental methods and results. A discussion of the effects of stress raisers and plane waves in non-linear viscoelastic materials completes the volume.

This book, which offers a competent survey of literature, was written with the aim of exploring the applicability and limitations of Griffith's concept for polymer solids. The concise and stimulating coverage may well initiate new investigations in this young branch of material science. The volume will, without any doubt, be of considerable value to both engineers and polymer scientists interested in this field—*Dusan C. Prevorsek*

Space Exploration, edited by D. P. LEGALLEY & J. W. MCKEE; 467 pages; $17.50, McGraw-Hill Book Co., 1964.

This book is a fifteen-chapter record of a statewide University of California Extension lecture series on a variety of astronautical subjects. The lectures generally must have been very interesting and profitable to the audience. The reviewer, however, questions the necessity or desirability of their being recorded in book form as a contribution to the literature of astronautics.

Most of the contributors have attempted to survey the current status and prospects in their fields of interest. These lectures, in the main, are up-to-date as of the end of 1962, but astronautics is a fast-developing field and the eighteen-month publication delay has dated some of the presentations. Few lectures read well without some extensive re-writing, and despite the time between presentation and publication this necessary task seems generally to have been shirked.

This reviewer does not wish to belittle the technical achievement of the authors. Most of the chapters contain interesting and informative material

But the literature of astronautics would have been better served by their separate appearance in more polished form in the specialist journals, or in the broader context of such periodicals as *Astronautics and Aeronautics*, *American Scientist*, or *Scientific American*. The one exception to the generally high technical level of the book is the last chapter entitled "Technological and Economic Impact of Space Exploration" in which this important topic, objective treatment of which has been largely neglected, is handled in a trifling fashion.

The book covers an enormous range of topics: "Space Nuclear Propulsion," "The Environment of the Planets," "Astrodynamics," "Meteorological Satellites," to name a few, but fails to achieve the comprehensive scope of the earlier volume of similar origin, "Space Technology" (Wiley, 1959). The latter, along with the valiantly attempted "Handbook of Astronautical Engineering" (McGraw-Hill, 1961), by virtue of their comprehensiveness, are good candidates for the personal bookshelf. "Space Exploration" seems more the responsibility of the libraries. The libraries should not have to carry this type of load in astronautics much longer. The discipline is well established. Good surveys and reviews belong in periodicals, where their appearance is most welcome. A book in this area should now be more than a mere collection of articles. "Space Exploration" is not.—*Neville A. Black*

Chemical Reactions of Polymers by E. M. Fettes; 1304 pages; $42.00; John Wiley & Sons, 1964.

A characteristic of high polymer science is that very minor side reactions which occur accidentally or purposefully after the polymer has been synthesized cause very major effects on the final properties of the polymer. For example, a small amount of oxidative scission or cross-linking is of overriding importance in the long-term behavior of polymers with respect to resistance to aging and weathering. A minor amount of chemical crosslinking converts a thermoplastic substance to a thermoset substance. An excellent example of the latter is the vulcanization of rubber with sulfur and accelerators. High energy radiation also causes cross-linking and scission of polymers.

Low molecular weight linear polymers can be chain extended to high molecular weight materials by means of difunctional and polyfunctional reagents. Interesting examples of this type are the urethane prepolymers and the epoxides and the liquid polysulfide polymers. All of these reactions and many, many more are of great industrial and theoretical interest. The understanding of these diverse reactions invokes a very broad range of chemistry.

The editor of this book has obtained some very capable authors, expert in the various fields, to write sixteen self-contained chapters on important chemical reactions of polymers. The book will be very helpful as a general reference book and the individual chapters will serve as very useful points of departure for research workers entering these various fields.—*Arthur V. Tobolsky*

Experimental Entomology by K. W. Cummins *et al.*, 176 pages; $6.50; Reinhold Publishing Corporation, 1965.

The Class Insecta contains over one million species and shows more variety of form, habitat, and life history than all other animal classes combined. This animal omnibus has baffled most efforts to teach insect biology, which have largely fallen back on comparative morphology and systematics to give students a "feel" for insects. At least three bulky books on insect physiology are available, but they are not quite suitable to provide a biology student with his first scientific acquaintance with living insects. This little book attempts to establish such a contact, and does it in a fresh and interesting fashion. It is intended to serve as a laboratory companion to *Introduction to Comparative Entomology* by R. M. Fox and J. W. Fox (Reinhold), and gives instructions for laboratory exercises in morphology (25 pages), systematics (25 pages), genetics (9 pages), physiology (41 pages), behavior (16 pages), and ecology (26 pages). In addition there is

an appendix on methods for raising live cultures, and making media and special equipment. Such space limitation could not but result in a treatment that is often thin and one-sided (in the direction of respiration and caddis flies). Nevertheless, the writing is concise and clear, and many of the experiments show novelty and freshness and bring the student into brief contact with a variety of up-to-date experimental techniques not usually associated with undergraduate laboratory work on insects. References are plentiful and each experiment implies that it is a departure platform for more intensive and detailed experiments. Although the price is steep for a book of this size it should bring refreshment to traditional college laboratory exercises on insects.—*Kenneth D. Roeder*

Bituminous Materials: Asphalts, Tars, and Pitches, Vol. I, edited by A. J. HOIBERG; 432 pages; $17.50; John Wiley & Sons, Interscience, 1964.

"Bituminous Materials" is to be in three volumes with "Asphalts, Tars, and Pitches," covering fundamental composition, nature and properties. Volume 2, "Asphalts," now in press, and Volume 3, "Manufactured Tars and Pitches," in preparation, will cover all phases of these topics, including manufacture, marketing, properties and usage, according to the publisher.

The volumes are compilations of monographs by specialists, short biographies of whom are included. Each monograph is preceded by an outline and finishes with a bibliography.

W. E. Hanson provides an analysis and discussion of the confused terminology which has become associated with bituminous materials.

The review of the physical chemistry of bituminous materials by C. Mack is the longest chapter. An appreciable part of it is devoted to a mathematical, theoretical discussion of the thermodynamic properties of bituminous materials as a function of their molecular structures. The section closes with an extensive and valuable discussion of the adhesiveness of bituminous materials as related to their surface properties.

The discussion on compatibilities of bituminous materials by G. L. Oliensis deals with saturated and coated roofing materials. Little attention is given to incompatibilities on mixing one bitumen with another.

The chapter by R. N. Traxler on rheology is concerned primarily with asphalt paving materials. Rheological modifiers other than elastomers are covered only by a discussion of mineral fillers in asphalt paving mixtures.

G. Abson and C. Burton present a classified compilation of descriptions of American methods of test for bituminous materials limited to the fields of highway and building construction.

The effects of atomic radiation on bituminous materials, largely the work of the AEC, are summarized by C. D. Watson and W. W. Parkinson.

R. W. Traxler presents an informative introduction to the action of microorganisms on bituminous materials with attention to the methods of investigation which have been developed.

The last two chapters on inorganic fillers and elastomers as modifiers of bituminous materials are written from the standpoint of practical application to the roofing and road construction industries.

This work will inevitably be compared with Abraham's "Asphalts and Allied Substances." It differs in scope and treatment from the corresponding sections, does not have the homogeneity of Abraham, and does not carry the wealth of literature and patent references. Final evaluation of Hoiberg must await the availability of all three volumes.—*Gilbert Thiessen*

Isotopes in Biology by G. WOLF; 173 pages; $2.45 paper; Academic Press, 1964.

George Wolf has written an excellent, albeit elementary, book on the uses of radioisotopes in biology and chemistry. Most appropriately, he deals with the uses of labeled compounds in biochemical investigations on both a classical and contemporary level. This book will be easily understood by the scientifically oriented college undergraduate, as well

as by the more advanced science graduate student. However, the text is clearly and succinctly written, so as to be comprehended by a sophisticated high school student; and one might well find a useful employment of this paper-back among the various high school honor programs.

Obviously, the author, having both a facility for pedagogy and a personal experience in isotope biochemistry, has aptly set out to print many of his lecture notes in this area. In being so directed, this book deals lightly but understandingly with the physics and chemistry of isotopes, which includes the necessary basic "vocabulary" as well as the units and parameters for isotope work. The most significant portion of this book is in Dr. Wolf's treatment of methodology, and principles of isotopic investigations in biology. It is here, as well as in the applications of isotope techniques in research, that his book excels. The author selects many valuable and significant uses of isotopes in biochemical research, and includes recent bibliographical references to many exciting experiments which have used radiochemicals.

The book would be a worthwhile addition for any graduate level course in molecular biology, as well as a book to introduce some young scientific novice into the applications and principles of isotopes in biology and biochemistry. Unlike most books of this type, it is not a classically theoretical discussion of radiation chemistry, nor is it concerned with details of instrumentation and sample preparation. While research-oriented, this book does not deal with any significant amount of the voluminous material available on radiation effects in living tissue. Aside from these, the book definitely has its usefulness, to be strongly recommended for the initiation of students into the wonderful world of isotopic research. The price also makes this attractive.—*Richard Ascione*

Principles of Radiation Protection by G. Eaves; 185 pages; $8.25; Gordon & Breach, 1964.

This straightforward British text, while primarily intended for the use of reactor physicists having no extensive biological background, might well prove to be of use to the biologist or chemist working with high levels of radioisotopes. The author omits an extensive physical and mathematical discussion of nuclear isotopes and for this reason makes the theoretical principles behind radiation protection extremely understandable for the chemist and biological investigator. It is to the nuclear physicist, though, that the first chapters are directed in the simplified and elementary discussions of the biology of cells and tissue radiation damage, as well as the specific hazards of working around nuclear reactors. This portion might not be of immediate and obvious use to the biological and chemical research worker; however, the book is useful as a handbook or guide to understanding and applying the principles of radiation protection which would be of specific use for those among us who deal with significant amounts of γ and strong β isotopes. One would certainly consider this text to acquaint isotope research personnel as well as those involved in health physics investigation work. While the cost of this book and its limited applicability might rule out large numbers of individual purchasers, this book should be found among libraries of institutions where isotopic investigations are being conducted. Needless to say, one would also recommend this primarily as a supplementary text for the nuclear reactor technologist.—*Richard Ascione*

A Handbook of Practical Amateur Astronomy, edited by P. Moore; 254 pages; $5.95; W. W. Norton & Co., 1964.

Mr. Moore has edited contributions from very experienced amateur astronomers in the British Astronomical Association. Many people spend years in perfecting a useable small telescope, then wonder what to do with it. This book reviews the possible applications which lie within the scope of such amateur equipment, written by those who know. It is only a beginning survey of such activity, however, by which a new-

comer to the field may find something stimulating. He will then find it necessary to read other books on his special topic of interest (if they exist), or to consult the literature in a good library.—*K. L. Franklin*

Astronomy & Space Research by G. A CHISNALL & G. FIELDER; 230 pages; $5.95; W. W. Norton & Co., 1964.

This is a very quick review of just about everything one should know to be well-versed in the fields mentioned in the title. The dust jacket states that "Some knowledge of physics and mathematics is assumed, but no more than that possessed by the average intelligent reader." This amount seems to be a little facility with integral calculus and an awareness of differential equations. The introduction of mathematics, where useful, allows the authors to present their subjects a little more deeply than one might think in a small book of such scope. No one, however, will become even an amateur space scientist from this book (it is one of a series called "The Amateur Astronomer's Library"), but he will surely know what to study.—*K. L. Franklin*

Introduction to Infrared & Raman Spectroscopy by N. B. COLTHUP, *et al.;* 511 pages; $12.00; Academic Press, 1964.

In their preface to this book, the authors state that it "has been written for the student or the organic or analytical chemist who does not feel qualified to call himself a spectroscopist." However, the development of the theoretical background is somewhat sketchy and phenomenological, so that the book does not properly serve as an introductory text. The correct results are given, and other sources where they are derived in greater detail are cited. Although the chapter in experimental techniques is necessarily incomplete (for indeed there are many books entirely devoted to technology), it provides an excellent background for the novice, and contains a very good list of references. An extremely useful table, showing the symmetry classification and literature source for almost every molecule analyzed through 1960, is found in Chapter 3, providing a most welcome supplement to Herzberg's 1945 tabulation.

The greatest portion of the volume is devoted to spectra-structure correlation; and here the authors have done an outstanding job. The vibrational analysis is broken down not only into functional group frequencies, but, further, into the particular type of molecule being considered, neighboring atoms, electrostatic effects, etc. The tables are clear; the examples illustrate the more subtle details nicely; the references are excellent. In Chapter 13 the roles are reversed and correlation is made according to spectral region, including the near infrared overtone region and the lower frequency cesium bromide region (700–250 cm^{-1}).

In sum, although the book can not be used as an introduction to spectroscopy without complementary material, as an analytical tool for the chemist primarily interested in characterization or correlation it is unsurpassed.—*George E. Leroi*

Biostatistics: An Introductory Text by A. GOLDSTEIN; 272 pages; $9.50; The Macmillan Co., 1964.

This textbook is written from the point of view of application. The author, a professor of pharmacology, has had extensive experience in teaching biostatistics to students of medicine and biological sciences.

The book is divided into four chapters. The first entitled "The Logical Basis of Statistical Inference" provides a general idea of what the subject is about. It includes some discussion about experimental design, statistical hypothesis, and decision rules.

In the second chapter, entitled "Quantitative Data" the author first discusses the normal population, the mean, the variance, and how to estimate the mean by a confidence interval. He then proceeds to problems involving two samples, and then to problems involving more than two samples. Analysis of variance, including one-way classification, randomized block, factorial, and nested

designs, is presented. Some simple nonparametric techniques are also briefly treated.

The third chapter entitled "Enumeration Data" discusses binomial probabilities, the chi-square test for contingency tables, and the Poisson distribution.

The fourth chapter is entitled "Linear Regression." It includes not only estimation of correlation and regression line but also some applications to biological assay. The dose-response relation, normal equivalent deviates, probits, and parallel line assays are among the topics discussed.

In each of the four chapters there is an abundance of examples interwoven throughout the presentation to illustrate the concepts introduced and discussed. At the end of each chapter are about twelve problems the solutions to which are sketched at the end of the book. A shortcoming, from the point of view of class room use, is that there are no problems without solutions.

As an introductory text, this book gets into the depths of the subject more quickly than the ordinary elementary textbook. This remark is not intended as criticism, however, because graduate students and medical students are more mature and more highly motivated than undergraduates. As a book for undergraduates it may prove more difficult, but one really never knows how successful or not a book is for classroom use until it has been tried.—*John Gurland*

Electronic Analog & Hybrid Computers by G. A. Korn & T. M. Korn; 584 pages; $17.50; McGraw-Hill Book Company, 1964.

Those familiar with the other writings of this husband-and-wife team will expect of this book just what it is: a comprehensive and authoritative treatment. The work was evidently inspired by the modern developments in high-speed iterative computers and hybrid computers, which occupy a considerable portion of the book.

There is a wealth of practical detail, especially on the circuitry and design of components, unlike the majority of computer books which are concerned mostly with characteristics from the user's rather than from the designer's viewpoint. That is not to say that this book is of little value to the computer user; he will just find more detail on components than is often of concern to him. As well as presenting techniques for more sophisticated users, the book includes an adequate portion on basic techniques. Amplitude scaling is done in terms of volts because this is most common, but the authors note that scaling in terms of machine units (as preferred by this reviewer) has decided advantages.

A large section of the book is devoted to computer elements, ranging from capacitor dielectrics to a great variety of amplifier circuits. Other devices such as multipliers and function generators are of course considered, again in considerable detail. As would be expected from the book's title, no mention is made of mechanical or fluid computing elements, except in connection with computer servomechanisms. Of most interest to many readers will be the material on hybrid computation, and on system optimization using fast iterative computation, containing much information that this reviewer has not previously seen in print, and which certainly has not appeared in other books.

Well illustrated, amply indexed, and with a large bibliography, the book is warmly recommended to anyone concerned with analog computation.—*Peter L. Balise*

Dislocations by J. Friedel; 491 pages; $17.00; Addison-Wesley Publishing Co., 1964.

Although dislocations in crystals were described in the 1930's by Taylor, Orowan, and Burgers, and the theory of dislocation contributions to plastic properties underwent considerable development in the next decade, it was not until Frank observed growth spirals on crystal surfaces in the late 1940's that the theoretical and experimental study of dislocations became the most fashionable subject in metal physics and physical metallurgy. It is strange that this field should produce so few outstanding

monographs in English suitable for use as a textbook. There have been frequent review articles, as one expects of so active a subject. Perhaps the excellent monographs published by Read and by Cottrell in 1953 simply discouraged competition. Whatever the reason, this elegant book by Professor Friedel fills the need for a reasonably up-to-date text for graduate courses.

The original French edition of 1956, translated by L. F. Vassamillet, has been revised by the author. Notably it takes cognizance of many of the fascinating observations by transmission electron microscopy that have been made in recent years as a result of greatly improved instruments and techniques. The increased understanding of the interactions of dislocations with point defects, with other dislocations, and with the underlying crystal structures are reflected here. Impurity hardening, work hardening, and network theory are discussed at length.

In so intensely active a field no author can anticipate the many new advances that will be made even during the time required for the publishing process. However, this book should wear well as a textbook if judiciously supplemented by reports on current events. The discussion is notable throughout for the frequent mention of the physical magnitudes of the quantities employed in the theory or indicated by experiment. The illustrations are excellent, including 30 plates (unpaged) showing photo—and electron transmission micrographs. Almost everyone interested in the mechanisms of deformation of crystals will want to have this book near at hand.—*C. E. Birchenall*

Progress in Solid-State Chemistry, Vol. 1, edited by H. Reiss; 536 pages; $17.50; The Macmillan Company, Pergamon, 1964.

The inauguration of a new hard-back review journal calls for some comment about its general contents in relation to similar series. The articles in this volume should each be compatible with the subject matter in one of three existing series: *Progress in Materials Science*, *Solid-State Physics*, or *Progress in Inorganic Chemistry*. Indeed, similar articles can be found in those journals. Nevertheless, the rate of investigation of the topics covered may justify expansion of the review publications at this time.

One disadvantage of this kind of collection is the wide diversity of subjects. The Thermal Expansion of Ceramic Crystals (by H. P. Kirchner), Lattice Energies and Related Topics (by M. F. C. Ladd and W. H. Lee), Phases with the Nickel Arsenide and Closely-Related Structures (by A. Kjekshus and W. B Pearson), Lattice Imperfections and the Thermal Conductivity of Solids (by D. Greig), The Relationship of Photoluminescence and Electroluminescence to Structure (by D. W. G. Ballentyne), Ferrielectricity in Crystals (C. F. Pulvari), Alloy Semiconductors (by J. C. Wooley), Physico-chemical Aspects of Organic Semiconductors (by H. A. Pohl), X-ray Diffraction Studies of Crystal Perfection (by L. V. Azaroff), Applications of Nuclear Quadrupole Resonance (by G. A. Jeffrey and T. Sakurai), Use of Infrared and Raman Spectroscopy in the Study of Organometallic Compounds (by D. K. Huggins and H. D. Kaesz). It is most unlikely that a single reader will have enough interest and background in every topic to appreciate all the work that has gone into these condensed accounts. However, I enjoyed particularly the extended article on the properties of delightful phases with the nickel arsenide structure and the relatively brief article on X-ray methods for the study of crystal perfection. I hope that future volumes in the series will maintain the good proportion of experimental observations among the theoretical considerations that this collection displays. There are also many tables which should be useful.

The volume is attractively printed but not securely bound. Typographical errors are present but not in greater concentration than normal for such ventures.—*C. E. Birchenall*

Stochastic Models in Medicine & Biology, edited by J. Gurland; 393 pages; $6.00; The University of Wisconsin Press, 1964.

The chief, if not only, way that we understand the natural world is by the construction of models and it is certain that the single, most powerful influence on the nature of scientific theories for the last three hundred years has been Descartes' machine model. Particularly in biology, the *bête machine* has been the guiding metaphor of research and theory. It is somewhat ironic that, at the time of Descartes' death, the foundation of an alternative world view was being laid in the *"De alea geometriae"* by Pascal. That alternative, in which chance events are of equal importance with deterministic mechanical forces, has slowly come to pervade scientific theory, beginning with the probabilistic reinterpretation of thermodynamics and entering the field of biology more than forty years ago. More and more biologists are turning from models in which outcomes are exactly predictable from initial conditions, and are including in their models a stochastic element that introduces uncertainty as to the state of a system, even when the initial conditions are completely specified.

The Symposium on "Stochastic Models in Medicine and Biology" is a wide-ranging sample of the kinds of biological problems that have been formulated in stochastic terms. Quite appropriately, it includes an historical contribution on the early use of stochastic models in epidemiology and carries this subject up to the present time with a very general treatment of epidemics by Neyman and Scott. The second major field in which stochastic models have been of great importance in biology is that of population genetics and evolution, and, appropriately enough, an excellent review of this field is presented by Sewall Wright. In addition, the two directions in which stochastic theory of evolutionary processes is proceeding are represented. On the one hand, there is currently an attempt to derive with utmost rigor some of the results previously attained by Wright and others. Because of the demand for such rigor, only the simplest models can be attacked, and a review of the work is presented by Karlin and MacGregor. On the other hand, the treatment of really complex population phenomena may defy even approximate mathematical treatment so that computer simulation is the only way to get answers. An example of this numerical approach is presented by Schull and Levin.

What is most interesting about this Symposium is that the circle has been completed, and that stochastic models are at last being applied to physiological problems—problems for which Cartesian mechanism was first invoked. The symposium contains papers on carcinogenesis by Arley, arteriosclerosis by Opatowski, drug dosage by Cochran and Davis, general mortality by Chiang, enzymatic reaction by Bartholomay, and a general paper on oscillatory systems by Norbert Weiner.

The last paper in the Symposium is a very general view of stochastic and deterministic models by Henry Lucas, in which the basic assumptions and problem of stochastic models are very well brought out.

There is a certain unity about this Symposium that is lacking in so many such collections. It provides an excellent view of what has been accomplished in the past by the use of stochastic models, what some of the problems are, and, in particular, what a variety of problems can be and ought to be framed in probabilistic rather than ballistic terms.—*R. C. Lewontin*

Mammalian Protein Metabolism, edited by H. MUNRO & J. B. ALLISON Vol. 1; 566 pages; $18.50; Vol. II; 642 pages; $21.; Academic Press, 1964.

It is difficult to think of two men more suitable to prepare a compendium on mammalian protein metabolism than Prof. Munro of the University of Glasgow, Scotland, and Dr. Allison of the Bureau of Biological Research, Rutgers. The result of their editorial collaboration is a notable achievement.

The concept of the role of protein in nutrition has undergone a revolution in the last two decades. As the authors state in their preface, an international meeting on nutritional deficiency, held shortly after World War II, did not include protein on its agenda but, by

1960, the Director of the Nutrition Division of the Food and Agriculture Division of the U.N. stated that "... protein malnutrition is without doubt the most compelling nutritional problem in the underdeveloped countries today."

After a fascinating introduction, taking the reader from the discovery of nitrogen to the modern concept of the structure and metabolism of protein, the editors conclude that protein study demonstrates decisively "that single-minded devotion to a subject is more profitable than dilettante exploration over a wide field in the hope of some chance discovery of importance." The present-day body of knowledge of protein chemistry is the result of dogged persistence by many, in a complex field where each bit is painstakingly teased out and fitted into the overall jigsaw.

Drs. Allison and Munro cover the subject in three broad divisions, namely, the biochemical, the nutritional, and the pathological aspects of protein metabolism. This is a logical presentation and makes for easy reference as well as for continuity in straight reading. In addition to their own considerable contribution, the editors have included chapters by 27 of the world's leaders in the study of protein metabolism. Each chapter is amply documented with references as up-to-date as can reasonably be expected for a work of this type.

For the biochemical aspects there are Gitler, Krebs, Korner, Neuberger, Bird; for the nutritional, Harper, Hegsted, McCance, Widdowson; and for the pathological, Arroyave, Holt, Scrimshaw naming only a few to emphasize the caliber of the contributors. The whole is blended by skillful editorial work into a smooth, easy-reading treatise.

Typographically the book is excellent. It is printed on good stock in easy-to-read type and the tables, line drawings and photographs are clear and well reproduced.

In such a dynamic field, there can be no final and complete work. These two volumes, however, come as close as possible. It will be a long time before they are superseded. Let them stand as a fitting memorial for Jim Allison whose passing has left a void which cannot be filled.—*David B. Sabine*

Behaviorism & Phenomenology: Contrasting Bases for Modern Psychology, edited by T. W. WANN; 190 pages; $5.00; The University of Chicago Press, 1964.

Contemporary psychology has moved and is moving rapidly away from its one-time emphasis on "systems." Nevertheless, some concern with ideological issues is still with us. The present symposium deals with two of the most active of the classical systems.

This small volume contains much that is old—already published in one form or another–but also a significant amount of new material. As the editor remarks, the original intention was markedly altered in the execution of the project. The direction of this change is neatly summarized by his whimsical suggestion in the preface of a new subtitle, "Complementary Bases for Modern Psychology, with One or Two Dissents."

Perhaps the most interesting facet of this rapprochement is the relationship between Skinnerian behaviorism and Rogerian ego psychology. The philosopher Malcolm, contributing a brief but incisive critique of behaviorism, confesses that Rogers' position, as expressed in the book, is puzzling to him. This is mainly because Rogers holds, on the one hand, that Skinnerian behaviorism cannot deal with the central problems of ego psychology, but on the other hand agrees that this world can be opened to scientific observation by means of external, behavioral indicants (presumed correlates of inner experiences). It is therefore difficult to see why he, nonetheless, continues to deny the feasibility of their functional analysis.

The remaining sections may be briefly characterized. Koch's turgid, essentially negative treatment of behaviorism offers little new or refreshing, and one may hope that some day this author will share with his readers more detailed documentation of his persistent lauding of revolutionary developments in the methodology of physical science, said to have great implications for psychology.

(Presumably he refers to somewhat the same phenomena which Skinner refers to as a "muddle.") MacLeod presents a provocative and quite readable treatment of phenomenology, and Scriven's chapter is a wide-ranging essay on philosophical-logical issues.

Finally, credit is due the editor for his paraphrasing some interesting post-lecture interactions of speaker and audience (even if the exact question was not always caught). More of this sort of thing should be done by editors of such symposia, if they are to serve most fully in published form, even if some trivia are necessarily included.—*Melvin H. Marx*

Primary Embryonic Induction by L. SAXEN & S. TOIVONEN; 271 pages; Logos Press Ltd. in association with ELEK Books Ltd., 14 Great James Street, London W.C. 1; published in the United States by Prentice-Hall, 1962.

Embryological research is, at the moment, fragmented. A gap is present between those modernists who owe much to molecular genetics and quantitative biochemistry and those somewhat more traditionally inclined. This book attempts to bridge the gap and, in a major sense, succeeds. The subject matter of the function of Spemann's primary organizer is treated selectively, albeit in depth, in terms of modern embryology.

Selectivity is, of course, required of the authors of a monograph of the size of this volume. To be sure, every embryologist interested in the advance of his field will find some fault with the topics selected. Also, he will not view a straight path from Spemann to the present in the same context as do the authors. To the reviewer's mind, their path is to their great credit, not because it is the only one, but because they have brought intensive thought to the problem and have winnowed the vast mass of material on embryonic induction in an attempt to analyze what is cogent today, for the progress of present day analysis, from collateral material important in itself but not necessarily cogent. In this sense then, the text is modern. Selectivity is its key. I would not want the serious young embryologist to spare himself Spemann's original text nor such invaluable compendia as Needham's *Biochemistry and Morphygenesis*. The present book is in part an extract and in part an analysis in the face of more modern data, of what appears to the authors as essential.

One of the primary benefits of the text is its treatment of embryonic induction as a phenomenological event in embryogenesis, not as due to a unitary agent or master molecule. Much recent research is focusing anew on the molecular events of late blastulation and gastrulation, deriving much benefit from more precise methodology and analysis. To those in the field, the book is a reminder of brilliant deductions from an earlier period, repeatable and sound, which must be considered as the new biology is applied to morphogenesis and differentiation.

The text is well translated into colloquial English. It is remarkably free from errors. The index is functional and efficient. References are clear.—*Charles E. Wilde, Jr.*

Intersexuality in Vertebrates Including Man, edited by C. N. ARMSTRONG & A. J. MARSHALL; 479 pages; $14.; Academic Press, London, 1964.

This book provides a valuable service in making available, in English, a compilation of the studies on intersexuality in all the classes of vertebrates.

The authors, who are noted workers in a particular field or with a class of the vertebrates, discuss the following topic in detail: The role of the chromosomes in determining sex. For most classes, the embryology and morphological development of the reproductive tract is reviewed. This provides a proper perspective for interpreting both the "natural" and experimental attempts to induce intersexuality. A final section is included on intersexuality in man.

Two important contributions of this book are the detailed discussion of specific experiments which have been undertaken in the various classes of vertebrates, as these provide an important background for further work on the basis of sexuality. Also, the final chapter will be of interest to many. This

presents a lucid account of the psychology and psychological problems that have been encountered in humans which might be classified as intersexes.

Something of interest will be found for the experimenter working on the determination of sex and for both the clinician and research worker who have an interest in the relationships between the psychological and morphological characteristics and their role in the determination of gender.—*Robert D. Lisk*

Paper Chromatography, A Comprehensive Treatise, Vol. I, 955 pages; $26.50; Vol. II. *Bibliography of Paper Chromatography 1944–1956,* 766 pages; $24., edited by I. M. HAIS & K. MACEK; Vol. III. *Bibliography of Paper Chromatography 1957–1960,* edited by K. MACEK, I. M. HAIS *et al.;* 706 pages; $24.; Academic Press, 1964 (Publishing House of Czechoslovak Academy of Sciences, 1962).

The editors were indeed justified in calling this "a comprehensive treatise" on paper chromatography, and as such, their books will serve as important reference volumes in most scientific libraries. Although earlier books have been concerned at least in part with paper chromatography, a comprehensive review of this nature had been lacking.

Important developments in other chromatographic techniques, especially thin layer and gas-liquid chromatography, are currently causing these latter techniques rapidly to assume much of the important role that paper chromatography has played in research. An extensive review of paper chromatography, which can certainly be considered a technique already highly developed in practical application if not in its theoretical aspects, therefore seems appropriately timed. It can be anticipated that these books will stand for a long time as the authoritative treatise on paper chromatography.

The authors generally did an outstanding job of describing and assessing the place of importance of the hundreds of detailed variations in technique which have been reported in the use of paper chromatography, e.g., in different manners of sample application, development, detection and preparative separation methods (General Section, 208 pp.). A Special Section (532 pp.) treats, in turn, different classes of compounds in a detailed manner which is extremely useful in that quick reference to a pertinent chapter can answer many of the needs of the researcher either long experienced in the use of paper chromatography or new in the field. A section of Practical Notes describing the preparation of detection reagents, solvent systems, samples and papers is ultimately compiled for greater convenience to the researcher.

The combined bibliographies contain 18,582 references (prior to 1961) conveniently arranged according to subject matter. The translation to English seems to have been accomplished very adequately and the text reads smoothly.—*J. J. Willard*

Stoichiometry & Structure, Part I by M. J. SIENKO; 365 pages; $2.95 paper; W. A. Benjamin, Inc., 1964. "*Freshman Chemistry Problems & How to Solve Them.*"

In this first, of a two-volume set, the author has written a problem book fully equal to the one or two that now dominate the field.

While a few may feel that much of the material covered is too elementary, Dr. Sienko exhibits his intimate knowledge of the actual weakness of incoming college freshmen by devoting the first 140 of a total of 695 problems to the review of fundamental mathematical operations. This is followed by a lucid explanation of the use of the slide rule. Thereafter, the book closely follows the order found in the usual first year college chemistry text: stoichiometry, gases, liquids, solids, thermochemistry, electrochemistry and solutions. The various aspects of equilibrium are reserved for Volume II. This organization both enhances its use as a self-study aid, and facilitates teaching from it.

With consistent clarity and interest, each subject is covered, first by descriptive material, then by problems having the method of solution illustrated, fol-

lowed by increasingly difficult problems to which only the answers are given. The number of problems in each section ranges from 24 and 30 for formulae and liquids, to 68 for electro-chemistry and 140 on mathematical review. There has been an obvious attempt to balance the space allotted each concept with its importance to the student and its difficulty to master.

The slide rule section is especially welcome in a book of this type, and there will be little dissent concerning the value of the enlarged mathematics review section. However, the advisability of devoting so much space to trigonometry and space geometry will be questioned.—*Charles Howard*

The Crisis in Medical Education by L. J. Evans; 101 pages; $4.00; The University of Michigan Press, 1964.

While recent newspaper headlines about medicine have gone either to political events or to the spectacular scientific and technical advances of its scientists and practitioners, many thoughtful observers have been studying the relationships between the medical profession and the society in which it functions. Their attention has focussed not only on the individual physician and his individual patient but on the whole profession and society in part or as a whole, and on the nature of medical education itself. From the stimulus of these studies has come this little eloquent plea for an intensified identification of medical education with the university and for an increased use of the new tools and knowledge of the social sciences in medicine.

Insofar as Evans really is writing about a "crisis," he describes it in the following sentence: "Generally speaking, the problems are (1) to rediscover the student . . . as the reason for medical and health profession education, and (2) to rediscover the patient . . . as the reason for medicine and without whom the student cannot learn (sic)." (p. 23) That is about as angry or agitated as he becomes about "the crisis" anywhere in the book, and at no point does he set off on a major polemic against current trends in medical education; his words form thoughtful and considered evaluations of the general scene.

What can be accomplished on such a vast subject within the span of one hundred pages is of course limited, but one wishes that Evans had taken a few pages to discuss the genuine, if rudimentary, steps which some medical schools have taken to meet the very problems raised in Evans' first chapter. For example, a very practical description of some of the accomplishments and some of the difficulties in applying the social sciences to medicine and medical education occurs in Stanley H. King's *Perceptions of Illness and Medical Practice* (Russell Sage Foundation, 1962). Evans is justified in taking a more purely philosophical approach, but some cross-references would have been helpful.—*Willard Dalrymple*

Coordination Chemistry by F. Basolo & R. Johnson; 180 pages; $3.95 cloth; $1.95 paper; W. A. Benjamin, Inc., 1964.

This book was "written to supplement the material now available on the subject to freshman chemistry students. The authors believe that the material presented will also be of value to students in the junior-senior level courses in inorganic chemistry." The reviewer expects that the latter case will be more general than the former. The authors have produced a well-organized, well-written and readable book. Chapter I includes a historical development and the conventions for naming coordination compounds. In Chapter II, theories of the coordinate bond are presented in a qualitative fashion, starting with the Lewis electron-pair bond, and carrying through to the ligand field and molecular orbital theories. Chapter III on steroeochemistry includes an interesting qualitative discussion on the Jahn-Teller effect, geometrical, optical, and other types of isomerism. Chapter IV on the preparation and reactions of coordination compounds reveals the authors' experience in this field and makes good reading for most any chemist, regardless of his background. The last

two chapters on complex ion stability and kinetics and mechanism of reactions are organized around trends based on the charge density of the metal ion and the crystal field stabilization energy of the complex. This is well done, although the material on reaction mechanisms is pretty compact for the uninitiated. The manuscript is remarkably free of errors although the authors seem to be unable to make up their mind (p. 114–116) whether the ammine-aquo species of copper (II) should be represented as six coordinate or four coordinate. On a graph showing the percentage of Cu^{2+} in the form of various ammine complexes, the formulae are given as $Cu(NH_3)_{6-x}(H_2O)_x{}^{2+}$, while, in the accompanying equations and text, the species are written as coordination number four species. There are a large number of drawings to supplement the textual material. The printing job is neat and clean, although the type may be a little on the small side.—*Edwin M. Larsen*

Introduction to Semiconductor Physics Vol. I, by R. B. ADLER *et al.;* 247 pages; $4.50 cloth; $2.65 paper; John Wiley & Sons, 1964.

This book is the first volume of a seven-volume series by an education committee. The series is oriented toward transistor circuits. This first volume covers some topics in semiconductor physics that are useful preliminaries to understanding junction transistors. The text reads smoothly, and the material covered is very well-planned.

The first two chapters cover concepts such as the covalent bond, holes, effective mass, donors, mobility, recombination, injection, energy bands, etc. The discussion here is mainly qualitative, as it should be, and does not utilize detailed band calculations. Accompanying figures and tables give many quantitative data on Ge and Si.

The last two chapters cover distributions, carrier concentrations *versus* temperature and doping, diffusion, drift, pair motion, etc. The discussion here is more mathematical, and the important relationships stand out in good perspective. Solutions for pair motion consistent with all relevant equations are developed in illustrative examples. A final section outlines carefully worked-out laboratory experiments.

The book provides physical background for Ge and Si junction transistors—as opposed, say, to background for optical properties of semiconductors, for injection lasers, or for the chemical problems peculiar to more ionic semiconducturs. The book is brief in the sense that every topic covered needs to be mastered by a person desiring to be expert in transistors. However, the book doesn't actually include how a transistor works, and it contains some material which many beginners could usefully postpone until after understanding transistor action. Such people should use the book in conjunction with Vol. II or its equivalent.—*W. H. Fonger*

Bargaining Behavior by L. E. FOURAKER & S. SIEGEL; 309 pages; $8.95; McGraw-Hill Book Co., 1963.

Choice, Strategy & Utility by S. SIEGEL, *et al.;* 180 pages; $5.95; McGraw-Hill Book Co., 1964.

It is of credit to the authors of these two volumes that they report clearly a number of well-executed laboratory experiments designed to verify several predictions based on formal mathematical models.

The first book, *Bargaining Behavior*, like its predecessor, *Bargaining Behavior and Group Decision Making* (McGraw-Hill, 1960), by the same authors, represents a skilled attempt at verifying experimentally the implications of several economic models. In *Bargaining Behavior*, Fouraker and Siegel report several experiments that examine the effects of amount of information, form of bidding, and position of equal split of profits, on bargaining in a bilateral monopoly (i.e., a situation in which there is only one buyer and one seller). The amount of information that bargainers received was either complete, each knew both his own and the others' profits, or incomplete, each bargainer knew his own profits. Form of bidding was either single transaction, i.e., bid-

ding ends after each player makes his choice, or repeated transactions, i.e., bidding process continues for 21 trials. The position at which profits for both bargainers was equal was either at the Bowely point or at the Paretian Optima. The Bowley point is the predicted outcome if bargainers play to maximize their own outcome, while the Paretian Optima is the predicted outcome if they play to maximize the joint outcome.

These experiments, as well as others in which the authors used two different economic models to predict the outcome in two and three-man oligoplies (i.e., the control of the amount and price of a given product by a few sellers) demonstrate that individuals do not invariably tend to maximize either joint or individual profits but, rather, their bargaining behavior is affected by the context in which it occurs.

Choice, Strategy and Utility reports a series of experiments designed to test the adequacy of a mathematical model which generates the following hypothesis: The tendency for individuals to select a strategy which will maximize the probability of a correct choice will increase (1) as the utility of the correct choice increases, and (2) as the utility of choice variability decreases. The authors assumed that the utility of a choice varies directly with the payoff for that choice. The utility of variability is based on the assumption that the individual values variability in his behavior. Thus, the more boring the task, the greater the utility of variability. Two experiments, each with a different age group, were concerned with ordinal predictions based on the utility of a correct choice assumption. The data indicated that subjects did tend to use a strategy of maximization as the utility of correct choice, i.e., the payoff, increased. Similarly by increasing the cognitive and kinesthetic richness of the situation, the authors were able to demonstrate an increased tendency to use a strategy of maximization.

In other experiments, the authors made specific quantitative predictions regarding the subjects' patterns of choice among the alternatives. In this case, there was a high correlation between the theoretical patterns of choice and the observed ones under two different experimental treatments: (1) where the marginal utility (i.e.. the ratio between the utility of correct choice and the utility of variability) is held constant over the two alternatives; and (2) where the marginal utility is varied over the three available alternatives.

In both these volumes, the authors neglect to review the available research. Thus, the reader is unable to place their research in any historical context or grasp its significance in relation to other pertinent theory or research. Nevertheless, both publications are excellent examples of the fruitfulness of integrating mathematical and psychological approaches in the study of human behavior.—*Harvey Hornstein*

Telemetry (Monographs on Rockets & Missiles) by R. E. YOUNG; 78 pages; $3.95; Gordon & Breach, 1964.

With the spectacular growth and increased complexity of engineering technics used in many experiments, a need for terse descriptions of specialized engineering has been created. Scientists who must use advanced engineering as part of their experiment but who do not have the time to become familiar with all the intricacies of the design will find it valuable, and in some cases necessary, to understand the essential practices and design problems associated with particular engineering specialties. This is certainly true in the area of space research.

The function of telemetry is to provide information to the experimenter who is on the ground of the physical conditions inside the space vehicle and also to transmit externally received data obtained during flight. The author of this short monograph has given a brief discussion of the basic methods involved in telemetry as well as a description of the components currently employed in the telemetry radio link and data handling systems. The author relates his discussion more specifically to missile telemetry although the basic systems are similar in satellite experiments. The book will be useful to the reader who

wishes to get an introduction to the components that make up a telemetry system. This reviewer feels that the book would be more useful if the author gave greater depth to his discussion of components peculiar to telemetry and fewer pages to discussing such elementary concepts as the noise figure of a receiver and the properties of transmission lines.—*W. K. Rose*

Aspects of Theoretical Mineralogy in the U.S.S.R., A Collection of Papers, edited by M. H. BATTEY & S. I. TOMKEIEFF; 507 pages, $30.00; The Macmillan Company, Pergamon Press, 1964. Vol. 18, International Series of Monographs in Earth Sciences, D. E. Ingerson, Editor.

This volume contains translations of 35 articles published between 1943 and 1959 by one Bulgarian and 11 Russian mineralogists. The articles are concerned with problems such as the definition of a mineral (6 articles, 67 pages), bases for and schemes of mineral classification (16 articles, 269 pages), and the correlation of physical properties with the crystal chemistry of minerals (13 articles, 148 pages). Even at the time they were written the articles contained little new data; the purpose was to extract generalizations, principles, and interrelationships from the voluminous data pertinent to mineralogy, both Soviet and western. The editors have attempted to update the articles by incorporating changes suggested by the original authors, and several inconsistencies between articles have been arbitrated, but the result lacks any spirit of controversy or revelation of dynamic development it might have had 6 to 10 years ago when the majority of the more recent articles were written. A modern mineralogist has the advantage that the test of time has been applied to many of the suggestions made in the text, and a great many have been found wanting. As a consequence, the book will be of most value to the authors of mineralogical reference works and texts, to historians of science, and to some crystal chemists and crystallographers as a source book. Few practicing mineralogists or geochemists will find it a worthwhile investment, as it is neither current nor a reference work.

The selection of articles for such a volume is necessarily arbitrary, but clearly A. S. Povarennykh is overexposed (15 articles out of 35); the five most represented authors and their collaborators wrote 83 per cent of the articles. The presentation is therefore probably not a completely balanced view of Russian thought and practice.

The translations and technical editing are of superior quality, and the composition and printing are excellent throughout. The author index is perfunctory and suffers from the usual poor quality of Russian literature citations; greater attention by the editors would have been helpful. The subject index is so lacking as to constitute a serious fault: it is a single page with about 60 line items "restricted to the main references to the principal subjects discussed in this volume" (p. 505), for example, "Minerals, classification of 3–8, 79–218"! Surely the casual reader needs more guidance through the topics covered in 35 almost unrelated articles, and one advantage this out-dated collection might have had was quick access to who thought what about a given topic. It would also have been helpful to find quickly where major mineral groups such as feldspars or pyroxenes were discussed, if at all.—*D. B. Stewart.*

Computing Methods in Optimization Problems, edited by A. V. BALAKRISHNAN & L. W. NEUSTADT; 327 pages; $7.50; Academic Press, 1964.

This book is a collection of papers delivered at a conference held at the University of California, Los Angeles, in January 1964. Although the editors have not divided the papers into general classifications, the reviewer has done so below:

A. *Mathematical Theory*, 4 papers; B. *General Applications*, 5 papers; C. *Specific Applications*, 3 papers; and D. *Hybrid Computing Techniques*, 3 papers.

Clearly there is no continuity between papers. None was intended by the editors. Conference attendees participated in discussions of the papers as they were presented; unfortunately, this

discussion is not included in the volume. The inclusion of discussers' comments would have been a valuable addition to the book and, indeed, would have more realistically reflected the spirit of the conference.

The style and lucidity of the papers differ considerably and the level of the assumed audience also varies from paper to paper. Such criticisms are to be expected in a book of this sort.

The volume is of value to researchers and students of the computational arts whose work falls in an area covered by a specific conference paper. It illustrates, among other things, that definite progress has been made in harnessing the large-scale digital computer for the solution of certain engineering problems. Computational experience with hybrid computing systems is discussed. This is a rather new area of research and relatively few published results in the control field are to be found. Specialists in process control and students of systems containing pure time delay should find interesting reading in two of the papers.—*Stephen J. Kahne.*

Star Evolution, Course 28, edited by G. POLVANI; 488 pages; $18.50; Academic Press, 1964. Proceedings of the International School of Physics "Enrico Fermi," Varenna, Italy, August 1961, L. Gratton, Director.

This book contains material on stellar evolution presented at the International School of Physics "Enrico Fermi." Except for one article; "Nuclear Astrophysics" by G. R. Burbidge, which was reprinted from the Annual Review of Nuclear Science, all of the articles were recently written and some contain new results. I find it gratifying that certain elementary articles were included, which makes the book more self-contained than some other proceedings.

The lectures can be grouped into several broad categories, they are:

(1) Interpretation of the Hertzsprung-Russel Diagram, including the following articles: "Observational Approach to Stellar Evolution" by A. R. Sandage and L. Gratten, "Construction of Stellar Models" by M. H. Wrubel, "Stellar Models" by J. Faulkner and K. Griffths.

(2) Nucleogenesis Problems, including: "Nuclear Astrophysics," by G. R. Burbidge, "Observed Abundance Anomalies Indicating Nucleosynthesis in Individual Stars" by E. M. Burbidge, "Notes on Cosmic Rays" by M. M. Shapiro.

(3) Stellar Evolution in General, including most of the remaining articles: "The Early Stages of Stellar Evolution" by E. Schatzman, "Relation Between the Evolution of Stars and Galaxies" by E. M. Burbidge, "Stars with Helium Rich Cores" by R. Kippenhahn, "White Dwarfs and Type I Supernova" by E. Schatzman, "Theoretical Models for the Pulsation of Cepheids" by N. H. Baker, "Stellar Stability and Stellar Evolution" by P. Ledoux, and

(4) Miscellaneous, the remaining articles are more speculative.

In general a weak correlation exists between articles in the stellar evolution category, but this is rather an indication of the lack of unity of our knowledge of stellar evolution than of the quality of editorship, which I think is fairly good. The quality of presentation is generally high, but it is regretable that most of the English in this book is awkward. The quality of the paper used in the printing reflects its import origin (poor) but the price of this book is very domestic (high). Despite these two defects, this book is highly recommended for every library. The individual buyer must be reminded that, in such a rapidly progressing field as stellar evolution, material in this book will be out-of-date soon and the individual buyer may want to reconsider the investment on account of the high price.—*Hong-Yee Chiu*

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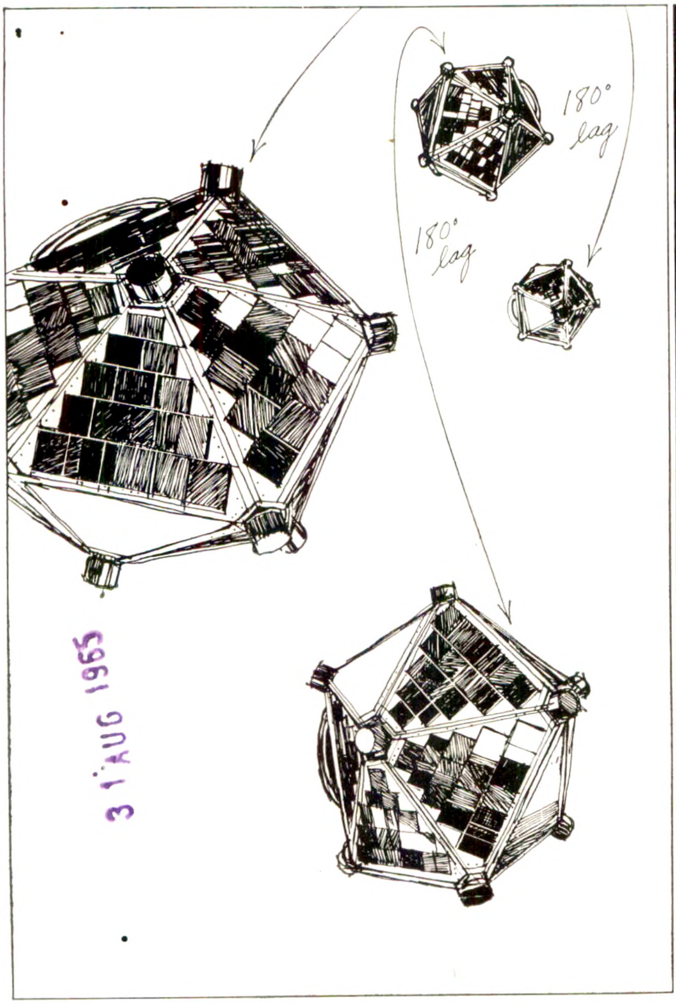

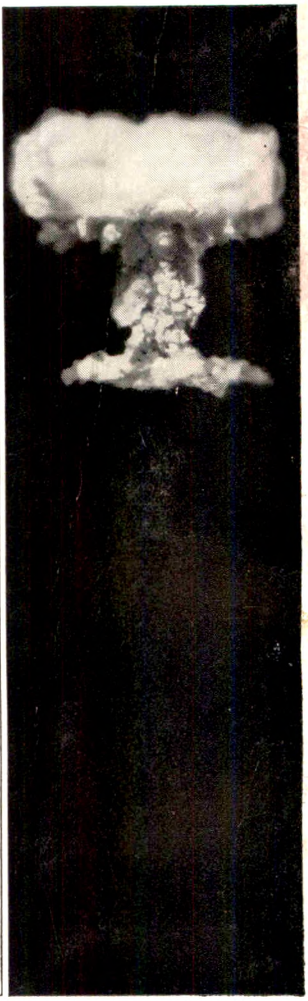

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SEPTEMBER 1965

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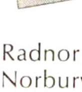

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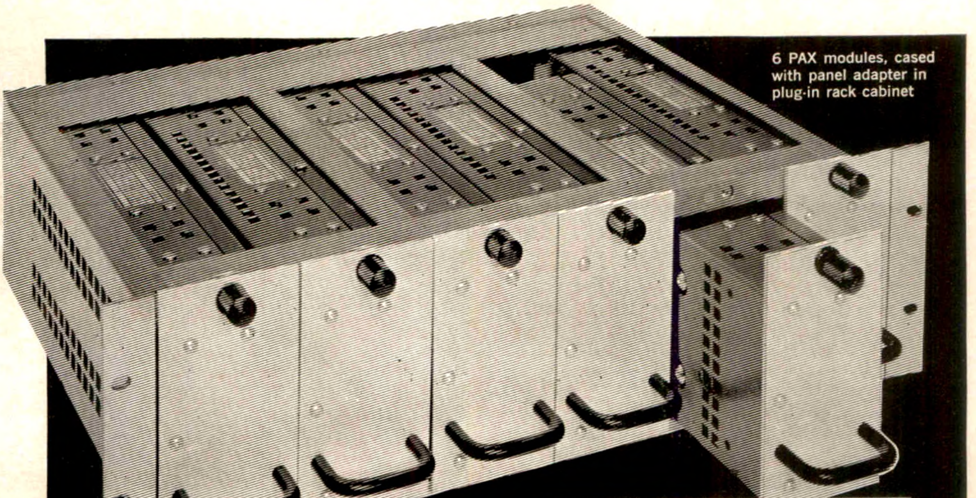

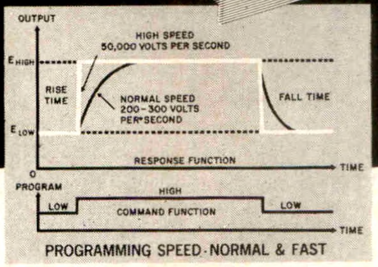

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VOLUME 53 SEPTEMBER 1965 NUMBER 3

PUBLISHED IN THE INTEREST OF SCIENTIFIC RESEARCH

CONTRIBUTORS

A. H. STURTEVANT, *The Fly Room* 303

Professor Sturtevant, who is himself one of the great pioneers of modern genetics, has devoted the last few years to writing *A History of Genetics* (to be published this fall by Harper & Row). This is one chapter in which he describes T. H. Morgan's fly room, where Dr. Sturtevant himself made such notable contributions. The chapter's appearance here owes much to the cooperation of Mr. A. D. Sinauer, Biology Editor of Harper & Row. Our frontispiece, a previously unpublished photograph, will also appear in a forthcoming book by Chandler Fulton of Brandeis University. Author's address: Division of Biology, California Institute of Technology, Pasadena 4.

C. M. SLIEPCEVICH, *Liquefied Natural Gas—A New Source of Energy: Part II, Peak Load Shaving and Other Uses* 308

A transfer student from Montana State College to the University of Michigan in 1939, a BS in 1941, and an MS in Chemical Engineering in 1942, Dr. Sliepcevich obtained his PhD in 1948 after a period of years devoted to classified research during the war. In 1951, he became an Associate Professor of Chemical and Metallurgical Engineering at the University of Michigan, and, in 1955, became Professor and Chairman of Chemical Engineering at the University of Oklahoma at Norman, Okla. Since 1958 he has been Chairman of the School of General Engineering. In this second section of his Sigma Xi-RESA National Lecture, 1961–62 series, he describes the application of Liquefied Natural Gas to meet peak consumption of gas by local utilities and as a possible rocket propellant.

SHELDON C. REED, *The Evolution of Human Intelligence—Some Reasons Why It Should be a Continuing Process* 317

Professor Reed has been the Director of the Dight Institute of Human Genetics at the University of Minnesota (Minneapolis 55455) since 1947. His attempts to bring human genetics to the attention of physicians resulted in the popular book *Counseling in Medical Genetics* (1963) and recently, in collaboration with his wife, Dr. Elizabeth Reed, he has published *Mental Retardation: A Family Study.*

AMERICAN SCIENTIST is published quarterly (March, June, September, December) by the Society of the Sigma Xi. *Office of Publication:* Mack Printing Co., 20th and Northampton Sts. Easton, Pa.
Second-class mail privileges authorized at Easton, Pa. Additional entry at the New Haven, Conn., post office, December 9, 1943. This publication is authorized to be mailed at the special rates of postage prescribed by Section 132.122.

A. Chapanis, *Color Names for Color Space* 327

The Sigma Xi-RESA National Lecturer for 1964–65 was born in Meriden, Conn., received his bachelor's degree in psychology at the University of Connecticut, and his PhD degree at Yale in 1943. After service in the Air Force in 1943–46, he has been attached to The Johns Hopkins University (Baltimore, Md. 21218), becoming, in 1956, Professor of Psychology and Industrial Engineering. A Fellow of the American Psychological Association, he received the Franklin V. Taylor award in 1963 for outstanding contributions in the field of engineering psychology and was elected President of the Human Factors Society for 1963–64. His lecture is concerned with the use of color names by people and with the agreement found in the use of such color names.

Lawrence B. Slobodkin, *On the Present Incompleteness of Mathematical Ecology* 347

Professor Slobodkin (of the Department of Zoology, University of Michigan, Ann Arbor) is an ecologist. His recent studies with fresh-water hydra were reported in terms of evolutionary strategy in a previous issue of American Scientist. Here he gives us some stimulating thoughts about the application of mathematics to ecology. From this fall until July 1966 the author will be a visiting professor in the Department of Zoology, University of Tel-Aviv, Tel-Aviv, Israel.

Peter J. Wangersky, *The Organic Chemistry of Sea Water* 358

A BS in chemistry from Brown University, he received a PhD in zoology from Yale University. Like many oceanographers he has been something of a migrant worker, at Scripps, the U.S. Fish and Wildlife Gulf Fisheries Investigation at Galveston, Texas, the Marine Laboratory of the University of Miami, Florida, and the Bingham Oceanographic Laboratory at Yale University. After trans-Atlantic expeditions and work in Long Island Sound, he has recently joined the Institute of Oceanography, Dalhousie University, Halifax, Nova Scotia, Canada.

G. J. Levenbach, *Systems Reliability and Engineering Statistical Aspects* 375

Born in Philadelphia, Pa., Mr. Levenbach was educated in the Netherlands, receiving the ME degree from the Institute of Technology in Delft in 1933. For many years he worked as a Communications Engineer with Philips Telecommunications Industries in Holland and with the Governmental Telephone Service in Indonesia. In 1953, he returned to the U.S.A. and joined the Bell Telephone Laboratories. Since 1960, he has been a member of Bell's Reliability Engineering Center, Whippany, N.J., in charge of Systems Reliability. An associate editor of *Technometrics*, he is also a member of professional statistical societies here and abroad.

LIENG-HUANG LEE, *Beyond the Horizon of Science* 292A

The President of the Midland Branch of RESA, a research organic chemist in the Dow Chemical Co., presented this address at the Seventh Initiation Meeting of the Midland Branch of the Scientific Research Society of America, January 1965. Home address: 1510 Ohio St., Midland, Mich.

LAWRENCE CRANBERG, *Ethical Problems of Scientists* 303A

Through the courtesy of *Physics Today, 18,* No. 4, 51–52 (1965), we are presenting a summary of a lecture given in April 1964 at a seminar of the Physics Department of the University of Washington, Seattle. The author is Professor of Physics at the University of Virginia.

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NEWS AND VIEWS

By the Board of Editors and the Membership of the Society of the Sigma Xi and the Scientific Research Society of America, RESA

EDITORIAL COMMENT

It is with a keen sense of regret that the Board of Editors of AMERICAN SCIENTIST has received the resignation of Professor George B. Field, who has been a consultant of the journal, devoting special attention to books received in the area of astronomy, advising the Board on the quality of articles submitted, and the possible sources of other contributions. Volumes 51 to 53 are the richer for his efforts. While we voice our thanks and our regrets, we extend to him our congratulations on the transition from the Princeton scene to a new position in Berkeley, where we hope for him new opportunities and further success.

As in previous September issues, we present the report of the Chairman of the Committee on Grants-in-Aid of Research, Dr. Harlow Shapley. The awards made this year are substantially less in number and amounts than those of the preceding year because of the special circumstances reported in the June 1965 issue, page 160A. It is hoped that this fruitful activity of the Society may be resumed on an even more generous scale in the coming year. The entire membership of the Society will be invited to assist in the task.

The new feature to be found in this issue include a list of Chapters and Clubs with the dates of their establishment and their number, together with the names of the officers of the Chapters and Clubs.

GRANTS-IN-AID OF RESEARCH

Report of Awards made by Grants-in-Aid of Research Committee for 1965

As reported to the Society in the June issue of AMERICAN SCIENTIST (page 160A), 1965 has been a most trying and disturbing year for the Grants-in-Aid of Research Committee. With all funds exhausted after the first two meetings of the Committee, the regular June meeting was cancelled and all applicants were notified.

President Farrington Daniels personally wrote to all chapter and club presidents to acquaint them with the financial problem of the Grants-in-Aid of Research operation and invited them to have their groups assist if at all possible. In response to this letter, contributions were received from a number of groups so that the Society has received to date $3000 more than had been budgeted for 1965 from this source of support. In view of this additional commitment the Executive Committee, upon Dr. Daniels' recommendation, voted a corresponding increase in the appropriation allocated to the Grants-in-Aid of Research Committee.

This added support permitted the Committee to make 17 awards to individuals whose applications had been pending for action by the June meeting prior to its cancellation.

Again this past year the Grants-in-Aid of Research Fund was the recipient of a gift from Mrs. Daisy Yen Wu in memory of her husband, Dr. Hsien Wu. The Hsien Wu and Daisy Yen Wu Fund has now reached a total of just over $20,000, which amount Mrs. Wu had pledged in support of Grants-in-Aid of Research.

The Committee wishes to call attention to two awards in the following list which were made possible by earnings from this Fund: The award to Elaine N. McIntosh and the award to Elizabeth C. Paulsen.

The Committee expresses its appreciation to all those who have given support to this most important activity of the Society, and we hope that in 1966 we shall be able to meet more of the deserving requests than it was possible to do in 1965.

HARLOW SHAPLEY
Chairman
Grants-in-Aid of Research Committee

GRANTS-IN-AID OF RESEARCH AWARDS FOR 1965

•Helmer A. Abramson, Michigan Technological University. "Solubility of cobalt ammine carbonate in ammonium carbonate solutions. Effect of ammonia and carbon dioxide concentrations"; $200.

Lowell Adams, University of California, San Francisco Medical Center. "Deep body temperatures of hibernating California ground squirrels measured by implanted radio telemeters"; $300.

Wayne M. Ahr, Rice University. "Petrology and depositional history of the point peak member of the Wilberns formation"; $300.

Leonard P. Alberstadt, University of Oklahoma. "Biostratigraphic study of the Viola and Fernvale formations (Ordovician) in the Arbuckle Mountain region of south-central Oklahoma"; $300.

Harrison W. Ambrose, III, University of Michigan. "The use of isotopes in a study of the ecological significance of home range"; $500.

Dennis E. Anderson, Humboldt State College. "Distribution and relationship of chromosome number races in Montia"; $450.

Edwin J. Anderson, Brown University. "Relationship of morphologic variation of *Gypidula coeymanensis* to Coeyman's limestone paleoenvironment"; $400.

Peter B. Andrews, University of Texas. "St. Joseph Island, Aransas Co., Texas; a washover fan. Lithofacies and biofacies analysis"; $400.

Theodore J. Armbrustmacher, State University of Iowa. "Mafic dikes of the Clear Creek drainage area, Eastern Bighorn Mountains, Wyoming"; $600.

Norman S. Bailey, Bradford Junior College. "The bioecology of the Tingidae (Heteroptera) and Tabanidae (Diptera) of Swans Island, Maine and vicinity"; $500.

Edwin M. Banks, University of Illinois. "The ethology of the varying lemming, *Dicrostonyx groenlandicus*"; $500.

Kenneth R. Barker, University of Texas. "The spermateleosis and sperm morphology of *Emerita talpoida* as revealed by electron microscopy"; $250.

Shelley Barker, University of Adelaide, South Australia. "The ecology of *Rattus greyi*"; $300.

Henry J. Bauerlein, Virginia Polytechnic Institute. "Structure and stratigraphy of the Cove Mountain Block—a possible key to the mechanics of Appalachian overthrusting"; $250.

James B. Benedict, University of Wisconsin. "Soil-moisture variations in the Colorado front range since 300 A.D."; $300.

Theodore L. Bissell, University of Maryland. "Classification and description of aphids inhabiting trees of the family Juglandaceae in North America"; $300.

Will H. Blackwell, Jr., University of Texas. "Taxonomic studies in the plant genus *Bouvardia*"; $350.

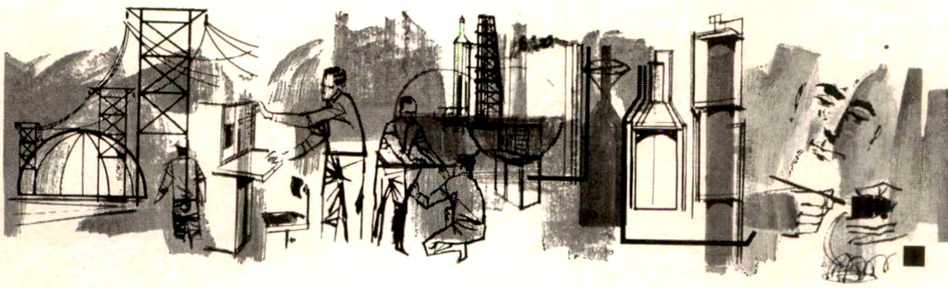
Stephen F. Bowen, Jr., St. Louis University, Medical School. "Resorption of *central* corneal gut sutures-time and method of resorption"; $200.

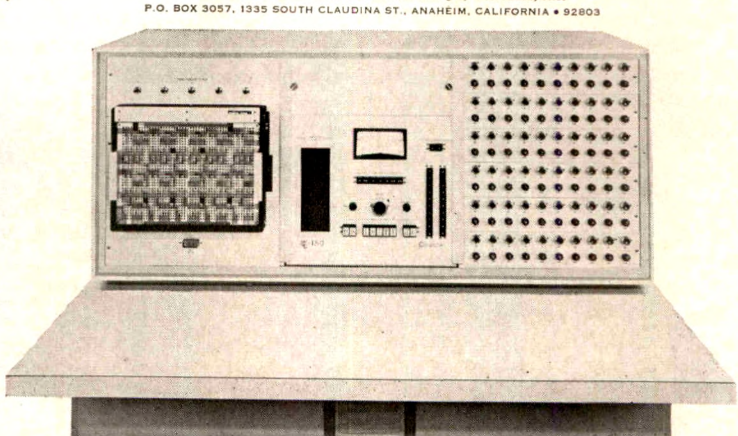
Peter W. Bretsky, Jr., Yale University. "Paleoecology of the *Orthorhynchula* zone (Ordovician), central Appalachians"; $200.

Garnett R. Brooks, Jr., College of William & Mary. "Efficiency of food utilization by lizards"; $300.

Leonard F. Brown, Jr., Baylor University. "Stratigraphic and depositional framework of upper Pennsylvanian and lower Permian rocks, north-central Texas"; $300.

Robert L. Burgess, North Dakota State University. "An annotated bibliography of Arizona vegetation"; $325.





Guy L. Bush, University of Melbourne, Australia. "The comparative cytology of the primitive mecopteran families Choristidae and Nannochoristidae"; $500.

Brother Joseph Cain, St. Edward's University, Austin, Texas. "Nitrogen utilization in the genus *Chlamydomonas*"; $314.

Larry D. Caldwell, Central Michigan University. "The dynamics of an island population of small mammals with reduced interspecific competition"; $250.

William S. Calkin, Colorado School of Mines. "The geology of the Alum Creek area, San Juan Mountains, Colorado"; $200.

John A. Campbell, Colorado State University. "Stratigraphy, sedimentary petrology, and paleoecology of the Devonian of Central Colorado"; $400.

Wayne F. Canis, University of Missouri. "Biostratigraphy of the Chouteau group of Missouri"; $275.

William F. Cannon, Syracuse University. "Geology of the La Cloche area: Ontario"; $325.

K. Douglas Carlson, Case Institute of Technology. "High temperature chemistry of refractory oxides, nitrides, and phosphides"; $400.

Richard A. Cavalero, University of Maine. "The stratigraphy and metamorphic and igneous petrology of the Clifton Township area, Maine"; $250.

Sarah Clevenger, Indiana State College. "The flavonoid compounds found in members of the genus *Impatiens*"; $400.

John J. Cochrane, Rensselaer Polytechnic Institute. "Theory of floc strengthening for filtration"; $500.

Martin L. Cody, University of Pennsylvania. "Coexistence mechanisms in grassland birds"; $600.

John W. Connolly, Marietta College. "The Wurtz reaction in organosilicon"; $400.

Omar G. Conrad, University of Texas. "Stratigraphy and structure of the Tertiary ignimbrites, Southwestern Utah"; $400.

Richard L. Cooley, Pennsylvania State University. "The role of mountain range structure and stratigraphy in ground water recharge to a semiarid basin"; $475.

John D. Cooper, University of Texas. "Stratigraphy of the Escondido formation"; $325.

Ursula M. Cowgill, Yale University. "Demography of York, Yorkshire, England in the 16th, 17th, 18th, and early 19th century"; $700.

Jesse L. Craft, Jr., University of Western Ontario, Canada. "Glacial geology of the Tupper Lake-Raquette Lake region, Adirondack Mountains, New York"; $600.

Richard W. Dapson, Cornell University. "Seasonal fluctuations in cranial size and shape in the short-tail shrew, *Blarina brevicauda*"; $300.

John C. Davis, University of Wyoming. "Petrology of the Mowry shale"; $200.

J. David Davis, Merrimack College, North Andover, Mass. "Preparation of arylorthoformates and substituted aryloxy-halogermanes"; $300.

Richard A. Davis, Jr., University of Wisconsin. "Paleoecology of the Shakopee Dolomite (Ordovician) in the upper Mississippi valley"; $300.

Dwight E. Deal, University of New Mexico. "Cenozoic history of the Gila River drainage, southwestern New Mexico"; $425.

Jared M. Diamond, Harvard Medical School. "Breeding habits and distribution of birds in the Karimui basin, New Guinea"; $400.

Roger L. Dominowski, Northwestern University. "Anagram solving as a function of word frequency and bigram frequency rank"; $250.

Lewis Dunbar Dove, Tulane University. "Relationships between the level of adenosine triphosphate, organic nitrogen, and the onset of water stress in excised tomato leaves"; $400.

James H. Dover, University of Washington. "Metamorphism and structure of the Pioneer Mountains, east-central Idaho"; $500.

Richard D. Estes, Boston University. "Biology and relationships of the Mexican cone-nosed toad *Rhinophrynus dorsalis*"; $300.

George F. Fisler, San Fernando Valley State College, Northridge, Cali-

fornia. "Activity patterns in small rodents"; $300.

James M. Forman, Tufts University School of Dental Medicine. "Study of isoenzymes found in salivary glands by electrophoretic separation"; $200.

Donald R. Fowler, Northwestern University. "Application of statistics to the study of the paleoecology of the Calvert and Choptank formations of the Maryland Miocene"; $300.

Robert Fried, Hunter College, New York. "Development of a method for obtaining conditioned aversion to cigarette smoking"; $400.

David S. Fullerton, Yale University. "Glacial geology of the upper Mohawk region, New York"; $250.

Herschel W. Garner, Texas Technological College. "Ecological aspects of of the brush mouse, *Peromyscus boylii*"; $300.

Mother Bonaventure Ghidoni, College of New Rochelle. "Character of the phytoplancton and zooplancton of Long Island Sound as a result of changing ecological conditions"; $600.

Joel E. Giddens, University of Georgia. "The molybdenum requirement of symbiotic nitrogen-fixing bacteria"; $550.

David L. Giles, University of New Mexico. "Geochemistry of selected welded tuff units, SW New Mexico and SE Arizona, with reference to the datil volcanic group"; $450.

William A. Girdley, Washington State University. "Shelf sedimentation of eastern Paradox Basin in southwestern Colorado"; $200.

Alice I. Goldsby, Loyola University. "Immunological response of laboratory animals to the antigen of the dog hookworm and the pig *Ascaris lumbricoides*"; $600.

George C. Gorman, Harvard University. "Behavior and blood proteins as indices of relationships in West Indian *Anolis* lizards"; $400.

Thomas Gray, McMaster University, Canada. "The acquired distinctiveness of a signal paired with food"; $500.

Wayne S. Greb, Bowling Green State University. "Sedimentary parameters of the Navajo Sandstone, Uintah County, Utah"; $400.

David W. Greenfield, University of Washington. "Systematics and zoogeography of *Myripristis* (Pisces: Holocentridae)"; $200.

Forrest C. Greenslade, Tulane University. "DNA and deoxyriboside metabolism in *Rana pipiens* nuclear transplant embryos"; $475.

John K. Greer, Michigan State University. "Ecology of rodents of the Province of Malleco,Chile"; $200.

Richard B. Grinols, University of Washington. "Systematic review of deep-water marine fishes off the Washington coast and adjacent waters"; $500.

James O. Guthrie, University of Massachusetts. "Geology of a portion of the Belchertown intrusive complex, west-central Massachusetts"; $200.

David L. Halpin, University of Massachusetts. "Geometry of folds and style of deformation in the Quabbin Hill area, Massachusetts"; $125.

Ronald L. Hancock, Jackson Laboratory, Bar Harbor, Maine. "Nucleolar organizers of blastocyst nuclei"; $70.

Robert C. Harriss, Rice University. "Biogeochemical control on concentrations of Cu, Mn, Pb, and Zn in natural fresh waters of New York"; $200.

Nathan H. Hart, Union College. "Study on origin of endocardial cells and origin and transformation of the septal anlages in the bulbus cordis"; $375.

Brian A. Hazlett, Harvard University. "Studies of Pagurid communications systems"; $800.

Ned D. Heindel, Marshall University. "New synthetic methods for 1,4-thiazines and 1,4-benzothiazines of pharmaceutical interest"; $500.

Philip M. Helfaer, Harvard University. "Psychology of religious doubt: A clinical study of Protestant theology students"; $300.

John A. Hendrickson, Jr., University of Kansas. "Revision of the genus Scellus Loew in North America (Diptera: Dolichopodidae)"; $300.

Magdalena M. Herman, University of Pennsylvania. "Effects of asymmetric stimulation on ego-centric localization"; $400.

Harold F. Hirth, University of Utah.

"The behavior and ecology of migrating snakes"; $400.

Deborah V. Howard, Massachusetts Audubon Society. "Investigation into the reproductive success of the American robin in eastern Massachusetts, as it may be influenced by DDT"; $500.

James F. Howard, Indiana University. "Biostratigraphy and paleoecology of the Waccamaw Fm. (Pliocene) and the Duplin Fm. (late Miocene) of N. and S. Carolina"; $225.

Neil C. Hulings, Texas Christian University. "Taxonomy of recent marine ostracods based on carapace and appendage morphology"; $400.

Arthur M. Hussey, II, Bowdoin College, Maine. "Regional metamorphism of southern York County, Maine"; $400.

James M. Jay, Wayne State University. "Biochemical characterization of beef proteins from beef freshness to spoilage"; $200.

Carl F. Jordan, Douglass College, Rutgers University. "Forest fire influences in the hardwood forests of New Jersey"; $450.

William F. Jud, Jr., Washington University, St. Louis, Missouri. "An investigation of crystallization of ZnS grown by diffusion"; $400.

Edward S. Katkin, State University of New York at Buffalo. "Relationship between autonomic activity, stress and individual differences in anxiety"; $1,000.

David E. Kidd, Michigan State University. "Comparative taxonomy of algae in nature, soil water culture, agar media, and liquid nutrient media"; $350.

Sister M. Laeticia Kilzer, Mount Marty College, Yankton, South Dakota. "Synthesis of 4,4′-bis(1,1-dimethylpropyl) Azobenzene"; $250.

James B. Koenig, Mackay School of Mines, University of Nevada. "Geology and physical chemistry of very fluid Pliocene latite lava flows, Sierra Nevada, California"; $450.

David H. Krinsley, Queens College of The City University of New York. "Study of the surface texture of river sands via electron microscopy"; $300.

Albert M. Kudo, University of California, San Diego. "A study of the melting relations of granitic rocks"; $350.

Byron R. Kulander, West Virginia University. "Evolutional history and structural analysis of Browns Mountain anticline in West Virginia"; $150.

Mel A. Kuntz, Pennsylvania State University. "A statistical analysis and evaluation of modal and chemical variation in a granitic pluton"; $350.

Robin C. Lenn, University of California at Davis. "Primary productivity and energy transfer in hot springs communities of Lassen National Park"; $400.

David J. Leveson, Brooklyn College. "Metamorphic tectonites of the Twin Island-Hunter Island area, Pelham Bay Park, Bronx, New York"; $500.

Norman Lin, Harvard University. "The population ecology of the cicada killer wasp"; $400.

Roy C. Lindholm, Johns Hopkins University. "Carbonate petrography of the Onondaga Limestone, New York"; $400.

Donald W. Linzey, Cornell University. "Ecology and behavior of the golden mouse, *Ochrotomys nuttalli*, in the Great Smoky Mountains"; $500.

John L. Livingston, Rice University. "Bedrock geology and the structural and metamorphic history of southwestern Transylvania County, North Carolina"; $300.

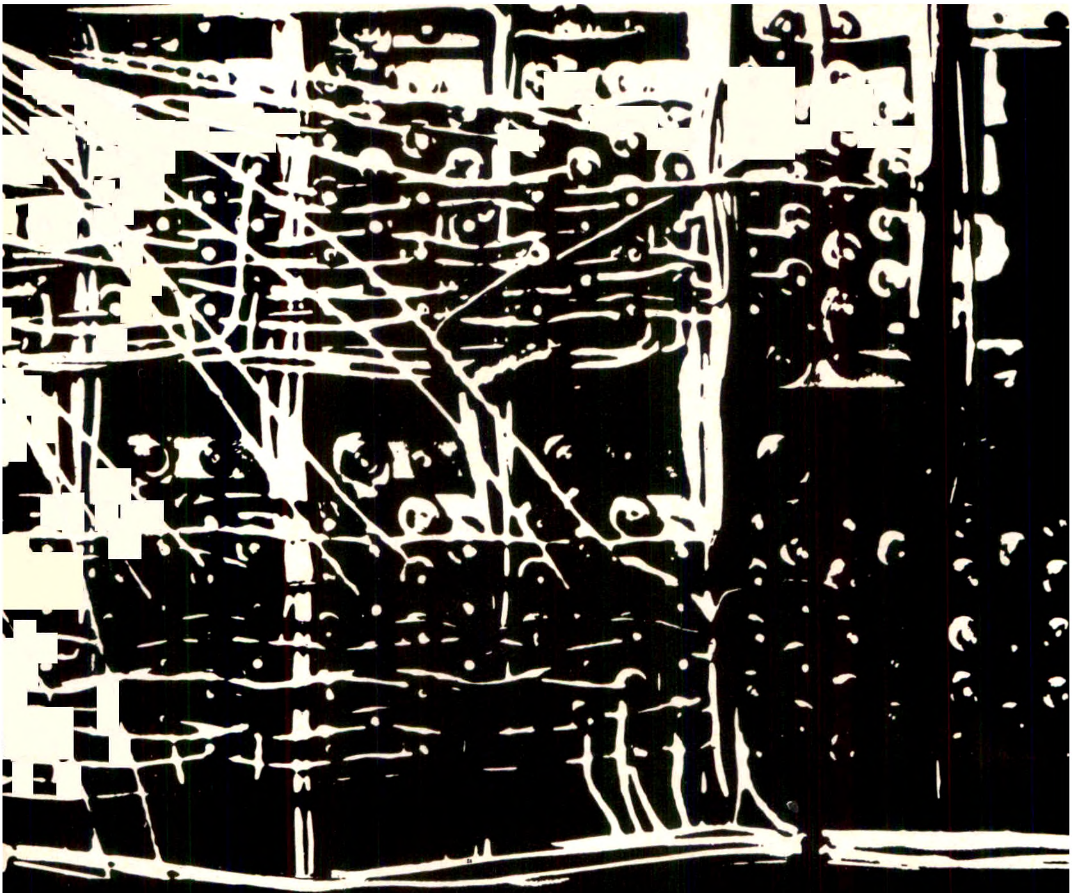
Joseph LoBue, New York University. "A quantitative, kinetic study of hemopoiesis in mice exposed to lowered barometric pressure"; $300.

Carl F. Lohrengel, II, Brigham Young University. "Palynologic correlation of upper Cretaceous formations in central and southern Utah"; $250.

Karl M. Looff, University of Missouri. "Petrology of the Fort Scott formation from the base of the Summit coal to the Higginsville limestone"; $200.

Philip A. Lydon, University of Oregon. "Geology of the Butt Mt. area, a source of the Tuscan formation in northern California"; $400.

John N. MacTavish, Western Reserve University. "Study of the silicified fossil assemblage of the Lodgepole formation (Mississippian), Darby Can-



yon, Teton County, Wyoming"; $300.

Kenneth O. May, University of California at Berkeley. "History of linear algebra"; $600.

Ian N. McCave, Brown University. "A sedimentological and stratigraphical analysis of some Middle Devonian elastic rocks in New York"; $400.

Charles L. McCormick, University of Massachusetts. "Influence of high tidal fluctuation on the sedimentation in marshes and tidal flats of the Merrimack and Parker River estuaries, northeastern Massachusetts"; $400.

Alan McGugan, University of Alberta, Calgary, Canada. "Permo-carboniferous stratigraphy of N.E. British Columbia"; $400.

Elaine N. McIntosh, Tucson, Arizona. "I. Isolation and characterization of mucoprotein in bovine skeletal muscle. II. Effect of post-mortem aging on the mucoprotein fraction of bovine skeletal muscle III. Effect of post-mortem aging on the intracellular proteins of skeletal muscle"; $300. Hsien Wu & Daisy Yen Wu Award.

Brian K. McKnight, Oregon State University. "Stratigraphy and sedimentology of some Cretaceous and Eocene rocks in the Medford-Ashland region, southwestern Oregon"; $400.

Carol A. McLeroy, Stanford University. "Angiosperm pollen from upper Cretaceous Panoche and Moreno formations west of San Joaquin Valley, California"; $400.

Philip A. Medica, New Mexico State University. "A natural history study of four sympatric species of whiptail lizards (*Cnemidophorus*, Family: *Teiidae*)"; $300.

John Merkle, Flint Community Junior College, Flint, Michigan. "Subalpine plant communities of the Teton Mountains, Wyoming"; $250.

Douglas E. Merrill, University of Washington. "Glacial geology in the vicinity of Chiwaukum drainage basin, Washington"; $300.

Robert Metz, Rensselaer Polytechnic Institute. "Stratigraphy and structure of the Cambridge quadrangle, New York"; $300.

Merle E. Meyer, Whitman College, Walla Walla, Washington. "Extrapolation of the sun-arc for astro-navigation in the pigeon"; $900.

Ralph R. Miller, Rutgers University. "No play: A means of conflict resolution"; $250.

William W. Miller, III, Howard College, Birmingham, Alabama. "Influence of central nervous system stimulants and depressants on peripheral nerves"; $600.

Douglas G. Mook, Brown University. "Physiological determinants of ingestion in rats"; $700.

Lorraine P. Morin, Southern Illinois University. "Histological effects of anthelminth drugs on tissues of the tapeworm *Hymenolepis diminuta*"; $400.

Peta Jane Mudie, Del Mar, California. "Biosystematic study of some *Lotus* species in southern California and adjacent areas"; $500.

Russell E. Mumford, Purdue University. "Migration studies of the red bat (*Lasiurus borealis*)"; $250.

Dhanonjoy Nasipuri, University of Calcutta, India. "Asymmetric reduction and a new method for the determination of absolute configuration of ketones"; $400.

Martin G. Naumann, University of Kansas. "Biology of the social wasps of Costa Rica (Hymenoptera, Vespidae, Polybiinae)"; $400.

Paul R. Nevergold, State University of New York at Buffalo. "A petrologic study of the Tallen Lake–Duck Lake amphibolite complex"; $300.

Eviatar Nevo, University of Texas. "Geographic variation in the mating call of the cricket frog, *Acris crepitans*, and its evolutionary implications"; $300.

Richard J. Newcomer, University of Maryland. "Serum proteins and hemoglobins of the salamanders of the family Ambystomatidae"; $400.

Tor H. Nilsen, University of Wisconsin. "Relationship of sedimentation to tectonics in Devonian rocks of the Solund area, Norway"; $500.

Gert B. Orlob, South Dakota State University. "Ancient and medieval plant pathology"; $400.

David W. Osgood, Duke University. "Effects of ambient temperature on the development of meristic characters in snakes"; $200.

William R. Pampe, University of Nebraska. "Biostratigraphy of the Devonian rocks of western Colorado"; $200.

E. Paul Papadopoulos, American University of Beirut, Lebanon. "Synthesis of new antimalarial drugs and study of reaction simulating their metabolism"; $300.

Stephen L. Pastner, Brandeis University. "Reactions to reservation disbandment among the Menomini Indians of Wisconsin"; $300.

Elizabeth C. Paulsen, Rutgers University. "The biochemical and central nervous system effects of phenylketonuria"; $600. Hsien Wu & Daisy Yen Wu Award.

Richard S. Peckham, Mt. St. Mary College, Newburgh, New York. "A determination of a non-specific response to pollutants in the guppy, *Lebistes reticulatus*"; $400.

William L. Peters, University of Utah. "Rearing studies of the leptophlebiid genera of mayflies in the southern hills of India (Ephemeroptera: Leptophlebiidae)"; $525.

Martin A. Piehl, University of Michigan. "Comparative morphology of the haustorial attachments of some parasitic flowering plants"; $300.

Joseph M. Piotrowski, University of Western Ontario, Canada, "Melting relations of alkaline rocks"; $450.

Austin P. Platt, University of Massachusetts. "Study of the inter-relationships between *Limenitis arthemis* and *L. astyanax* in New England"; $250.

Charles C. Porter, Harvard University. "A systematic and biological study of the neotropical species of the Mesostenina genus *Trachysphyrus* (Hymenoptera: Ichneumonidae)"; $300.

Rama S. Rai, University of Rajasthan, Jaipur, India. "Photolysis and radiolysis of dilute solutions of metallorganic complexes in liquid phase"; $500.

J. K. Sridhar Rao, University of Minnesota. "Elastic analysis of continuous flat slabs with drop panels"; $500.

Jeremy Reiskind, Yale University. "Paleobiology of the marine molluscan faunas of the Bearpaw formation (upper Cretaceous) in southwestern Saskatchewan"; $500.

Frederick Reiss, Montefiore Hospital, New York. "Role of endogenous Vitamin K as growth-inhibiting factor of the intestinal *Candida albicans* flora"; $1,200.

William R. Reynolds, Florida State University. "The nature and controls of the development, and the genesis of the lower Tertiary bauxite of southeastern Alabama"; $225.

John Riva, Villanova University. "Study of the micro-structure of the thrusts on China Mountains and the H. D. Range southeast of Contact, Nevada in order to interpret their direction(s) of movement"; $250.

Janet Robbins, Brooklyn College. "Probability learning in the T-maze under a correction procedure as a function of arm length"; $500.

Landon T. Ross, Florida State University. "The genus *Crepidula* in North America and its evolution and distribution"; $200.

Charles L. Rowett, University of Alaska. "Paleontology of upper Paleozoic Rugose corals from the eastern Alaska range"; $600.

Michael K. Rylander, Tulane University. "Anatomical and physiological study of *Erolia* (Aves; Charadriiformes; Scolopacidae)"; $269.50.

Timothy E. Saylor, Western Reserve University. "Geology and geochronology of the Placerville-Pioneerville area, Boise Basin, Idaho"; $450.

Robert W. Seabloom, University of North Dakota. "Ecological and taxonomic study of the genus *Sylvilagus* in North Dakota Badlands"; $400.

Charles R. Sewell, University of Texas. "Geology of Candela Belt of igneous intrusions, Nuevo Leon and Coahuila, Mexico"; $600.

Gretchen Schabtach, Johns Hopkins University. "Interhemispheric relations in the chicken as indicated by experiments in interocular transfer"; $500.

Ralph W. Schreiber, University of Maine. "Correlation of environmental conditions with daily movements of gulls (Laridae) in vicinity of University of Maine"; $150.

Jack W. Shields, University of Minnesota. "Fluorescence microscopy of pol-

len development in *Ornithogalum L.*"; $500.

Morgan E. Sisk, Jr., Murray State College, Murray, Kentucky. "A survey of the fishes in West Kentucky with keys to the species"; $200.

Russell E. Siverly, Ball State Teachers College, Indiana. "A systematic and biological study of mosquitoes of Indiana"; $500.

Dorothy Skinner, New York University School of Medicine. "The nature of ribonucleic acid in growing muscle"; $700.

Richard E. Smith, Pennsylvania State University. "Relationship between certain petrographic variables and porosity and permeability of the Gatesburg Dolomite"; $200.

Michael E. Soule, Stanford University. "Analysis of phenetic variation and asymmetry of the lava lizards (*Tropidurus*) in Malawi, Southeast Africa"; $500.

Dorothy B. Spangenberg, Little Rock University, Arkansas. "Study of effect of pH change on initiation of strobilation in *Aurelia aurita*"; $500.

Dennis M. Sparks, Michigan State University. "Microfloral zonation and correlation of lower Tertiary rocks of western Washington"; $400.

Allen Spitzer, Saint Louis University. "Investigating Redfield theory of great and little traditions in interaction"; $400.

Donald C. Staiff, North Dakota State University. "Conformational and configurational analysis of some 1,2,3-trisubstituted cyclohexane compounds"; $400.

James R. Steidtmann, University of Michigan. "Stratigraphy and paleotectonic environment of the Hoback and Pass Peak formations in the Hoback Basin, Wyoming"; $400.

Frederick A. Stevens, University of California at Los Angeles. "*Clarkia unguiculata*, an investigation of physiological tolerances during ontogeny"; $300.

Joan G. Stewart, San Diego State College. "Ecology, morphology, and taxonomy of *Pterocladia pyramidale* (Gardner) Dawson"; $350.

James H. Stout, University of Alaska. "The bedrock geology between Rainey Creek and Denali Fault, central Alaska range"; $400.

John L. Strother, University of Texas. "Cyto- and chemo-taxonomy of *Dyssodia* (Compositae)"; $325.

Jeffrey C. Sutherland, Syracuse University. "The biotite-garnet magnesium distribution coefficient and its application in metamorphic geothermometry"; $200.

Roderick A. Suthers, Harvard University. "Sensory physiology and behavior of three tropical bats"; $600.

Robert M. Sweazy, University of Wichita. "Comparative ecology of two streams"; $300.

Michael J. Switek, Jr., University of Oregon. "Stratigraphy and structure of the Elkhorn Ridge Argillite, Elkhorn Ridge area, Oregon"; $300.

James R. Tamsitt, Southern Conn. State College, New Haven, Conn. "A study of Colombian and other Neotropical bats (order Chiroptera)"; $300.

Allan M. Thompson, Brown University. "Stratigraphy and facies relations of the Bald Eagle formation, central Pennsylvania"; $300.

James L. Throne, Ohio University. "Controlled cycling in a packed distillation column"; $400.

Richard L. Timken, University of South Dakota, "Ecology and distribution of *Graptemys pseudogeographica* in the upper Missouri River"; $250.

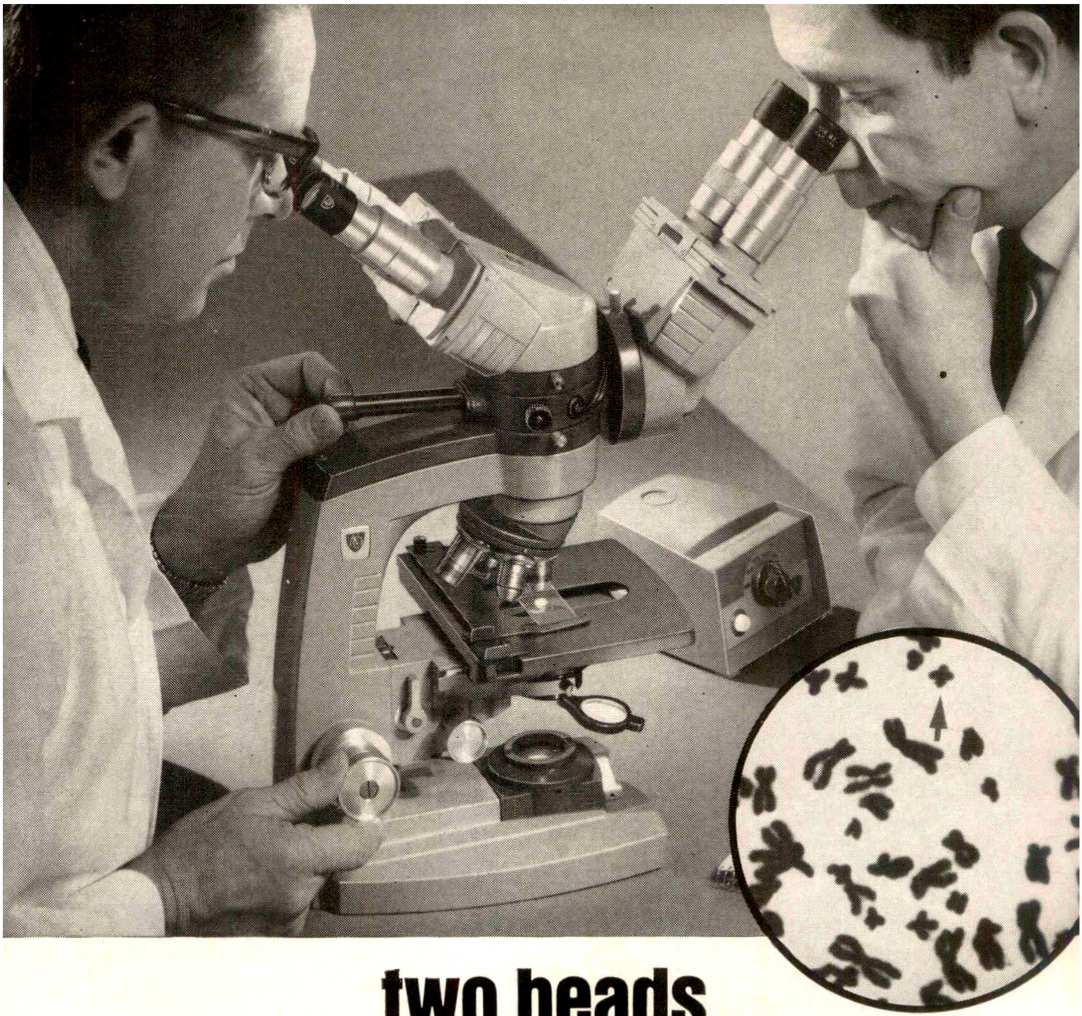
Steven E. Toth, Bowling Green State University. "Electrophoretic analysis of proteins in *Podocoryne carnea* before and after carbon dioxide-induced sexuality"; $200.

Matsuo Tsukada, Yale University. "Pollen morphology and identification; II. Cactaceae; III. Tropical modern and fossil pollen with emphasis on Bombacaceae"; $600.

Milton G. Tunzi, University of California at Davis. "Metabolites controlling algal and bacterial populations in fresh-waters"; $400.

Christy G. Turner, II, University of California at Berkeley. "Estimating prehistoric Hopi Indian population density by ceramic vessel capacities"; $300.

Joseph Vagvolgyi, University of Florida. "Evolutionary studies on the



genus Ashmunella (Moll: Pulm: Polygyridae)"; $400.

Bradford B. Van Diver, University of Oregon. "Geochemical and petrographic investigations of part of the crystalline core of the northern cascades of Washington"; $400.

S. Venketeswaran, University of Pittsburgh. "Studies on isolation of green pigmented callus tissue of tobacco and its continued maintenance in suspension cultures"; $200.

John R. Vogel, Rutgers University. "Effect of interpolated extinction on change in performance following shift in reward magnitude"; $600.

Francois Vuilleumier, Harvard University. "Species formation in Andean birds"; $375.

David B. Wake, University of Chicago. "Functional and evolutionary morphology of the Salamander tongue"; $500.

Alma T. Walker, University of Georgia. "Licheu acids in North American *Cornicularia* species"; $500.

Ruth A. Walker, Hunter College, City University of New York. "Metal chelates of anthraquinone derivatives"; $800.

Kung Tsung Wang, National Taiwan University, Taipei, Taiwan, China. "Chemical studies of constituents of plants indigenous to Taiwan"; $500.

John L. Warner, University of Minnesota. "Studies in semantics and syntax"; $350.

John A. West, University of Washington. "Electron microscopy of sex cells of several red algae"; $300.

Terry R. West, Purdue University. "Outlining boundaries of water-bearing outwash materials in a glacial terrain near West Lafayette, Indiana"; $200.

Richard L. Whitley, Bowling Green State University. "Petrographic and geochemical study of Precambrian granites at Butternut, Wisconsin"; $250.

James P. Wightman, Virginia Polytechnic Institute. "Interactions of atomic species with solids"; $400.

Merle W. Wing, Cornell University. "A taxonomic revision of the Nearctic ant genus, *Acanthomyops*"; $600.

James L. Wolfe, Cornell University. "Ecological study of the eastern chipmunk, *Tamias striatus*"; $375.

Emerson H. Wright, Fort Lauderdale, Florida. "Compilation and periodic arrangement of the binary phase diagrams of the Group VIb elements"; $1000.

Ronald M. Young, Utah State University. "A systematic revision of the western United States species of *Polyphylla* Harris. (Coleoptera, Scarabaeidae, Melolonthinae)"; $275.

Donald J. Zinn, University of Rhode Island. "An annotated checklist of psammic copepods"; $300.

Of interest in connection with the Grants-in-Aid of Research Program is the following letter.

GENTLEMEN:

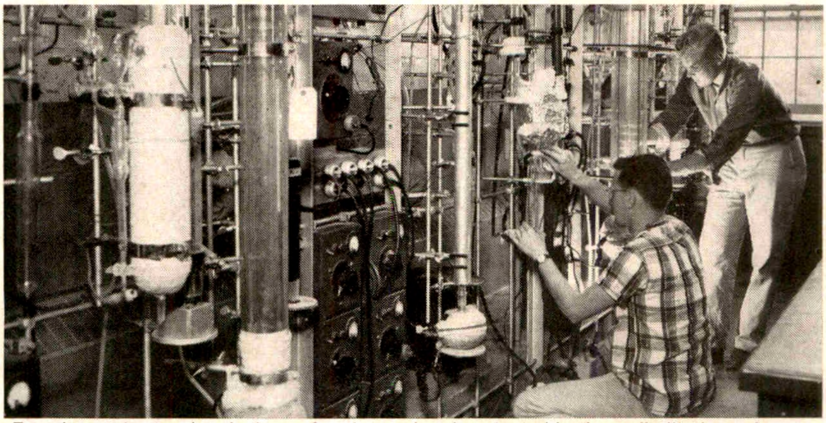
This letter concerns my application for a 1965 Grant-in-Aid of Research. Unless my application has been, or will be, considered at a June meeting of the Committee please return it rather than hold it for future consideration.

I have obtained partial financial support ($500 of a required $1200) from NDEA, under a provision for summer funds of my graduate fellowship. It was not until late May that I learned about this provision.

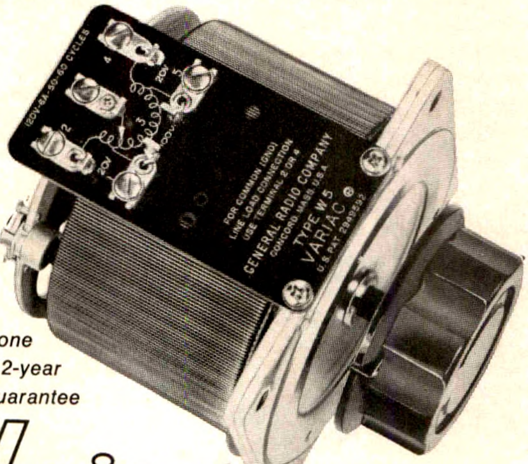
As a member of Sigma Xi I congratulate you for your fine administration of the grants program. The growing number of requests for grants indicates, I think, both a growth in the amount of scientific research which requires modest sums and the great service the Society performs in aiding as much of this research as it has funds for.

Sincerely,

THOMAS R. DETWYLER



Experimental petrochemicals are fractionated and evaluated in these distillation columns at the Texaco Research Center in Beacon, New York. Eight GR Variac® autotransformers are used in this setup — one for each column. Each Variac is electrically connected to the resistance windings of an asbestos-covered heating jacket, and the Variac is precisely adjusted to obtain the desired temperature of the boiling mixture.



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50 GR ANNIVERSARY

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THE EXECUTIVE SECRETARY'S PAGE

The final pages of this issue of American Scientist contain a listing of local chapter and club officers for 1965–66. Every effort has been made to have this compilation as up-to-date and as accurate as possible. It is hoped that this directory will enable Members and Associate Members who wish to establish contact with the officers of a particular chapter or club to do so. It is planned to publish this listing each year in the September issue of American Scientist.

The conversion of National Headquarters to a computerized system of record-keeping and communication is proceeding on schedule. Each chapter and club is, this month, being supplied with a detailed listing of its active membership as of September 15, 1965, and with the request that National Headquarters be furnished with information to correct errors and deficiencies. Since the new system is dependent upon accurate membership data, the cooperation of all Members and Associate Members is urgently requested.

The 66th Annual Convention of The Society of the Sigma Xi will meet at Berkeley, California, on December 29 in association with the Annual Meeting of the American Association for the Advancement of Science. Dr. Jacob Bronowski of the Salk Institute for Biological Studies will deliver the Phi Beta Kappa-Sigma Xi Address on the evening of the 29th. The December issue of American Scientist will contain the complete agenda of business to be presented to the Convention. The agenda of business to be presented to the Convention will be sent to each chapter and club prior to October 29 and will be published in the December issue of American Scientist.

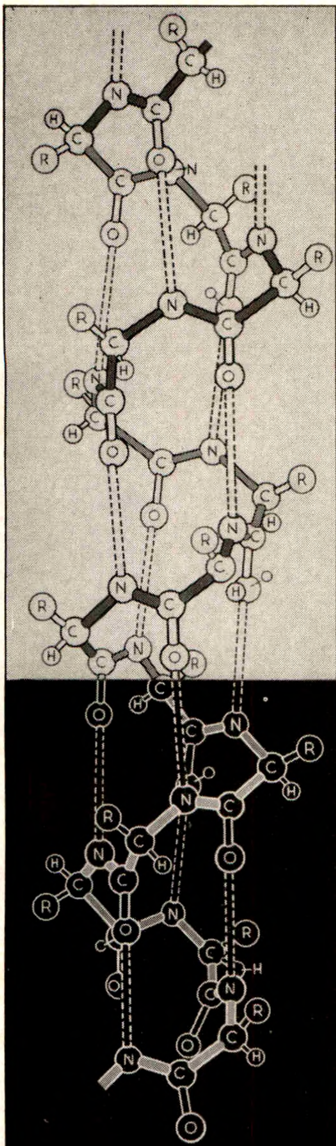
The 1964 Convention approved a National billing of the Society's membership based on an academic year—July 1–June 30—commencing in 1966. In order to facilitate the change-over to the new system, it is planned to bill the Chapter-at-Large this November for one and one-half years' dues—to June 30, 1967. The chapter and club membership will be billed for one and one-half year's dues on or about May 1, 1966, for the period January 1, 1966 to June 30, 1967. Thereafter, the entire membership will be billed for annual dues on May 1 of each year starting in 1967.—T.T.H.

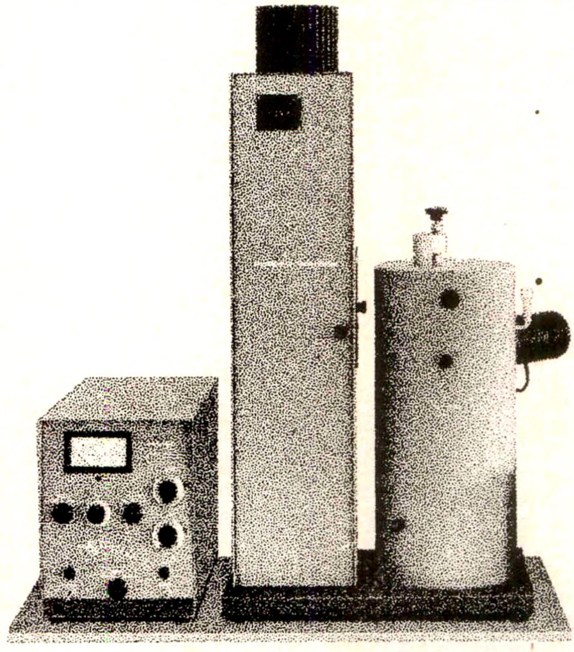
ANNOUNCEMENT

As Chairman of the Committee on Membership-at-Large, it is my pleasure to announce that, under the provisions of Article VII of the Constitution, the Chapter-at-Large has *Promoted* to full membership in the Society:

G. Gordon Connally
Edward Friedman
Ralph Alex Klawitter
Henry H. Kramer
Allen I. Laskin

James H. Marks, Chairman
Committee on Membership-at-Large





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SCIENTIFIC RESEARCH SOCIETY OF AMERICA

As noted in the June issue of the AMERICAN SCIENTIST, RESA has now a branch in Costa Rica. On June 11 a charter was presented to a group of research scientists at San Jose by Dr. Burton W. Jones of Colorado on behalf of the Board of Governors of the Society. Doctor Jones also gave the principal address of the occasion on the subject "Block Designs and their Mathematical Counterparts."

On May 19 a charter was presented by the National Chairman, Dr. Hanford, to the group at the Picatinny Arsenal at Dover, New Jersey. A branch of the Society has been in operation at the laboratory of the Frankford Arsenal since 1949.

On April 2 a charter was presented to the Autonetics Branch at Anaheim, California, by Dean Emeritus Boelter of the Engineering School of the University of California at Los Angeles. Dean Boelter was formerly a member of the Board of Governors of RESA. This installation brought the number of branches in California to twelve, the largest number in any state. New Jersey and New York have eleven branches each.

From January 1 to June 20, 1965, 1246 research scientists have become members of RESA.

Officers and committees as of July 1, 1965:

Finance Committee

D. B. PRENTICE, *Chairman*
W. E. HANFORD
F. R. FAIRCHILD
R. P. SOULE

Budget Committee

D. B. PRENTICE, *Chairman*
W. E. HANFORD
J. F. WEIFFENBACH

Nominating Committee

LEO FLEXSER, *Chairman*
DANA YOUNG
W. O. BAKER

Procter Award Committee

W. E. HANFORD, *Chairman*
D. L. BENEDICT
H. S. TAYLOR
D. B. PRENTICE
W. O. BAKER

Representative on the Lecture Committee

DANA YOUNG

Representative on the AMERICAN SCIENTIST *Board*

J. F. WEIFFENBACH

BEYOND THE HORIZON OF SCIENCE*

By LIENG-HUANG LEE

Since the last World War, science has been recognized to be one of the important human endeavors which can eventually determine the destiny of the entire human race. Many new frontiers, such as nuclear physics, space technology, computation, genetic codes, and human behavior are developing with an accelerated speed. The men behind these frontiers are no longer regarded as second-rate citizens. In fact, the prestige of scientists has been growing steadily. According to a recent report, "Occupational Prestige in the United States: 1925–1962," [1] issued by the University of Chicago, the first nine occupations ranked according to prestige as follows: supreme court justice, physician, nuclear physicist, scientist, government scientist, state governor, cabinet member in the federal government, college professor, and U.S. representative in Congress.

As a result of the rising prestige of both science and scientists, our responsibilities multiply. Each one of us is supposed to play the three well-balanced roles as an employee, as a professional,

* This was presented at the Seventh Initiation Meeting of Midland Branch of the Scientific Research Society of America, January 1965.

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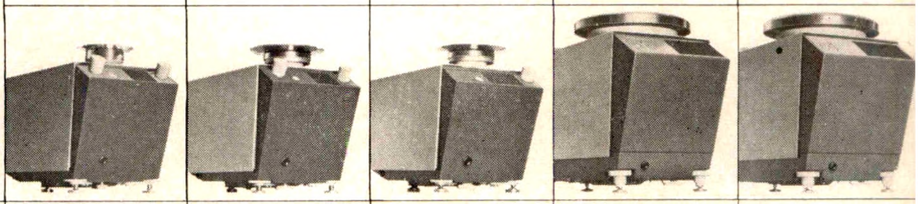

	P120	P1200	P1000	P3	P10
weigh unknowns to...	120 g +10 g tare	1200 g +100 g tare	1000 g +300 g tare	3000 g +2500 g tare	10 kg +3 kg tare
with precision better than...	±0.5 mg	±0.005 g	±0.05 g	±0.05 g	±0.5 g
precision/ capacity relationship of...	1 part in 250,000	1 part in 250,000	1 part in 25,000	1 part in 100,000	1 part in 25,000
checkweigh directly to over-under values from...	+60 mg to −60 mg of target weight	+0.6 g to −0.6 g of target weight	+5 g to −5 g of target weight	+11 g to −11 g of target weight	+50 g to −50 g of target weight
weigh-in to... (Including container)	130 g	1300 g	1300 g	5500 g	13 kg
batch weigh to... (Including container)	130 g	1300 g	1300 g	5500 g	13 kg
measure mass or force below the balance	This is done by simply hanging a suspension device on the hook built into the bottom of the balance. Weighing at some location vertically below the instrument is recommended for work with corrosive, poisonous or radioactive substances in protective compartments, for weighing objects in heating or drying ovens, or objects submerged in liquid.				

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and as a human being. In fact, we constantly find ourselves to be somewhere within the triangle but rarely at the center of the triangle. As industrial scientists or engineers, we are obligated to work toward the management goals where profit is of prime concern. Our freedom in searching the truth is undoubtedly limited. Furthermore, we find the spirit of ivory tower gradually dwindles. In industry, we are honestly finding fewer genuine companions in zealous research. As a result, we gradually lose the balance between being an employee and being a scientist.

To be a professional scientist or engineer is getting more difficult today than it was in the old days. Just think of the information explosion! In some fields one would have to spend over a solid year of reading for the literature published in that year. We find that it is as difficult to be a specialist as to be a generalist. Frequently, there are new fields with which we are not familiar. Truthfully, we are unable to find time to keep up with many new developments.

As was recently pointed out by Dr. Seaborg [2], we cannot today be science wizards, because we have to be human beings, besides being professionals. As human beings, we cannot neglect the other half of the culture: i.e., humanities. How well we play this role determines the destiny of the entire human race. It is not easy to answer the question: "How can I play the three roles with balance?" or "How can I stay at the center of the employee-professional-human being triangle?" However, in my opinion, it may be possible to obtain a solution by raising ourselves above the plane of the triangle. We might not get a solution but truly we need a proper perspective before we can reach any solution.

Science, as defined by Dr. Conant [3], is that which emerges from other progressive activities of man to the extent that new concepts arise from experiments and observations, and the new concepts, in turn, lead to further experiments and observations. We may then visualize science as constituting an ocean of living fruitful concepts. From this living ocean, we draw our power and resources. In return, to this ocean we should deposit our truthful fruits of labor. And to this ocean we should devote ourselves with passion. Truly, we should ask not only what science can do for us but also what we can do for science!

Beyond the horizon of this living ocean, there stand the grand pillars of science: Galileo, Newton, Einstein, Pasteur, Planck, Mendeleev, Madame Curie, and many other immortals. By looking beyond, our productivity and creativity could be stimulated within. And by looking beyond, we may be able to extract the essence of being a scientist from the great saints of science. Shall we set forth to test this hypothesis and become an integrated man of science?

REFERENCES

1. *Science, 145*, 145, Aug. 21, 1964.
2. Seaborg, Glenn T., *Chem. and Eng. News*, Dec. 21, 1964.
3. Conant, James B., "On Understanding Science," Oxford University Press, 1947.

The SIGMA Xi-RESA National Lectureship Program

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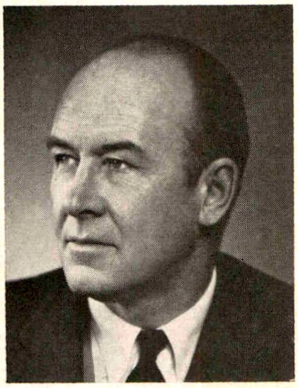

CENTRAL TOUR—FALL 1965

"Some Evidence and Implications of Continental Drift"

DR. J. TUZO WILSON

Director, Institute of Earth Sciences

University of Toronto, Canada
November 1–12, 1965

NOV. 1 University of Notre Dame ΣΞ Chapter, Notre Dame, Indiana
2 Mead Johnson & Company RESA Branch, Evansville, Indiana
3 Wittenberg University ΣΞ Club, Springfield, Ohio
4 Ohio State University ΣΞ Chapter, Columbus
5 Ohio Wesleyan University ΣΞ Club, Delaware

* * *

8 Denison University ΣΞ Club, Granville, Ohio
9 Kent State University ΣΞ Club, Kent, Ohio
10 Oberlin College ΣΞ Chapter, Oberlin, Ohio
11 *Ford Motor Company RESA Branch, Dearborn, Michigan
Wayne State University ΣΞ Chapter, Detroit, Michigan
12 Albion College ΣΞ Club, Albion, Michigan

* *Host for joint meeting.*

METROPOLITAN NEW JERSEY NEW YORK TOUR—FALL 1965

"The Life History of a Cepheid Variable"

DR. CECILIA PAYNE-GAPOSCHKIN

Professor of Astronomy

Harvard College Observatory
Cambridge, Massachusetts
November 8–18, 1965

NOV. 8 Union Carbide Corporation RESA Branch, Bound Brook, New Jersey
9 Hofstra University ΣΞ Club, Hempstead, Long Island, New York
10 U.S. Naval Applied Science Laboratory RESA Branch, Naval Base, Brooklyn, New York
11 The Rockefeller Institute ΣΞ Chapter, New York, New York
12 State University of New York at Stony Brook ΣΞ Club

* * *

15 State University of New York at Buffalo ΣΞ Chapter
16 Cornell University (New York State Agricultural Station) ΣΞ Chapter, Geneva, New York
17 Texaco Research Center RESA Branch, Beacon, New York
18 American Smelting & Refining Co. RESA Branch, South Plainfield, New Jersey

MID-ATLANTIC TOUR—FALL 1965

"The Primate Affectional Systems"

DR. HARRY F. HARLOW

George Cary Comstock Research Professor at the University of Wisconsin and Director of the Primate Laboratory and the Wisconsin Regional Primate Center

The University of Wisconsin, Madison, Wisconsin
October 18–29, 1965

OCT.	18	Carnegie Institute of Technology ΣΞ Chapter, Pittsburgh, Pennsylvania
	19	The Pennsylvania State University ΣΞ Chapter, University Park
	20	Franklin and Marshall College ΣΞ Club, Lancaster, Pennsylvania
	21	Lehigh University ΣΞ Chapter, Bethlehem, Pennsylvania
	22	Villanova University ΣΞ Club, Villanova, Pennsylvania
		* * *
	25	*George Washington University ΣΞ Chapter, Washington, D. C.
		Catholic University ΣΞ Chapter, Washington, D. C.
		Howard University ΣΞ Chapter, Washington, D. C.
	26	Medical College of Virginia ΣΞ Chapter, Richmond
	27	Lynchburg ΣΞ Club, Lynchburg, Virginia
	28	Hollins College ΣΞ Club, Hollins College, Virginia
	29	Virginia Polytechnic Institute ΣΞ Chapter, Blacksburg

* *Host for joint meeting.*

MID-WEST TOUR—FALL 1965

"Oxidation-Reduction Polymers, Genesis of a Research Program"

DR. HAROLD G. CASSIDY

Professor of Chemistry

Yale University, New Haven, Connecticut
October 4–15, 1965

OCT	4	University of Wisconsin ΣΞ Chapter, Madison
	5	Knox College ΣΞ Club, Galesburg, Illinois
	6	South Dakota State University ΣΞ Chapter, Brookings
	7	University of North Dakota ΣΞ Chapter, Grand Forks
	8	University of Manitoba ΣΞ Club, Winnipeg, Canada
		* * *
	11	Drake University ΣΞ Club, Des Moines, Iowa
	12	Northwestern University ΣΞ Chapter, Evanston, Illinois
	13	University of Illinois ΣΞ Chapter, Urbana
	14	Illinois Institute of Technology ΣΞ Chapter, Chicago
	15	Rockford College ΣΞ Club, Rockford, Illinois

NORTHEAST TOUR—FALL 1965

"Mass Spectroscopy—an Old Field in a New World"

DR. ALFRED O. NIER

Chairman, School of Physics, University of Minnesota

University of Minnesota
Minneapolis, Minnesota
October 25–November 5, 1965

OCT.	25	Union College ΣΞ Chapter, Schenectady, New York
	26	Rensselaer Polytechnic Institute ΣΞ Chapter, Troy, New York
	27	University of New Hampshire ΣΞ Chapter, Durham
	28	*Avco Rad RESA Branch, Wilmington, Massachusetts
		Allied Research Associates, Inc. RESA Branch, Concord, Massachusetts
	29	McGill University ΣΞ Chapter, Montreal, P.O., Canada

* * *

NOV.	1	Olin Mathieson Chemical Corporation RESA Branch, New Haven, Connecticut
	2	Hamilton College ΣΞ Club, Clinton, New York
	3	Clarkson College—St. Lawrence ΣΞ Club, Potsdam, New York
	4	Corning Glass Works ΣΞ Club, Corning, New York
	5	Alfred University ΣΞ Club, Alfred, New York

* *Host for joint meeting.*

PACIFIC TOUR—FALL 1965

"Structural and Biogenetic Studies of Arthropod Secretions: some New Directions in the Chemistry of Natural Products"

DR. JERROLD MEINWALD

Professor of Chemistry

Cornell University
Ithaca, New York
October 4–15, 1965

OCT.	4	California State College at Los Angeles ΣΞ Club
	5	University of California at Riverside ΣΞ Chapter
	6	University of California at Santa Barbara ΣΞ Club
	7	California State Polytechnic College ΣΞ Club, San Luis Obispo
	8	Fresno State College ΣΞ Club, Fresno, California

* * *

	11	U.S. Naval Radiological Defense Laboratory ΣΞ Club, San Francisco, California
	12	Oregon State University ΣΞ Chapter, Corvallis
	13	Western Washington Experiment Station ΣΞ Club, Puyallup
	14	San Diego ΣΞ Club, San Diego, California
	15	Occidental College ΣΞ Club, Los Angeles, California

PLAINS TOUR—FALL 1965

"The Technology of Teaching"

DR. BURRHUS F. SKINNER

Edgar Pierce Professor of Psychology

Harvard University
Cambridge, Massachusetts
November 29–December 10, 1965

NOV. 29 University of Nebraska ΣΞ Chapter, Lincoln
30 Boulder Laboratories, National Bureau of Standards RESA Branch, Boulder, Colorado
DEC. 1 Brigham Young University ΣΞ Chapter, Provo, Utah
2 Idaho State University ΣΞ Club, Pocatello
3 Whitman College—Walla Walla College ΣΞ Club, Walla Walla, Washington

* * *

6 Montana State College ΣΞ Chapter, Bozeman
7 University of Utah ΣΞ Chapter, Salt Lake City
8 Colorado School of Mines ΣΞ Chapter, Golden
9 *St. Louis University ΣΞ Chapter, St. Louis, Missouri
Monsanto Science Club RESA Branch, Monsanto Company, St. Louis, Missouri
Washington University ΣΞ Chapter, St. Louis, Missouri
10 Southern Illinois University ΣΞ Club, Carbondale

* *Host for joint meeting.*

SOUTHEAST TOUR—FALL 1965

"Galaxies and Mankind"

DR. HARLOW SHAPLEY

Professor and Lecturer on Cosmography in Harvard University

Harvard College Observatory
Cambridge, Massachusetts
October 18–29, 1965

OCT. 18 Duke University ΣΞ Chapter, Durham, North Carolina
19 Lithium Corporation of America RESA Branch, Bessemer City, North Carolina
20 Clemson University ΣΞ Club, Clemson, South Carolina
21 *Emory University ΣΞ Chapter, Atlanta, Georgia
Communicable Disease Center RESA Branch, Atlanta, Georgia
22 Florida State University ΣΞ Chapter, Tallahassee

* * *

25 University of Florida ΣΞ Chapter, Gainesville
26 Florida Presbyterian College ΣΞ Club, St. Petersburg
27 University of Alabama, Medical Center ΣΞ Chapter Birmingham
28 Mississippi State University ΣΞ Club, State College
29 Atlanta University ΣΞ Club, Atlanta, Georgia

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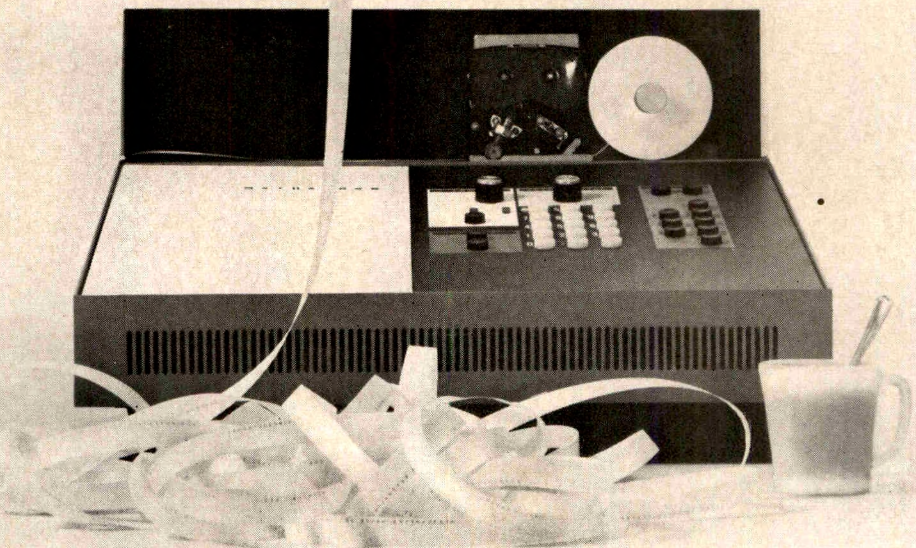

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SOUTHWEST TOUR—FALL 1965

"Explorations of the Food-Conducting System in Plants"

DR. KATHERINE ESAU

Professor of Botany (status of Emeritus, June 30, 1965)

University of California
Santa Barbara, California
October 11–22, 1965

OCT. 11 New Mexico State University ΣΞ Chapter, University Park
12 Texas Technological College ΣΞ Chapter, Lubbock
13 University of Texas Medical Branch ΣΞ Chapter, Galveston
14 Central Texas Research Society RESA Branch, Temple
15 Baylor University ΣΞ Club, Waco, Texas

* * *

18 Oklahoma State University ΣΞ Chapter, Stillwater
19 University of Arkansas ΣΞ Chapter, Fayetteville
20 Texas Christian University ΣΞ Club, Fort Worth
21 *Socony Mobil Dallas RESA Branch, Dallas, Texas
Southwestern Medical School ΣΞ Club, Dallas, Texas
22 University of Arizona ΣΞ Chapter, Tucson

* *Host for joint meeting.*

ETHICAL PROBLEMS OF SCIENTISTS

By LAWRENCE CRANBERG

The relationship of ethics to science has long been discussed as a theoretical problem by a few specialists. In recent years ethical problems associated with activity in science have acquired for many scientists a major importance on the practical level, often without having been tagged explicitly as "ethical," or, in many cases, without having been brought above the threshold of awareness as problems worthy of serious attention. Ten such problems are identified below.

1. The "selling" of research and development proposals whose fate may strongly affect the scientist's career, or even the economy of a whole section of the country, must be reconciled with the traditional responsibility of the scientist to evaluate the defects as well as the virtues of his hypotheses, and with recognition of the need for a balanced development of science within its different branches and in relation to the other demands on the resources of society.

2. Increased intensity of scientific work creates a setting of personal competitiveness and temptations of self-aggrandizement which must be reconciled with traditions of courtesy, of open communications, of full opportunities for expression of dissident views, and of accurate and adequate acknowledgment of the contributions of others.

3. Demand for rapid marketing under competitive conditions of an increasingly wide range of technically novel materials and devices must be reconciled with maintainance of standards of workmanship and care in determination of the consequences of rapid introduction into general use of those materials and devices.

4. The increasing specialization of science and the demands for highly visible research productivity in highly specialized areas, must be reconciled with the aspiration of science to infuse a spirit of reason into all the affairs of men, and with the obligation to secure a competently trained, properly motivated citizenry, and scientific posterity.

5. Increasing managerial and administrative power in the hands of scientists unfamiliar with the uses of such power creates opportunities and temptations for arbitrary wielding of authority which must be reconciled with traditional emphases on appeal to reason and on courtesy among colleagues.

6. Increasing attention and respect accorded the advice of scientists on a wide range of policy questions generate temptations of self-assertion which must be reconciled with the traditional obligation to furnish evidence and carefully weighed limits of error with every prediction.

7. The advent of expensive large-scale research activity imposes obligations for economical use of large resources which must be reconciled with the demands of flexibility and freedom which are intrinsic to research.

8. The increasing importance of group effort in science, which carries with it an obligation for coordination and appreciation of contributions at many levels of skill, must be reconciled with the continuing importance of distinctive contributions by highly talented and motivated individuals.

9. The increasing importance of science is reflected in the assumption of new roles of social and scientific importance by the professional organizations of scientists. The effectiveness of these organizations in their new roles is a responsibility of the many, which must be reconciled with a long-standing tendency in the scientific community to delegate authority heavily to a few, with a minimum of surveillance or interest on the part of the many.

10. The continuing tension of international relations requires that scientists reconcile the demands of national security and prestige with the openness of communications required for scientific progress, and with recognition that the

essential function of science is to serve as a mechanism of adaptation to the requirements of survival of the human species.

These are some of the problems of decision, frequently novel in difficulty if not in kind, which many scientists face today. Relating as they do to choices which involve the balancing of short-range personal advantage against social interests, or national against international interests, they are conventionally designated "ethical" when there is no question of violation of law. Aspects of these problems are sometimes characterized as "conflicts of interest."

A common element in many of the areas of decision identified above is the inherently low visibility of the situations in which the decisions are often taken, and the absence of a consensus with respect to the norms which should govern the decision-making process. Prompt and full exposure to public scrutiny via publication, which is one of the most effective devices in science, as elsewhere, for inducing the correction of error, is often precluded. It seems prudent, therefore, to examine the need for clarification of norms and for means of detecting and dealing with "error" in these frequently novel, low-visibility situations. Interests of scientists, scientist-educators, professional associations of scientists, and of society as a whole are vitally involved.

Problems having similar ethical and, sometimes, similar substantive content have long been encountered by members of other occupational groups—for example, lawyers and engineers. After study and trial—and often under public pressure—these, and many other occupational groups, have adopted special administrative and educational devices to observe and improve ethical performance in their occupational domains. Despite the widespread occurrence of these devices, little is known about their effectiveness, about how they may be improved, or about their relevance to the needs of scientists. Scientists now have a personal interest in encouraging research in these areas of ignorance; they offer direct challenge to students of behavioral science—in particular to the disciplines of rationalized decision-making, conflict resolution, and descriptive sociology, and are clearly of interest to scientists generally.

Concern with ethical problems can itself produce ethical problems. Among these is the possibility of infringing on the privileges of privacy. Another is a temptation toward overemphasis on the ethical aspects of intrinsically complex questions whereby ethical argumentation becomes a means of question-begging, oversimplification, or intimidation. These possibilities illustrate the need for great care and alert self-criticism if ethical problems are to be studied and dealt with in a competent manner.

Many of the ethical problems of scientists clearly parallel those of other occupational groups. When scientists examine ethical problems with their own investigatory tools, however, those problems become part of the unfinished business of science itself. The nature of the relationship between science and ethics has long been a subject of speculation and controversy. Systematic empirical study of operating systems of ethical self-regulation may contribute to the clarification of this relationship. Such study should furnish a basis for evaluating and possibly extending and improving those systems.

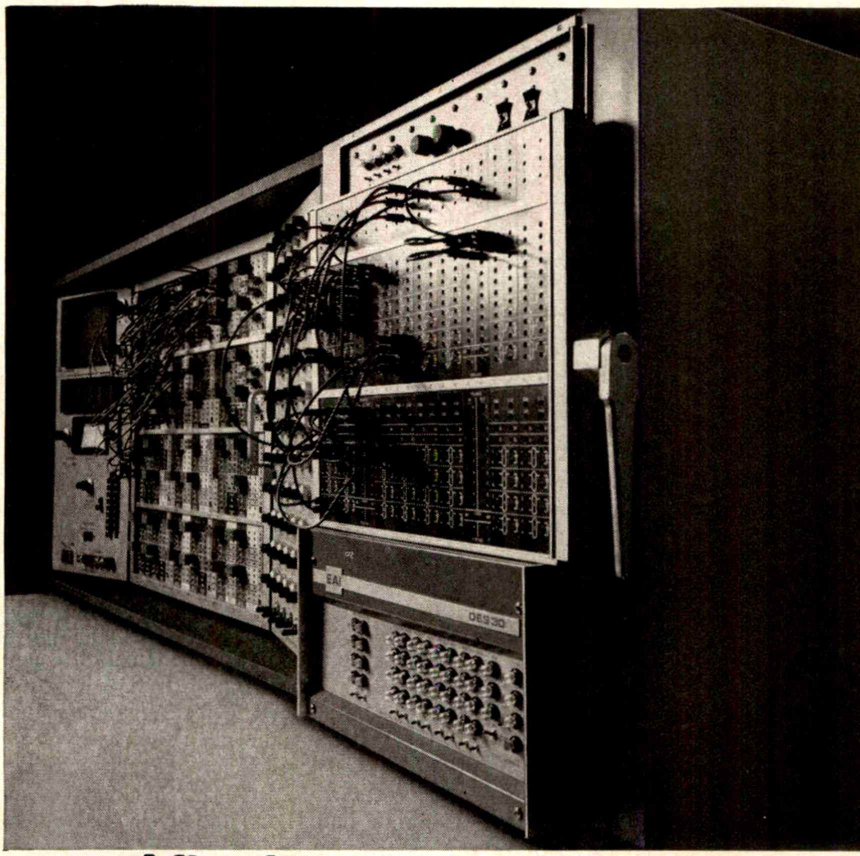
Reprinted from *Physics Today, 18,* No. 4, pp. 51–52 (1965), with permission.

LETTERS TO THE EDITORS

GENTLEMEN:

In his article on cyclic population variations of lemmings, Clough [1] cites evidence for a population periodicity of 3–4 years, and he recounts his own observations of the spectacular changes in lemming population densities. Clough then recalls three types of theoretical models capable of accounting for the population periodicity; external environmental cycles, "feedback" in the interaction with the environment, and internal self-regulatory mechanisms.

We would like to point out that Cole



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- New expansion to 58 amplifiers for more problem-solving capacity.
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- New transistorized Digital Voltmeter improves accuracy (4½ place readout) with BCD output available.
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- New Packaged Limiter simplifies programming of problem constraints.
- New Sin/Cos Generator for convenient programming of trigonometric functions.

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and Kac[2] have proposed a fourth alternative; that the 3–4 year cycles are purely subjective artifacts of the observation technique, that no periodic regulation or feedback control is active, and that the population is a purely random phenomenon. This stochastic model was extensively discussed in a Symposium on Cycles in Animal Populations.[3] Consider a discrete sequence of random numbers which, in the present problem, is presumed to correspond to the annual population of the lemmings. Each number may assume a continuous range of values, according to some arbitrary probability distribution. Suppose the numbers are plotted as ordinates, at unit intervals along the axis of abscissae, and joined by a simple smooth curve. It is easily seen that if one determines the periodicity (a) by counting maxima, the period will be 3 years, or (b) by counting nodes, the period will be 4 years.

(a) Any given point will be a local maximum if it exceeds in value the two points flanking it. But since any of the three points may be the greatest (with equal probability) the probability of the given (central) point being a maximum is one-third. Thus one third of all points are local maxima and the mean distance between local maxima is 3 units. (b) First diminish each number by the mean in the distribution. Then a pair of successive points may have any of the four combinations of signs, with equal probability: $(+\ +)$, $(-\ -)$, $(-\ +)$, $(+\ -)$. The last two of these correspond to nodes. Hence the probability of a node of positive slope $(-\ +)$ is one fourth. Therefore the mean distance between nodes of positive slope is 4 units.

As Cole points out, the argument does not imply that fluctuations in animal population are not causal but only that the influences are so complex that they combine to produce a purely random variation which subjectively appears to be cyclic. This apparent periodicity is not restricted to zoological problems. In fact our interest in this matter arose from a study [4] of the magnetization variation in thin films used as computer memory devices.

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Dear Drs. Doyle and Callen:

I thank you for the chance to comment on a widespread misunderstanding. I used the term cycle to describe some regular, but not rigid, periodicity in the fluctuations of animal numbers. By this usage a series of populations either is or is not cyclic. In itself, such a description does not carry with it any explanation of the mechanisms or the causation of the changes in numbers. Most ecologists accept the lemming records as 3- to 4-year cycles and the arctic hare, lynx, and arctic fox records as ten-year cycles.

In 1949 P. Palmgren in Finland, and later Cole, pointed out that a series of random numbers generates a cycle which resembles that formed by northern small mammal populations. They then hypothesized that the causes of the population fluctuations vary in a random manner, but they did not examine the causes any further. In my paper in the June issue of American Scientist I listed efforts to explain the mechanisms involved in the observed cyclic fluctuations of small mammals. For convenience I arranged these into three groups. I do not think that Cole's ideas form a fourth group because he did not attempt to answer the question of mechanism or causation of the cycles he discussed.

In respect to the random model which you use, I offer these comments. Often lemming density has been recorded as either large or small, or as high, medium, or low. L. B. Slobodkin, in his book

"Growth and Regulation of Animal Numbers" (1964, Holt, Rinehart and Winston) shows that, in the first case, theoretically only one out of the eight possible three-year sequences is a maximum, i.e., small, large, small. This has a $\frac{1}{8}$ probability of occurrence and thus, the "mean cycle length in a population of this sort is eight years." Similarly, in the case of three recognizable levels of abundance, Slobodkin continues, there are five out of 27 possible three-year sequences which represent maxima, and the mean cycle length will be 27/5 or 5.4 years. As the range of levels expands to 100 the mean cycle length essentially reaches three. This corresponds to the case presented by you. Refinement of census techniques for almost all wild populations is far from this point. Significant population variables for discussing rates of change also include differential age structure and physiological condition.

In a random series one number does not exert any influence on its neighbors. In animal populations there is direct biological continuity from one sampling period to the next. The influence of the present population on the future population will be increased in accord with the degree of density-dependency or intrinsic control, but it will always be present to some extent. This was taken into account by Cole and Kac, in the reference you cite, by application of different degrees of smoothing to two-point moving averages.

The problems are: what are the mechanisms of the fluctuations or what are the relative roles of the various factors which influence population numbers? Are certain mechanisms primarily checks on dangerous overabundance or are they basic causes of the cycles? The responsible factors certainly may be complex, as Cole has suggested and as I emphasized. They may vary from species to species, from time to time they may combine mechanisms of all three groups of hypotheses. But complexity of cause does not necessarily follow from the resemblance to a random series, which may be coincidental.

We are able to predict on the basis of past records that the lemming populations of central Norway will be high in 1966 or 1967. We still do not know why. The hypotheses which I listed all attempted to answer that question, however unsuccessful some of them were and unproven others are. The statement that "the population is a purely random phenomenon" provides no basis for further investigation.

Sincerely,

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The assertion by Clough in his letter that population readings on a two-value (large, small; or +, −) scale lead to a mean cycle length of eight years is in error. In fact, it is easy to see that in this case for every "node of positive slope" (as defined in our letter) there is precisely one maximum. Consequently, the mean distance between maxima must be four units. The erroneous value of eight arises from ignoring maxima with flat tops, e.g., large, large, small. Furthermore, any other scale (such as the three-value high-medium-low scale) is intermediate between the two-value scale and the infinite-value (continuous) scale, and therefore gives a cycle length between three and four units.

W. D. Doyle
H. B. Callen

Dear Sirs:

I should like to comment on the report by the AAAS Committee on Science published in the June 1965 issue of the American Scientist under the title, "The Integrity of Science."

1. I would have assumed that "integrity" cannot be possessed by the subject matter, Science, but must be possessed by the scientists personally. As more and more scientists are appointed to administrative and policy-making positions, the demands on their persons are increased, and conflicts may indeed arise between their integrity as scientists (duty to science) and the demands, real or assumed, of their positions. Examples could be cited where, e.g., under the stress of war, "scientific"

reports were produced under instructions that conflicted with integrity as scientists. This, of course, is a very serious matter.

2. I believe that Section III of the AAAS report is in part misleading and is unfair to the NASA scientific staff. As Chief Scientific Investigator on the Ranger project, I have had the opportunity to participate in numerous NASA meetings, both at headquarters and at various field centers. The several scientific programs, Ranger, Surveyor, Orbiter, were projected in a sequence of increasing technological complexity, with the scientific objectives always clearly defined. Once the governmental decision was made, in 1961, to proceed with Project Apollo, it was natural that its tremendous requirements would be considered as the details of the scientific missions were periodically reviewed. I want to state categorically, however, that the scientific objectives of Ranger were never compromised by the technological requirements of Project Apollo; quite the contrary, the Associate Administrator in charge, Dr. Newell, specifically instructed the scientists to let scientific merit be the sole criterion in the selection of the Ranger VII, VIII, and IX target sites. Quite naturally, the representatives of the Apollo program were given the opportunity to present their interests; but the decisions were made on the basis of scientific promise only, in accordance with Dr. Newell's instructions. It is regrettable that so many scientists have ignored the obvious warning contained in Dr. Jerome Weisner's statement quoted on page 182, reference 21. Obviously, once a decision has been made of the magnitude of Project Apollo, an enormous program of preparation, mostly technological, is required that cannot always wait at every step for the desired scientific background information; though in fact it is likely that much of the essential engineering data will be available in time from the lunar scientific programs referred to.

The conclusion on page 184, central paragraph, "... sacrifice the advantages of free development of basic scientific research ..." is therefore unwarranted and unjust. Equally unfair is the sentence later on this page, "If the scientific community is subjected to pressure or blandishments ..." which follows a quotation from Dr. Newell. The NASA statement is merely an obvious statement of fact; the over-all program is too massive to be halted for lack of any one item of specific detail.

3. In reflecting on current threats to the integrity of scientists, it seems to me that one is not so much concerned with occasional errors in judgment by scientists in policy-making positions (though such errors are of course regrettable); because such errors usually lead to clearer definitions of the issues involved and thus ultimately strengthen the integrity. Also, university scientists cannot fail to be impressed by the high standards of integrity of many scientists in government service. Rather, the competition for grants and contracts, both within universities and in industry, has led to some practices that involve the integrity of the scientist much more directly. The absence of literature on this subject is no measure of its importance. Who does not know about bright young Ph.D.'s who forego university careers to become professional project writers; or about ambitious men driving scientific ideas beyond their point of usefulness in building research organizations. NASA is probably as much a victim of this growth as any organization, due to the loosely-defined boundaries of Space Science, its wide appeal, and the sheer impossibility to review competently and in detail the torrent of incoming proposals. If integrity of the scientist is called for, would this not be an area of concern? Or might not the scientist take a critical look at his own subject and ponder how it may develop during the remaining part of this century? He might then conclude that the opportunities for science stimulated by the political pressures of this period are challenging and often extremely potent, and deserve more than negative criticism.

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DEAR SIRS:

The very significant AAAS report on the *integrity of science*, reproduced in AMERICAN SCIENTIST, *53*, 174, 1965, has prompted me to submit for publication in your letter columns the following thoughts with which I have been plagued for some time:

The question, barely touched upon and in passing only in the last paragraphs of the above report, is the crucial one of the *integrity of the scientist.* In this respect I wish to register strong disagreement with the point of view of its authors. To stress the importance of the integrity of a *system* quite apart from that of the human scientists who created it, is I think, misleading. There is the tacit assumption (inherent also in the very language of the report) among scientists and perhaps non-scientists, that the individual who chooses science as his life's occupation is bound by certain standards of personal integrity dictated by the history of science, standards which are akin to the vows of the medieval knight.

In the "good old times" when the devotees of the scientific method were a very small minority whose pursuits and product had minor social impact, those who "broke their vows" were quickly discovered and ostracized by their colleagues—and if not, not much harm done! Obviously the situation today is radically different as made very clear by the AAAS report. There is no doubt at all that today an irresponsible and ambitious scientist, through proper "salesmanship" and other socially acceptable devices, can very well—and does—have his ideas accepted and implemented, over and above those of a more responsible and equally competent (but self-effacing) colleague. There is no need to elaborate on the consequences. (See for example "Ethical problems of scientists—a summary" by Lawrence Cranberg in *Physics Today*, *18*, No. 4, p. 51, 1965, reprinted in this issue p. 302A.)

There is, in my opinion, a striking parallel between genuine science versus big, organized science (the new religion) on the one hand and the genuine religion of the Christian proto-fathers versus that of the organized big church that emerged when the new faith engendered vast social impact. I shall not dwell on the parallel but merely direct the reader to some of the acute insights of Brooks Adams in his admirable *The Law of Civilization and Decay.* Omitting several links in an obvious chain of thinking, one can make the following aphorism: *Ambition for power and the pursuit of vested interest will tend to replace scientific objectivity and dedication to truth as the driving force behind the efforts of the individual scientist.* Pushing the parallel to the extreme, unless the present trends are somehow corrected and reversed, the following steps in the self-destruction of *science as a system* through failures in the moral fiber of its human practitioners, may be postulated:

1. Certain fraudulent "scientific" notions gain ascendency by dint of proper promotion on the part of otherwise "recognized and established" individual scientists or groups. The government implements these notions with all but disastrous consequences for the nation and perhaps humanity.
2. The government, with the advice and consent of the reigning *scientific establishment,* institutes a supreme scientific-judicial body—a kind of Scientific Inquisition—to pass judgment on the legitimacy (I almost said orthodoxy) of proposed scientific schemes, in order to protect itself against the recurrence of similar disasters. The Scientific Inquisitors, protected in their independence by life appointments or other devices, become the official arbiters of *scientific dogma* and their judgments are not subject to appeal.
3. All genuine and free scientific inquiry ceases to exist. The scientific system becomes a hollow form, devoid of substance or meaning.

Hoping that the above is only a nightmare contrived by one pessimistic scientist,

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DEAR SIR:

The problems of integrity described in the AAAS Committee Report are, in my opinion, problems of technology not of science. Scientists cannot maintain their scientific integrity if they expect to act also as technologists. Science is the discovery and organization of the properties of the physical world and is not the development of jam-proof military communications systems or even atomic explosives. Technologists can have integrity too, but the same people cannot simultaneously meet the standards of science and technology. The reason is that the purposes of technologists and scientists are different and, in many cases, mutually exclusive. Defense technologists must win wars. Secrecy is one of their tools. Chemical technologists must sell chemicals. Keeping them out of the water supply is not a problem until it is shown to exist, which it now does and is now being solved.

The idea that technological application of science should proceed only after "the orderly acquisition of the related basic knowledge" is a misunderstanding common to scientists but far from the true facts of life. Technological developments start with an idea which may or may not be based on a known scientific principle. They are typically carried off by an enthusiast who believes in his idea, often in spite of the fact that it can be demonstrated by detailed analysis that it will not work, and who, through hard work, luck, and the help of others, overcomes the barely surmountable difficulties that at the start he never expected to appear.

WILLIAM M. BROBECK

The AAAS committee replies to these letters thus:

The whole point of the "Integrity of Science" report was that we *cannot* separate technology from scientific principles. Large scale technological enterprises are, in fact, experiments with the whole of our environment. When "defense technologists" and "enthusiasts" ignore this, as in the testing of nuclear weapons, there are unforeseen effects whose gravity cannot be estimated even years after the fact.

Scientists and technologists are, of course, very often people with different trainings and different jobs. There is no need for the same people to "simultaneously meet the standards of science and technology." The very power of scientific methods is in the fact that they do not depend on the virtues or capabilities of particular individuals. The principle of free and open debate within the scientific community is the self-correcting mechanism by which science approaches the truth. Brobeck is quite right in pointing out that there is a conflict between this principle and the idea that "secrecy is one of (the) tools of defense technology." In deciding whether it is the principles of science, or the operations of technology, which need revision, we must bear in mind the very considerable dangers of weakening the capability of science to predict the consequences of our actions, to serve society in the search for knowledge, and to yield results useful to technology.

That there is no conflict, *at present*, between traditional scientific procedures in the Ranger program and the admittedly nonscientific goal of getting a man to the moon by 1970 is, however, immaterial to the general argument of this Committee's report. Dr. Kuiper says, with reference to Apollo, "the overall program is too massive to be halted for lack of any one item of specific detail." On this basis we believe that he would agree that should a conflict arise between the scientific content of a particular Ranger shot and the overall goal of Apollo, the latter will prevail, and in the context of Kuiper's argument *should* prevail, for the Apollo program "is too massive to be halted," by scientific considerations.

The danger here, of course, is that in a program of the size of Apollo, an increasing proportion of scientific research, and scientific manpower, will be harnessed to tasks determined by a political mission, rather than traditional scientific considerations. Dr. Wiesner's reminder that there are serious social, political, and military justifications for proceeding with such a program should

certainly be borne in mind as Kuiper suggests; we wish only to point out that there is another face to the coin. There are grave consequences for science and society in the erosion of the integrity of science. These too must be borne in mind.

Concerning Dr. Kuiper's comments on the personal integrity of scientists, we can only say that this subject was outside the scope of the Committee's report. The use of the term "integrity" was quite clearly defined: "We shall refer to these processes and to the organization of science on which they depend as the *integrity of science.*" It is far from unusual to use the word integrity with reference to "subject matter." The word denotes wholeness and completeness in such a context.

AAAS Committee on Science in
the Promotion of Human Welfare

GENTLEMEN:

Isn't Mr. C. E. Pepper's most readable note (June 1965) entitled "A new way to teach science in secondary schools" a new ghost of "progressive education" returned to haunt us? A sort of do-it-yourself science? To use an analogy, if I, for one, wanted a champion race horse that might bring a fine "return" (or even a child who might become a champion in some beneficial activity) I wouldn't lead the horse to a race track turn him loose, and then let *him* decide what is excellent racing; on the contrary, I would find the best task-master available for the job—for miseducation is most likely more dangerous to everybody concerned than no education. It seems to me that researchers can make, eventually, re-evaluations of all established norms only if they have learned the established paradigms *first*. This is effectively realized, I believe, by strong interaction processes between students and teachers. To use another analogy, with strong interactions, "parity" between teachers and students is conserved, but with weak interactions "parity" is not conserved, in general. On the other hand, it seems that the strength of interaction and the period of interaction are inversely related.

Finally, his trinity "Time, Space, and Matter" seems, to me, to be a strange particle to absorb, by itself, since it involves primarily empirical subjects without *any* emphasis on logical organization of those results. Mightn't "Time, Space, Matter, and Number" or some related combination be even more appropriate?

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Classical Electromagnetic Radiation, 479 pages; $10.75; *Classical Dynamics of Particles & Systems* by J. B. MARION; 576 pages; $11.50.

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Progress in Dielectrics, edited by J. B. BIRKS & J. HART; Vol. 6; 334 pages; $14.50.

Statistical Theories of Spectra: Fluctuations, edited by C. E. PORTER; 576 pages; $9.50 cloth; $5.95 paper. A Collection of Reprints & Original Papers. Vol. I of *Perspectives in Physics*—A Series of Reprint Collections.

Industrial Wastewater Control, edited by C. F. GURNHAM; 476 pages; $16. Vol. 2 of *Chemical Toxicology* Monograph series.

Optical Circular Dichroism: Principles, Measurements, & Applications, by L. VELLUZ, *et al.;* translated from the French by J. MacCordick, 247 pages; $10, Verlag Chemie.

The Evolution of Genetics by A. R. RAVIN; 216 pages; $6 cloth; $2.95 paper.

Human Chromosome Methodology, edited by J. J. YUNIS; 258 pages; $8.50.

International Review of Cytology, Vol. 18, 1965, edited by G. H. BOURNE & J. F. DANIELLI; 248 pages; $16.

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The Upper Atmosphere, Meteorology and Physics by R. A. CRAIG; 509 pages; $12; Vol. 8 of International Geophysics Series.

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Progress in Radio Science, 1960–1963, Vol. 8: Space Radio Science, edited by K–I MAEDA & S. SILVER; 235 pages; $13.50. 14th General Assembly, URSI, Tokyo, Sept., 1963.

Probability & Statistics by H. FREUDENTHAL; 139 pages; $5.

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Introduction to Paleoecology by R. F. HECKER; 166 pages; $7.50.

Principles of Astronautics by M. VERTREGT; 2nd rev. edn.; 339 pages; $9.

The Assessment of Scientific Speculation by R. A. R. TRICKER; 200; $6.

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Interpretation of NMR Spectra, An Empirical Approach by R. H. BIBLE, JR.; 150 pages; $12.50.

Nodes & Weights of Quadrature Formulas, 16-Place Tables by A. S. KRONROD; 143 pages; $12.50.

Soviet Advances in Nuclear Geophysics, edited by F. A. ALEKSEEV; 189 pages; $27.50 paper.

Electronics Dictionary (German - English, English - German) by C. J. HYMAN; 182 pages; $14.

Soviet Research in New Semiconductor Materials, edited by D. N. NASLEDOV & N. A. GORYUNOVA; 121 pages; no price given; paper.

The General Theory of Sorption Dynamics & Chromatography by V. V. RACHINSKII; 90 pages; no price given; paper.

High-Temperature Compounds of Rare Earth Metals with Nonmetals by G. V. SAMSONOV; 280 pages; $17.50.

Materials Science Research, Vol. 2, edited by H. W. OTTE & S. R. LOCKE; 319 pages; $13.50.

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From **Thomas Y. Crowell, Co.:**

Mathematical Entertainments, A Collection of Illuminating Puzzles, New & Old by M. H. GREENBLATT; 160 pages; $4.95.

From **Doubleday & Co.:**

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Solid Organoalkali Metal Reagents (A New Chemical Theory for Ionic Aggregates) by A. A. MORTON; 248 pages; $14.50 reference edition; $7.50 professional edition, 1964.

The Atomic Nucleus by M. KORSUNSKY; 454 pages; $12.50.

Irreducible Representation of the Space Groups by O. V. KOVALEV; 160 pages; $7.

Engineering Physical Metallurgy by Y. LAKHTIN, translated from Russian by N. WEINSTEIN; 485 pages; $14.50

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Torchbooks/The Science Library Series: *A Sophisticate's Primer of Relativity* by P. W. BRIDGMAN; 164 pages; $1.35 paper; *Symbols, Signals & Noise, The Nature & Process of Cummunication* by J. R. PIERCE; 305 pages; $2.25 paper.

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* The first 10 titles under Harper & Row were mistakenly attributed to another publisher in the June 1965 issue.

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Ergodic Theory in Statistical Mechanics by I. E. FARQUHAR; 235 pages; $12. 1964. Vol. 7 of *Monographs in Statistical Physics & Thermodynamics.*
Error in Digital Computation, Vol. I, edited by L. B. RALL; 324 pages; $6.75. Proceedings of Army Mathematics Research Center Seminar, University of Wisconsin, October 1964.

Report from

BELL LABORATORIES

Programmed Measuring Set for High-Quality Communications

In a Long Distance telephone office, hundreds and often thousands of circuits must be maintained at prescribed levels of transmission quality. Test equipment and test procedures therefore must be such that preventive maintenance will ensure high-quality communications channels whenever customers need them.

At Bell Telephone Laboratories there is a continuing program under way to improve such test procedures and equipment by taking advantage of the latest advances in technology. One of the results of this program is a new test set developed by Bell Laboratories for use in the Bell System. Much of the memory and logic required for the tests has been built into this push-button programmed set. With it, a man can make accurate measurements at speeds ten times faster than possible with earlier test equipment. It also greatly simplifies equipment alignment when necessary.

The tests are performed by measuring transmission "pilots"—special tones interspersed throughout the frequency band containing the channels. Instead of following a complicated series of tuning and measuring steps involving several pieces of equipment, the craftsman now rapidly sets up the test to be made by pushing buttons, performs simple balancing steps, and receives a digital readout of any deviation from prescribed values.

The entire test, including connections, takes only a few seconds. The test can be performed—and any required adjustment made—with the telephone equipment in service, without disturbing any conversations under way.

Bell Telephone Laboratories
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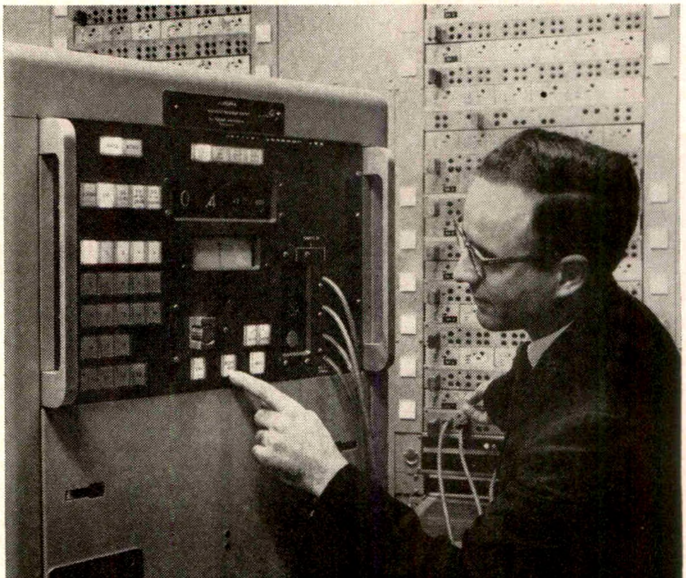
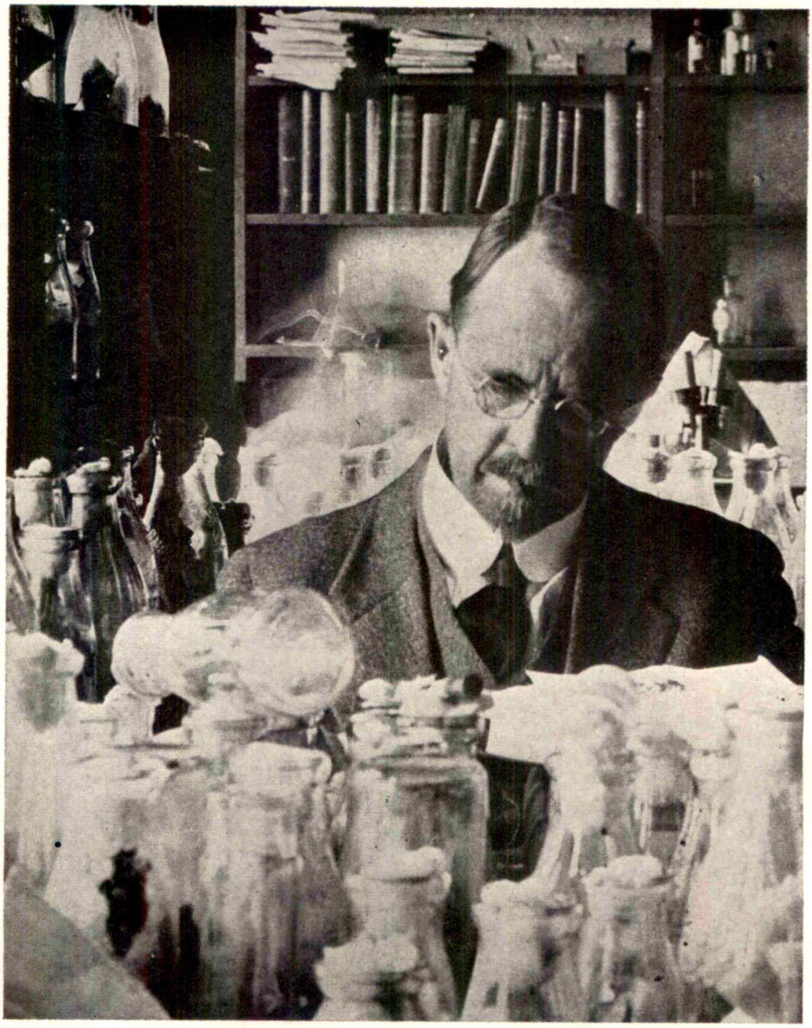


T. L. Maione demonstrating use of new measuring set developed at Bell Laboratories. He is about to adjust the in-service loss of transmitting group No. 1. The message displayed in the digital readout above the meter indicates that the loss of this group is 0.4 db higher than prescribed value. The testing is programmed so that buttons and lights operate only if the correct procedure is followed. The set permits very narrow bandwidth measurements (.003% at 3 mcps, for example) of pilots as low as −63 dbm (0.5 nanowatt) with 0.1 db (1%) accuracy.



T. H. Morgan

Taken by the author in 1917. At that time Morgan was camera-shy, so Bridges and I hid my camera in Bridges' incubator, with a string attached, and got this shot without Morgan's knowledge. The books and binocular microscope in the background are at my desk in the fly room.

AMERICAN SCIENTIST, 53, 1965

AMERICAN SCIENTIST

AUTUMN · SEPTEMBER 1965

THE "FLY ROOM"*

By A. H. STURTEVANT

When Daniel Coit Gilman became the first president of Johns Hopkins University in 1875, he assembled a remarkable group of scholars to supervise the graduate work there. Among these were two biologists: W. K. Brooks, who had studied with L. Agassiz, and H. Newell Martin, a student of Michael Foster and T. H. Huxley. These two trained a whole generation of outstanding zoologists, two of them being of especial importance in the history of genetics—Edmund Beecher Wilson and Thomas Hunt Morgan.

E. B. Wilson (1856–1939) took his Ph.D. at Johns Hopkins in 1881, with a thesis on the embryology of the colonial coelenterate Renilla. He then went to Europe, where he worked at Cambridge, at Leipzig under Leuckart, and at Naples. The Naples station, where he returned several times later, greatly influenced him, and led to lasting friendships with such men as Dohrn, Herbst, Driesch, and especially Boveri. In 1885 he became the first professor of biology at the newly opened college at Bryn Mawr. In 1891 he became professor of zoology at Columbia University, and remained there for the rest of his life. He also spent many summers working at the Marine Biological Laboratory at Woods Hole. Wilson's early work was largely in embryology, at first descriptive and later of an experimental nature; his interest here was chiefly in the analysis of the gradual limitation of the potentialities of the cells of the developing embryo, and the extent to which "formative stuffs" were involved in development. The first edition of his great *The Cell in Development and Inheritance* appeared in 1896, the second in 1900, and the third (really a new, much larger, and wholly rewritten book) in 1925. This was the standard work for many years, and exerted a very great influence on biology. Wilson's own studies on chromosomes began

* Chapter 7 from Dr. Sturtevant's forthcoming book, *A History of Genetics* to be published by Harper & Row in the autumn.

about 1905, with work on the sex chromosomes, and led t(detailed accounts that are models of accuracy and clarity oi

The career of T. H. Morgan (1866–1945) resembled that (many respects. He took his Ph.D. at Johns Hopkins in 18£ went to Europe, where he was also much influenced by a sta; and made lasting friendships, especially with Dohrn and 1891 he succeeded Wilson as professor at Bryn Mawr, and joined him at Columbia. Like Wilson, he wrote a thesis on (and continued in this field, first with descriptive work, and la experimental approach. He also studied the chromosomes ir with sex determination. At Woods Hole he and Wilson wer and they and their families were very close friends, both at and in New York.

For all that, the two men were very different. As R. G. H: was a close friend of both) has expressed it: "... Wilhelm his interesting book on great men of science, classified ther to their talents, as romantics and classics ... To the ror come thick and fast; they must find quick expression. His fi get a problem off his hands to make room for the next. T more concerned with the perfection of his product, with sett in the proper relation to each other and to the main bod: His impulse is to work over his subject so exhaustively a that no contemporary is able to improve upon it ... It is t that revolutionizes, while the classic builds from the ground

"Wilson is a striking example of the classic, and it is intere that for many years his nearest colleague and closest fri equally distinguished romantic."

In 1909, for the only time during his 24 years at Colum gave the opening lectures in the undergraduate course i zoology. It so happened that C. B. Bridges and I were both While genetics was not mentioned, we were both attracted and were fortunate enough, though both still undergrad given desks in his laboratory in the following year (1910 possibilities of the genetic study of Drosophila were then ju to be apparent; we were at the right place at the right time. tory where we three raised Drosophila for the next 17 y miliarly known as The "Fly Room," a rather small room (with eight desks crowded in it. There were always others w besides the three of us—a steady stream of American and dents, doctoral, and postdoctoral. One of the most important H. J. Muller, who graduated from Columbia in 1910. He spe of 1911–1912 as a graduate student of physiology at Cor School, and then came back to take a very active part in th work.

There was an atmosphere of excitement in the laboratory, and a great deal of discussion and argument about each new result as the work rapidly developed.

In 1909, Castle published diagrams to show the interrelations of genes affecting the color of rabbits. It seems possible now that these diagrams were intended to represent developmental interactions, but they were taken (at Columbia) as an attempt to show the spatial relations in the nucleus. In the latter part of 1911, in conversation with Morgan about this attempt—which we agreed had nothing in its favor—I suddenly realized that the variations in strength of linkage, already attributed by Morgan to differences in the spatial separation of the genes, offered the possibility of determining sequences in the linear dimension of a chromosome. I went home, and spent most of the night (to the neglect of my undergraduate homework) in producing the first chromosome map, including the sex-linked genes *y*, *w*, *v*, *m*, and *r*, in the order, and approximately the relative spacing, as they still appear on the standard maps (Sturtevant 1913).

The finding of autosomal linkage in Drosophila has been described by Morgan and Bridges (1919) and by Bridges and Morgan (1923), in their accounts of the mutant genes of the second and third chromosomes. The first test of two autosomal genes was by Sturtevant (February 1912), and showed that black and pink were independent. It was concluded that they were probably in different chromosomes, though this was only a tentative conclusion, since it was known that linkage could approach independent segregation in the frequency of recombination. In March 1912, Bridges found that the newly discovered mutant curved, when crossed to black, gave no double mutant types in F_2, so it was clear that autosomal linkage could occur. It was evident that by this time there were more autosomal mutants than there were chromosomes, so Bridges and I began a systematic search by testing the available types against each other. These tests quickly yielded results, but, about a week before they did, the second case was discovered by C. J. Lynch, who had made a cross of black to vestigial for another purpose, and noted the absence of black vestigial in F_2 (Morgan and Lynch 1912). This was the first published case of autosomal linkage in Drosophila, and was soon followed by the discovery (Morgan 1912) that there is no crossing over in the male for these genes. This relation was soon shown to be general for both the second and the third chromosomes. By the middle of July 1912, the tests carried out by Bridges and Sturtevant had shown that this linkage group (the "second") included not only black, curved, and vestigial, but 5 additional mutant types. In the same month, we also found two additional types linked to pink, thus beginning the study of the third linkage group (Sturtevant 1913). The fourth and last linkage group was found by Muller in 1914.

Stevens (1908) described the chromosomes of the female of *Drosophila melanogaster* (under the name *D. ampelophila*) as they are now known, but she found the male difficult to study, and we interpreted her figures as meaning that there was a rather small X attached to an autosome, and no Y. This interpretation was followed in early genetic literature on the species, until the work of Bridges (at first on XXY females in 1914), and then of Metz (at first on other species of the genus, also in 1914) established the relations now known. Bridges insisted from the first and rightly, that the Y is J-shaped, and longer than the rod-shaped X; but the rest of the group was at first unwilling to accept this, since in other animals (even in other species of Drosophila) the Y was known to be absent, smaller than the X or equal to it, but never larger. A corollary of the Stevens interpretation was that there were four, rather than three, pairs of autosomes, with one of them having some sort of relation to the X.

Bridges' cytological work grew out of his studies of non-disjunction. In the first paper on the sex-linkage of white eyes, Morgan reported a few white sons from the original mutant male, which he supposed represented further mutation; there can be no doubt that they were in fact due to non-disjunction.

Further examples kept appearing, and in 1913 Bridges published an extensive genetic analysis of the phenomenon, giving it the name non-disjunction. Further studies led to no satisfactory causal interpretation —until he looked at the chromosomes and saw that females that gave exceptional offspring were XXY in composition (1914). As Bridges understood, this was really a proof of the chromosome theory, and made it inconceivable that the relation between genes and chromosomes was merely some kind of accidental parallelism—especially after the publication of his detailed account in 1916, as the first paper in Volume 1 of the newly founded journal *Genetics*.

A further consequence of the cytological work of Bridges and of Metz was that it became clear that *D. melanogaster* had three pairs of autosomes—two large and one small—corresponding to the three autosomal linkage groups, of which two were also large, and one was small.

By 1915, the work with Drosophila had progressed to the point where the group at Columbia was ready to try to interpret the whole field of Mendelism in terms of the chromosome theory. The resulting book, *The Mechanism of Mendelian Heredity* (Morgan, Sturtevant, Muller, and Bridges, 1915), is a milestone in the history of the subject.

There had been much reluctance among geneticists to accepting the chromosome interpretation. Johannsen, for example, in the 1913 edition of his book, referred to it as "a piece of morphological dialectic"; and Bateson, in a review of *Mechanism* (1916), wrote ". . . it is inconceivable that particles of chromatin or of any other substance, however complex,

can possess those powers which must be assigned to our factors [i.e., genes] . . . The supposition that particles of chromatin, indistinguishable from each other and indeed almost homogeneous under any known test, can by their material nature confer all the properties of life surpasses the range of even the most convinced materialism."

It should be added that by his third edition (1926) Johannsen accepted the chromosome interpretation, and that Bateson closed the review from which the above quotation is taken: ". . . not even the most skeptical of readers can go through the Drosophila work unmoved by a sense of admiration for the zeal and penetration with which it has been conducted, and for the great extension of genetic knowledge to which it has led—greater far than has been made in any one line of work since Mendel's own experiments."

Not all critics were as generous, nor did they always get soft answers. In short, there were a good many polemical papers; and there surely would have been more if the work had not had the wholehearted support of Wilson, who had the respect and admiration of all zoologists, making him an invaluable ally.

With the publication of the *Mechanism,* and of Bridges' 1916 paper, this part of the story closes. There was still much exciting and fundamental work to be done with Drosophila, and the Columbia laboratory was still the center for such work, but it was now a question of how the chromosome mechanism works, not of whether it can be demonstrated to be the true mechanism.

There was a give-and-take atmosphere in the fly room. As each new result or new idea came along, it was discussed freely by the group. The published accounts do not always indicate the sources of ideas. It was often not only impossible to say, but felt to be unimportant, who first had an idea.* A few examples come to mind. The original chromosome map made use of a value represented by the number of recombinations divided by the number of parental types as a measure of distance; it was Muller who suggested the simpler and more convenient percentage that the recombinants formed of the whole population. The idea that "crossover reducers" might be due to inversions of sections was first suggested by Morgan, and this does not appear in my published accounts of the hypothesis. I first suggested to Muller that lethals might be used to give an objective measure of the frequency of mutation. These are isolated examples, but they represent what was going on all the time. I think we came out somewhere near even in this give-and-take, and it certainly accelerated the work.

* There are, in the later literature, some examples of a concern about priority in the development of ideas in the early period, but, at the time, such a concern never inhibited free and open discussion.

AMERICAN SCIENTIST, 53, 1965

LIQUEFIED NATURAL GAS—A NEW SOURCE OF ENERGY: PART II, PEAK LOAD SHAVING AND OTHER USES

By C. M. SLIEPCEVICH

IN THE previous issue* the development by Constock International Methane Ltd. of the liquefaction of natural gas and transportation by ship was traced. Although the principal use for liquefied natural gas (LNG) is in domestic heating and industrial use, there are several other outlets.

Peak Load Shaving

At least one promising use for LNG which does not involve the liquid transportation problem is known as peak-load shaving, or simply, peak shaving. This concept can be most easily described by referring to Figure 1, in which the variation in daily demand for gas by a local utility throughout the year is shown. It is obvious that the sinusoidal curve represents a highly idealized smoothing of the actual curve which has numerous peaks and crevices. The demand peaks on the coldest day in winter and bottoms on the hottest day in summer. The yearly average daily demand is represented by the horizontal broken line such that the shaded areas labeled deficit and surplus are equal. In order to obtain the best price and to be assured of a guaranteed supply, the local utility must contract to purchase from the gas pipeline company at a fixed rate the year round. If the purchases are made on the yearly average daily rate, customer demand in the winter months cannot be met. On the other hand, there will be a surplus of gas available in the summer months.[1] The obvious solution is to "save" the *surplus* in the summer time and draw on it to make up the deficit in the winter time. However, because huge volumes of gas would have to be stored, no man-made storage is practical.

In some regions of the country there are depleted gas and/or oil fields or aquifer[2] where large volumes of gas can be stored underground. In this case the solution is simple; the utility company stores surplus gas in

* AMERICAN SCIENTIST, June 1965, page 260. "Liquefied Natural Gas—A New Source of Energy: Part I, Ship Transportation."

A Sigma Xi—RESA National Lecture, April 1962.

[1] The treatment here is considerably simplified. Gas distribution and pricing is a complex matter [1].

[2] Aquifer here refers to a geologic anticline or trap having a stratum of permeable water-bearing sandstone or limestone which can be developed for storage of gas. In some cases, salt caverns or abandoned coal mines can be used for storing gas.

the natural, underground reservoir and draws on it during cold weather when consumer demands exceed pipeline capacity. The amount of such storage in the United States is increasing rapidly; in 1962, the capacity was 3.56 trillion cubic feet (about $^1/_2$ is usable) as compared to a total marketed gas production of 13.3 trillion cubic feet [2]. The big storage concentration, fortunately, is in the more populated areas in such states as Pennsylvania, Michigan, Ohio, West Virginia, California, and Illinois. Nevertheless, there are many areas, particularly New England, the East and South, where other measures have to be taken. For example, the surplus gas could be sold to local industry at a bargain (interruptible)

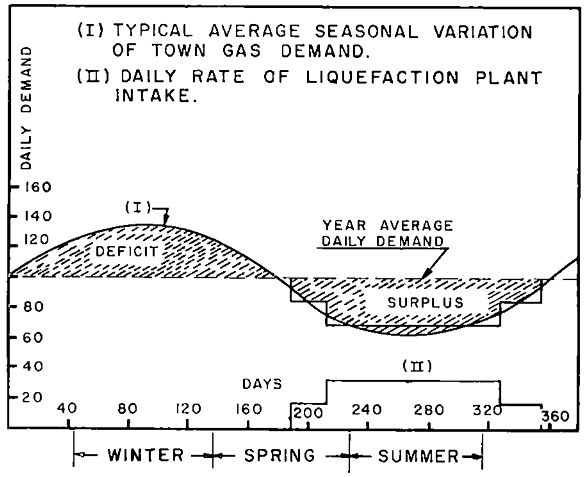


FIG. 1. Variation in daily gas demand with seasons of the year.

rate, in which case synthetic gas would have to be used to supply the deficit. The synthetic gas is frequently manufactured by thermal cracking of residual oils, catalytic reforming of propane and butane, or blending propane and air with the pipeline gas.

As an alternative to manufactured gas for peak loads, but not competitive with natural, underground gas storage, natural gas could be liquefied and stored during the summer months, and then vaporized during the winter months to augment the pipeline supply for meeting peak demands. The Cleveland plant was built for this purpose in the early 1940's. Within the last ten years, technological improvements in liquefaction equipment and cryogenic storage have tipped the economic balance in favor of LNG over manufactured gas for peak-shaving. Realizing this potential, Constock International Methane Ltd. and J. F. Pritchard Co. formed a new company, Constock-Pritchard, Inc., early in 1958, to develop and construct LNG peak-shaving plants.

As a further boon to LNG peak shaving, the concept of in-ground storage for LNG was developed in the early 1960's. Working independently, Conch [3, 4, 5], the American Gas Association [6, 7, 8], and Phillips Petroleum Co. [9] each developed their own version. A schematic of the in-ground storage, piloted by Conch at Lake Charles in 1961, is shown in Figure 2. The construction of the cavity-in-ground (CIG) is as follows:

(1) Freeze pipes for circulating refrigerant are sunk to the required depth along the periphery of a circle whose diameter is 20 to 40% greater than the diameter of the intended cavity.
(2) The ground is prefrozen by circulating an external refrigerant, such as propane, to form a frozen ring.

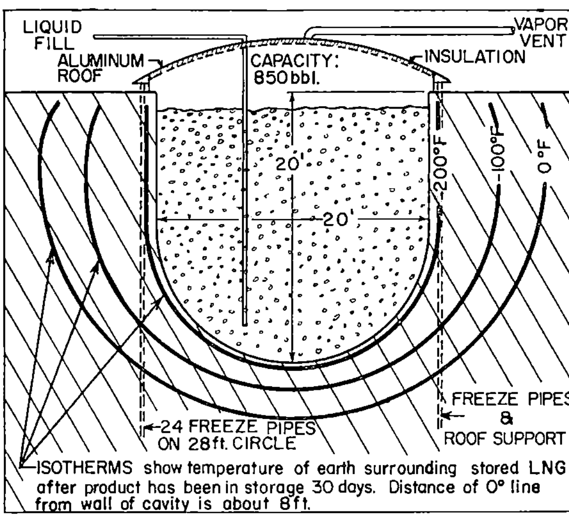


FIG. 2. How liquid methane is stored in frozen ground by Conch.

(3) When the inner diameter of the frozen ring reaches the diameter of the cavity, the unfrozen ground, bounded by the frozen ring is excavated.
(4) On completion of excavation, an insulated roof is installed.
(5) The cavity is now ready for filling with LNG.

The prefreezing operation eliminates the need for costly shoring during excavation since the frozen ground has structural strength comparable to concrete. It also prevents moisture penetration during excavation. The frozen ground acts as an impermeable barrier to any penetration or loss of LNG into the surrounding ground and also provides adequate insulation to prevent excessive boil-off during storage.

The temperature distribution measured during the Lake Charles tests

is also shown on Figure 2. The problem of theoretically predicting these isotherms and the rate of heat leakage into the cavity is not easy [8, 10, 11, 12]. It is necessary to know the heat leak quite accurately in order to establish the size of the refrigeration equipment needed.

Equally important is a knowledge of the properties of frozen ground, concrete, and other construction materials at these low temperatures in order to be able to predict the structural integrity of the cavity with time [13, 14, 15, 16]. Review of the design problems involved in cavity-in-ground storage is given by Sharp [17] and by Khan, *et al.* [18].

A number of articles on the economics of peak shaving with LNG have been published [18, 19, 20, 21]. The basic differences between LNG transportation and LNG peak-shaving plants are:

(1) Peak-shaving plants tend to be of much smaller liquefaction capacity, but their vaporization capacity is larger since they must be able to deliver gas at a very high rate for the few (5–15) coldest days in the year.

(2) The liquefaction section of peak-shaving plants lies idle $^1/_3$ to $^1/_2$ of the year, whereas, in LNG transportation, it operates around the clock.

(3) Peak-shaving plants require relatively more storage than LNG transportation plants. The cost of storage in the former runs over 25% of the total investment, whereas, in the latter, it is usually below 10%.

(4) Even though the peak-shaving plants require no investment in tanker, loading and unloading docks, reforming facilities, etc., the total investment per unit of liquefaction capacity tends to run 10–20% higher than LNG transportation plants. Oddly, both of them run pretty close to $1 of total investment per cubic foot of gas liquefied.

(5) The unit selling price of gas delivered to the pipeline will run about 30–50% higher in a peak-shaving plant than an LNG transportation plant, depending on whether reforming costs are included in the latter. Round figures for the LNG transportation plant are $1.10 per 1000 cu ft as compared to $1.50 per 1000 cu ft for peak shaving.

It is to be understood that the above figures are used for illustrative purposes only, in an attempt to typify and compare a peak-shaving plant having a liquefaction capacity of around 10 million cu ft of gas per day with a transportation plant of 100 million cu ft of gas per day. Depending on the location of the site, these figures can vary drastically.

Thus far, four LNG peak shaving plants in this country are scheduled to begin operations in 1965. The first one will go on stream early in 1965. It is being built by Constock-Pritchard Inc. for Transcontinental Gas Pipeline Corp. (Transco) on 420 acres in the Jersey Meadows near South Hackensack, N.J., at a reported cost of $12 million. It will have a liquefaction capacity of 5 million cu ft per day into 1 billion cu ft of equivalent gas storage with a maximum vaporization capacity of 200

million cu ft of gas. Transco will initially sell the gas to eight utility companies from Georgia to New Jersey for winter peak shaving at a cost (reported to be $2.20 per 1000 cu ft) about $^1/_2$ to $^3/_4$ the cost a utility would pay to manufacture its own peak-shaving gas. Because of the marshy nature of the ground, which rendered above-ground storage impractical, Transco will use in-ground storage consisting of a cavity 115 ft in diameter and 165 ft deep [22], with a capacity of 12 million gal of liquid or a billion cubic feet of gas.

The second LNG peak-shaving facility is under construction at a cost of $3 million near Birmingham, Ala.; it will be jointly owned by Air Products and Chemicals Co. and Alabama Gas Co. This plant is slightly smaller than Transco's. It has a daily liquefaction capacity of about 4 million cu ft of gas and a vaporization capacity of 85 million cu ft. The storage consists of a conventional, above-ground, double-walled tank having an inner liner of 9% nickel steel and insulated with 5 ft of perlite. The storage capacity is 7.2 million gal or 600 million cu ft of gas [23]. In proximity to this plant, Air Products is involved in a cryogenic complex which includes a gas reforming plant to produce hydrogen from natural gas. The hydrogen is used for manufacturing ammonia and as feed to a 30 ton per day hydrogen liquefaction plant which will supply NASA's Pearl River test site near New Orleans. In addition, some of the LNG from the peak-shaving plant might find outlet as a rocket fuel. A multiplicity of cryogenic products from a single complex offers intriguing economic returns and could set the stage for a healthy growth in cryogenic processing, which, to date, has been monopolized by the production of liquid nitrogen and oxygen.

A third LNG peak-shaving plant should be in operation late in 1965 at Chula Vista, Calif. It is being built by American Messer Corp. for San Diego Gas and Electric Co. The liquefaction plant will use pipeline pressure drop to liquefy natural gas in an expander cycle at a capacity of 2 million cu ft of gas per day. The storage tank is identical to the one at Birmingham except that it uses 3 ft of perlite insulation instead of 5 ft. The vaporization capacity is 60 million cu ft of gas per day. The cost is $2.7 million [24].

A fourth peak-shaving plant is being built for Wisconsin Natural Gas Co. at Oak Creek, Wis., between Racine and Milwaukee. The plant will have an above-ground metal, double-wall tank providing the equivalent of 250 million cu ft of gas storage, a vaporization capacity of 50 million cu ft of gas per day and a net liquefaction rate of 750,000 cu ft of gas per day. Chicago Bridge and Iron Co. are the designers and builders of the plant, scheduled to go on-stream in the Fall of 1965 [25].

Other Uses for LNG

As LNG peak-shaving plants become more widespread across the

United States, a number of other uses for LNG will develop because of its increased general availability at reduced costs. Two promising possibilities are as a rocket propellant and as a fuel for supersonic aircraft.

Prior to 1959, LNG was considered a possible candidate as a rocket fuel. However, it seemed to drop out of the picture when NASA took over the space program and proceeded to concentrate on such fuels as RP-1,[3] hydrazine, and liquid hydrogen. Within the last two years, NASA has had to modify its thinking somewhat. Although liquid hydrogen, in combination with liquid oxygen, provided the high performance characteristics needed, there were some applications, such as space storability, recoverable vehicles, etc., for which the hydrogen–oxygen system—as well as other fuels—was unsuited. In this respect, LNG offers the most attractive alternative. When LNG is used in combination with the oxidizer, FLOX (a liquid mixture of fluorine and oxygen), its performance is commensurate to the hydrogen–oxygen system.[4] Compared to RP-1, hydrazine, and hydrogen it is the safest material to handle [26] and its cost is substantially lower than hydrazine and hydrogen, being only slightly higher than RP-1. In addition, its properties are such that it can be adapted to existing rocket hardware, or it can lead to simplification in new hardware.

The enormous heat dissipation problem presented by supersonic jets will require the development of more thermally stable fuels which can serve as a heat sink for cooling the engine and the leading edges of the air frame, in addition to maintaining the required temperature in the cabin for both the payload and electronic instrumentation. Some of the thermally stable fuels which have been considered cost 20- to 50-times as much as jet fuel. Whether or not supersonic jets will ever become a reality hinges on fuel economy. LNG, with its superior performance and cooling capabilities, added to the fact that its cost is comparable to jet fuel, offers the most promising solution.

Both of these applications are being investigated by the Research Division of Continental Oil Co. For their studies, and to supply test quantities for others, they have erected a liquefaction and purification unit at Ponca City which can produce up to 600 gal per day of liquid methane having a purity of 99.9+%.

The fact that LNG is currently not generally available should not serve as a deterrent to its ultimate use as a rocket or supersonic jet fuel.

[3] Similar to jet fuel.

[4] Another interesting comparison of performance is afforded by the 3-stage, Saturn V rocket. If the present RP-1/LOX booster stage were replaced by LNG/FLOX, the payload for solar system escape could be increased from 3500 to 10,300 lb. This performance even exceeds the payload of 7000 lb which could be put into solar system escape by replacing the third stage, LH_2/LOX, in the present Saturn V, with a nuclear engine.

Even if LNG were not being developed for heating purposes, it would merit development for rocket and aircraft fuels. Although the total consumption of LNG by the rocket industry would represent an insignificant fraction of the total natural gas currently produced, it can be readily liquefied at any site near a pipeline or imported as LNG at a cost substantially lower than the other rocket fuels, with the exception of RP-1 (and ammonia if it enters the picture). On the other hand, supersonic jets, with their voracious appetites for fuel, could eventually consume up to 10% of the total natural gas production in the United States. In this case, the demand is sufficient to realize the savings in manufacturing costs in large-scale plants; consequently, the LNG could be produced competitively solely for the purpose of replacing present-day fuels. On the other hand, the day is rapidly approaching when LNG will be available in practically every major population center in the world to satisfy either a peak-shaving or a base-load demand. Since supersonic jets will largely confine their operations to such centers, the general availability of LNG may well exceed the current, general availability of jet fuel. Even today, certain preferred jet fuels are difficult to obtain abroad.

Summary

The next decade will probably reveal an unprecedented growth of a single commodity, LNG, as a major factor in supplying the world's demands for energy. Although the pioneering efforts of Constock were primarily directed toward the foreign markets, the demand for LNG in the United States, both as peak shaving and base load, could even surpass many of the foreign markets. Both the East and West Coasts are feeling the pinch for more gas. Although the nation's 1500 gas transmission and distribution companies are laying pipelines at the rate of about 30,000 miles per year and a cost of about $1.7 billion per year to an already established piping network of over 700,000 miles [27], it is questionable whether they can keep pace with the growth in demand, notwithstanding predictions to the contrary [28]. The cost of constructing pipelines in this country varies from $10,000 to $280,000 per mile depending on the terrain and pipe size. An average cost for good pipelining country is around $100,000 for a 30-inch pipe, or roughly an average of from $2000 to $3000 per mile per inch diameter of pipe. These costs include right-of-way, surveying, communications for remote operation, maintenance, meter stations, miscellaneous materials, and compressor stations [29]. The cost of transporting natural gas by pipeline is about 1.7 cents per 1000 cu ft per 100 miles, which is about double the corresponding cost for transporting it by LNG tanker. When one adds to the LNG figure the costs of liquefaction and regasification, the pipeline and LNG begin to approach an even trade-off for the same distance between the

source and the market. In fact, Venezuelan gas for the East Coast and Alaskan gas for the West Coast, or possibly Mexican gas for either coast, could put the squeeze on pipeline gas, which is experiencing a steady rise in cost, year after year. If LNG does not dent the pipeline market, it certainly will establish a ceiling for the "city-gate" price. Furthermore, if gas prices at the producing fields continue to rise at present rates, coal shipped from the North by barge or tanker could displace gas for power generation in the Southwest.[5]

In the last analysis the total energy picture will depend greatly on the continued development of atomic energy and direct conversion devices such as the fuel cell, the thermionic converter, the thermoelectric generator, and the magnetohydrodynamic (MHD) converter. Of these, the fuel cell and the MHD generator will probably have the greatest effect on the increasing use of natural gas for generating power.

The use of LNG as a starting raw material for petrochemical processing represents a good, profitable usage even though the quantity, as compared to fuel consumption, is only a matter of a few per cent.

Lastly, political factors, such as import regulations and nationalization of privately-owned plants in foreign countries, could be the tail that wags the donkey.

Acknowledgment

The author appreciates the permission granted by Conch International Methane Ltd., Constock-Pritchard Inc., and Continental Oil Co. to use liquefied natural gas as the subject for the Sigma-Xi-RESA National Lectureship in April 1962.

[5] A tragic, but not inconceivable, turn of events would be a flow reversal in which coal slurries are pipelined from the Northeast to the Southwest.

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AMERICAN SCIENTIST, 53, 1965

THE EVOLUTION OF HUMAN INTELLIGENCE

Some Reasons Why It Should Be a Continuing Process

By SHELDON C. REED

THERE are now many experiments with man which give reality to the concept that all human behavior must have some kind of direct or indirect connection with the genetics of the reacting person. A stimulating review by Gottesman (1963) covers most of the recent developments in this area of research.

The clamor resulting from the nature-nurture misunderstanding seems to have frequently obscured the obvious relationship between the evolution of the cerebral hemispheres and the basic intelligence of each species. The chimpanzee is smarter than the cat, and the cat is brighter than the canary. The very readable article by Bitterman (1965) shows that the intelligence of animals on various rungs of the evolutionary ladder differs not only in degree but also in quality.

The correlation between the evolutionary increase in the size of the brain and the elaboration of intelligence is excellent. One can observe the astonishing expansion in size and efficiency of the forebrain from the modest organs of our insectivorous-like ancestors to the incredible mental equipment of *Homo sapiens*, man the wise.

Increases in the size of the whole body should not be mistaken for increases in intelligence. Those terrible lizards, the dinosaurs, provided a splendid example of a remarkable response of body size to the pressures of natural selection, accompanied by an almost complete failure of the brain to keep pace with the evolutionary luxuriance of the other organs of the body.

There will be no attempt to define intelligence in this paper. The word is used in the same general sense as people use it everywhere. We can present some useful concepts without any concern for the validity of particular tests of intelligence or the reliability of the findings for particular individuals. The only required profession of faith from the reader is that each person is the product of the interaction of his biology (genetics) and his environment.

How Evolution Proceeds

Credence in the inheritance of acquired characteristics has finally lost its last claim to importance with the collapse of Lysenkoism in the Soviet Union. All evidence indicates that evolution proceeds as the

result of the alteration of the genotype under the influence of natural selection.

Sewall Wright (1932) demonstrated that the most favorable population structure for rapid genetic change would be composed of isolated units within which local differentiation could take place. The more successful local units, concerning intelligence, would result in part from environmental good fortune but also from the gradual incorporation of new mutations of genes for higher intelligence. These advantageous new mutations, with other genes for higher intelligence, would spread to other communities. They would gradually replace less advantageous genes for intelligence as the result of natural selection.

It is usually assumed that the prehistoric community structure consisted of a dominant male with several wives and that the dominance of a male depended not only upon environmental fluctuations but also upon genes for physical and mental strength. One million years ago, such a breeding structure would have been nicely suited to the rapid accumulation of genes which would contribute to the development of the brain and the evolution of problem solving. Genetic selection through the male in a polygamous social structure should be rapid because more than half of the males in each generation would fail to reproduce; thus, their "unsuccessful" genes would die out when they died. Environmental vagaries would disguise the genotype of each male to some extent but, over the long haul, there should be selection for genes favoring higher intelligence. The mentally retarded male would have little chance of reproducing in a polygamous society.

It is difficult to estimate the approximate number of different genes which must be involved in the development of the brains of the more than three billion people now alive. It is unlikely that any two of the three billion people have precisely the same combination of genes for intelligence—except for the multiple births derived from a single fertilized egg! This is an astonishing observation but not an improbable one. Sewall Wright (1932) pointed out, as a general illustration, that if a species had 10 alleles at each of only 1000 gene loci there would be 10^{1000} possible different gene combinations. Some idea of the incomprehensible number this represents comes from the estimate that the total number of electrons and protons in the visible universe is less than 10^{100}. Perhaps it should be mentioned that most of the astronomical number of different genotypes produced would be indistinguishable from each other when measured by any known instrument.

A new mutation could spread throughout the small isolates of prehistoric time with some rapidity but, in today's gigantic population groups, it would take thousands of generations for a highly advantageous mutation to become established in the majority of the people in any one large geographic area. Man has become one of the dominant species in

terms of numbers, and all so recently. It took until the 1800's to reach a total of one billion persons. It took only 100 years to add the second billion; some 30 years to add our third billion; and it will take only 15 years more to reach the fourth billion. Probably, extra billions will be added rapidly even if the present rate of increase can be slowed down. It is hard to imagine what the world will be like a few hundred years from now, with its teeming billions. Obviously, the occasional new mutation for higher intelligence will have little chance of spreading throughout any appreciable fraction of the billions on billions of people then alive.

It is clear that, if worldwide changes occur in our genes for intelligence during any relatively short period of time, they will not be due to new mutations to any important degree. But there could be an increase in the proportion of genes for higher intelligence now available with continuing elimination of those for lower intelligence. This basic kind of selection takes place at the level of the individual person and family and can be expected to continue without regard to the total size of the population.

In the early days of man's evolution, favorable genes for higher intelligence could have spread rapidly because of the small size of the population units. Previous to the development of agriculture about 10,000 years ago, there could have been no urban life. Before the advent of agriculture, the world population must have comprised only a few million people. In fact, there were so few people over such a very long period of time that it is impossible to accept any average annual growth rate for those long periods of human existence. For instance, if we assume that there were 20 million people alive in 8000 B.C. and that the rate of increase was 0.02 per cent per year, then there would have been about 10 million people in 11,000 B.C. and only 5 million in 14,000 B.C. Unfortunately, this uniform rate gives us only 3 persons alive in 75,000 B.C., a nonsense answer. Thus, in order to extend the genus *Homo* back for even 500,000 years, a uniform annual rate of increase would have to be so small as to be almost meaningless. A rate of increase of considerably less than 0.02 per cent per year does emphasize the terrifying change we find of 2 per cent increase per year of the world population at present.

Presumably, the evolution of higher intelligence, as distinguished from the lower intelligence of the apes, occurred during the last one million years or so, which would involve about 30,000 generations, if the average generation length was 33 years, while it would be around 40,000 generations, if the average generation length was 25 years. The 10,000 years which have elapsed since the appearance of agriculture have seen the population grow from a few million to over three billion persons, all in the last 1 per cent of the time during which higher intelligence was evolving.

Evolutionary Rates

The question now arises as to whether higher intelligence evolved at a uniform rate over the last one million years, or did the process accelerate during the last 10,000 years, and, if so, is it still accelerating? If we regard intelligence as responsive to cultural development, we would have to assume that intelligence has been, and probably still is, approaching higher levels as civilization advances. The increasing mechanization and complexity of our culture seems to discriminate against reproduction of the mentally retarded and to encourage that of the mentally more fortunate. A discussion of this point is presented in our book on mental retardation, Reed and Reed (1965). What do we know about trends in evolution?

One of the clearest and most widespread trends in evolution is the gradual increase in body size; animals tend to get bigger than their ancestors. Several scholars, including Rensch (1959), have shown that such trends are best understood as the outcome of orthoselection, a long-continued process of natural selection favoring a certain quality or feature. Being large in size is advantageous in defense, offense, and sexual selection; hence, animals tend to evolve toward genetically larger types. However, it is conceivable that extremely large size would become a handicap under some conditions, and that evolution would then be reversed in regard to this one trait. Intelligence would seem to be an orthoselective trait of the first degree because it is unlikely that there is any environment where lower intelligence *really* would be advantageous to a person, and higher intelligence disadvantageous.

Let us now face the baffling problem of the rate of evolution of higher intelligence. We are trapped by the same kind of absurdity that was so apparent with the numerical growth of the human population. Any annual *average* rate of change would be so small that it would not reflect the random fluctuations which probably occurred. However, in present-day terms, let us assume that our ancestors of 35,000 generations ago had an average IQ of at least 30. There is no methodology which allows us to determine the intelligence of earliest man, but he must have had some intelligence and the exact value is irrelevant to our argument. If we assume that the average intelligence evolved from an IQ of 30, equated to the present average of 100, there has been an increase of about 70 IQ points in about 35,000 generations. This is 0.002 of an IQ point for the average rate of change *per generation*. Thus, while our imaginary rate of change of two thousandths of an IQ point per generation is not a practical figure, it does serve notice that, even during the greatest spurts of man's evolution, the largest change in intelligence for any one generation must have been modest indeed. Presumably, the worldwide change per generation during the greatest increases in intelligence would have been only some small fraction of an IQ point. It is hard to imagine any re-

versals of intelligence which would correspond to the drops in population size due to disastrous epidemics. Changes in intelligence in each generation should be much more conservative than fluctuations in population size.

Recent Research

It should be clear that no large changes in the average intelligence of a nation, or the world, are likely to occur in one generation. If striking fluctuations are reported, one should question them. They are likely to result from bias due to the use of different psychological tests, different sampling methods, or other pitfalls, rather than from fundamental environmental or genetic changes. One of the best surveys of the change in IQ for a population is the well known study of the Scottish Council for Research in Education (1949). Over 87,000 children were tested in 1932 and a different 70,000 were tested in 1947. This is approximately one half of a generation between the two test periods. The average IQ was about 2.2 points higher for the second group of children. It would be foolish to think that any appreciable part of this gain, if real, was genetic over such a short period of time. Nor would it be safe to assume that the gain was all due to an improvement in Scottish education during the years of the Second World War. It is quite possible that some of the gain resulted from bias due to sampling, testing, or other unknown factors. Further attempts to determine what factors were responsible for the 2.2 point increase in the IQ at the end of the 15-year period might yield results of the greatest value.

In our own work, Higgins, Reed and Reed (1962), the children averaged about 4.0 IQ points higher than their parents. This was due primarily to a technical bias and we know what it was. The mean age of the parents at testing was 14.24 years while the mean age of the children was 8.65 years. The younger the children when tested, the higher the average IQ. Thus, when the age of the children was equated to the age of the parents, at testing, all gain in the IQ vanished. We would like to re-emphasize that what in the "old days" might have been published as a 4.0 IQ point gain for an intelligence score in one generation is no real gain; it is clearly the result of a statistical bias which, when corrected, results in the disappearance of the "gain." The likelihood of including one or more biases of substantial importance in any studies of intelligence of different groups or generations is so large, and the likelihood of a substantial genetic or environmental effect upon intelligence in one generation is so small, that we should perhaps think of our data as being more erroneous than our speculations, if one may entertain such an unorthodox idea. Clearly, the worst error possible in this area of study is to be *certain* of your data.

The Eugenic Paradox

The nature-nurture controversy was not the only smoke screen which inhibited progress in the study of the evolution of intelligence. The well-intended efforts of the eugenics movement rested upon the misconception that the mentally retarded more than replace their numbers, while the most brilliant citizens fail to marry or have few children. If this were true, the average intelligence of the nation should be falling, and man would be losing the trait which makes him intellectually superior to all other species.

The basis for the eugenic concern was the well-established fact that the larger the family of children, the lower the average intelligence of those children. One investigator calculated that the intelligence of one area of England was dropping by about four IQ points per generation. This could not continue for long without ensuing disaster, if true. It occurred to me in 1949 that the eugenic premise might be based on a seriously faulty design of the experimental method. All members of each generation with no children had been omitted from the studies of the past, which might lead to a large enough bias to vitiate the possible significance of the negative correlation between the number of children in the family and their average intelligence. My most helpful wife, Dr. Elizabeth Reed (and later a graduate student, James V. Higgins), and I set out to explore the problem. We found that it made a striking difference when the childless members of each generation were included. See Higgins, Reed and Reed (1962).

The results of interest here concern 1016 families where an IQ value was available for both the father and the mother and at least one child; these amounted to a total of 4071 persons with known IQ values in this sub-sample. We are well aware that the value of a single intelligence quotient may be slight, and that testing errors are frequent. However, the sharp differences to be presented are certainly not the result of testing errors.

These data provided the expected negative correlation of -0.30 ± 0.02 between the number of children in the family and their intelligence, similar to the findings of previous investigators. We found that the low average intelligence of the children in large families was anticipated by low average intelligence of their parents, as measured when the parents were children themselves. Thus, the mothers of the 370 two-child families had an average IQ of 104.5 ± 0.7 while the mothers of the 5 families of nine children averaged only 90.0 ± 11.3. The important and sharp drop in intelligence of the parents occurred in those who produced six or more children. The IQ for the total population of fathers and mothers showed a substantial negative correlation with the size of the families they produced some years after they were tested. This last point is very important and it had not been demonstrated in a quantitative fashion previously.

The negative correlation of -0.30 ± 0.02 between the number of children in a family and their intelligence is arithmetically satisfactory and has been confirmed by numerous investigators. However, it is conceptually inadequate and leads directly to the eugenic paradox. The parents of this generation of children represent only a part of their generation of people. It ignores the fact that these parents had a sizeable number of brothers and sisters who remained childless. The failure to consider the intelligence of the members of a generation who failed to reproduce provides a large statistical bias, or error, which could easily vitiate the significance of the negative correlation which was just presented.

The necessary innovation of including the childless brothers and sisters of our parents results in a striking change in the picture. This is because there is an extremely important differential in intelligence between the unmarried in the population and the married. This must be so because the severely mentally retarded seldom marry. The severely mentally retarded thus pull down the average of intelligence for all unmarried persons. This should not be interpreted as a reflection upon the intelligence of single persons; it is merely stating the obvious fact that the severely retarded are handicapped in obtaining mates, especially when they are institutionalized. The sharpness of this differential can be indicated by pointing out that 42 per cent of our smaller group of unmarried persons were retarded while only 4 per cent of the larger group of married persons had IQ values of 70 or below.

TABLE 1

AVERAGE NUMBER OF OFFSPRING PER INDIVIDUAL IN RELATION TO IQ (BOTH MARRIED AND UNMARRIED SIBLINGS INCLUDED)

Data of Higgins, *et al.* (1962), and Bajema (1963) Combined

IQ Range	*Number of Siblings in Parental Generation*	*Average No. of Offspring*
70 and below	106	2.09
71–85	283	2.30
86–100	1010	2.22
101–115	1122	2.20
116–130	376	2.49
131 and above	48	2.98
Total Combined Sample	2945	2.26

Bajema (1963) published a study similar to ours in its results, though for a different population. He studied 979 individuals born in 1916 and 1917 and tested when in the sixth grade of the Kalamazoo Public School System. The 979 persons had 2189 offspring who lived past the age of one year, an average of 2.24 offspring per individual. Our comparable average for both married and unmarried persons was 2.27 offspring. He analyzed

the reproduction of the 979 persons in terms of intelligence and included our data next to his in his Table 5. The two populations were so uniformly comparable in their reproductive capacities that it seems reasonable to me to combine them and present them here as Table 1. It can be seen that the parents with above-average intelligence had slightly more children than the below-average parents.

The inclusion of the adults who failed to reproduce altered the situation dramatically. It is still possible that man's intelligence is suffering a decline due to unknown factors. However, the eugenicist's old paradox resulting from the observed negative correlation between family size and intelligence seems to have been resolved.

One further comment may help to explain why the eugenic worries about the effect on national intelligence of a possible differential birth rate were so firmly entrenched. There was always the tendency to equate intelligence with the amount of education obtained or even with social class. To be sure, there is a significant correlation between these items but the correlation is far from perfect. There is a real negative correlation between the number of children in the family and the amount of education or the socio-economic status. However, there will be only a negligible proportion of persons in the Ph.D. group, for instance, with low genetic endowments for intelligence. There will be a much larger proportion of the poorly educated who will exhibit high intelligence on the standard tests. Each of these groups can be expected to reproduce in accord with its social standing. If the persons were rearranged according to their intelligence instead of according to formal education, the higher birth rate of the mass of the poorly educated would be only slightly lowered by the addition of the few Ph.D.'s with low birth rates and low intelligence but extensive education. However, the average number of children produced by the highly educated group would be raised substantially when the persons with high intelligence and many children, but with little education, are added to the highly educated group. Thus, the small negative correlations between the amount of education and number of children produced which were found by Dice, Clark, and Gilbert (1964) would almost certainly disappear if the correlation of family size is made with tested intelligence rather than with education.

Now that we have exorcised the devil of the detrimental differential birth rate we can accept more comfortably the concept of orthoselection for higher intelligence as a continuing present day process.

Guiding Our Own Evolution

Are there any ways in which the process could be accelerated? Could we cause a more rapid increase in the proportion of genes for higher intelligence with the corresponding decrease in the proportion of genes for lower intelligence?

It is not necessary to accept any specific theory for the basis of intelligence to conclude that it can be improved. Those oriented toward the environmental philosophies expect improved education, the "war on poverty," and other social actions to bring this about. There may be considerable disillusionment in this regard as little change is likely to occur in one generation for the whole nation. The important question, from the practical point of view, is how to manipulate society so that the genetic gains and the environmental improvements will both be optimal. The aspirations of mankind are committed to this goal.

It is clear that the level of intelligence is transmitted from parents to offspring, and that no distinction is mandatory as to what is transmitted. All the data indicate that parents provide their children with both genetic and environmental endowments.

It follows that if the less intelligent have no offspring, neither their genes nor their below-average environments will be reproduced in the next generation. On the other hand, those with the most intelligence might be persuaded to have more children than they do. They would then transmit both their genes and their above-average environments to a greater proportion of the next generation. Our social institutions should be able to do a much better job than they are doing in approaching these goals. The major deficiency has not been that of too little money but of too little understanding of the ideas under consideration here. The first step needed is the provision of more appropriate education for our policy makers and the general public.

It is not likely that the evolution of intelligence will proceed very rapidly in the world as a whole in a single generation. However, the level of intelligence of the world could improve significantly within a few generations if the world populations understand how genetic selection works or could be made to work. A considerable reduction in the frequency of mental retardation could result in a very few generations if appropriate steps were taken. The reason for expecting some success lies in the high transmissibility of mental retardation. Reed and Reed (1965) showed that more than one third of all the retarded persons in our study had one or both parents who were also retarded. It follows that, if these retarded parents had not reproduced, there would have been one third less retarded children added to that population. There is increased emphasis upon the prevention of mental retardation and if this becomes a worldwide trend there should be a gradual improvement in intelligence.

It would seem that we are directing our evolution toward higher intelligence though the change in each generation is probably very slight. It is also gratifying to realize that, if decreases in genetic intelligence were to occur, they would also be small in terms of single generations.

Summary

The development of civilization has taken but a moment of the long period of time during which man's brain and his intelligence have evolved. Our cultural legacy is the product of man's intelligence. Today, an important question concerns the effects of civilization upon the direction of changes in the genetic components of intelligence at present and in the immediate future.

When we correlate parents' income, education, social status, or IQ with the number of children they produce, correlation coefficients are obtained which are negative in sign. These well-established negative correlations are statistically biased and seriously misleading because they ignore the important segment of the parental generation which had no children. The negative correlation (-0.30 ± 0.02) between family size and intelligence of the children in it has no meaning for the next generation because the mentally retarded children will have little income, education, or social status; they will seldom marry or have offspring. The mentally retarded who do reproduce may have large families but the *average* number of children produced by the retarded is lower than that for normal persons. It is impossible to determine what is happening to the intelligence of mankind as a whole but it is clear that it is not decreasing at an appreciable rate.

The main conclusion is the perhaps euphoric concept that genetic orthoselection for higher intelligence is continuing at the present time. The challenge of our complex world, with an ever greater premium placed on higher intelligence, should result in an acceleration of the rate of evolution toward that end.

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AMERICAN SCIENTIST, 53, 1965

COLOR NAMES FOR COLOR SPACE*

By ALPHONSE CHAPANIS

THE richness, variety, and importance of normal color experience are truly remarkable. Our ability to see colors contributes immeasurably to our ideas of beauty and to the aesthetic appreciation of objects in our everyday world. Color is used routinely in business, industry, science, and medicine to code and identify objects and to communicate information. And, finally, color seems to be associated with a variety of affective responses, feelings, emotions, and moods—such as liking, disliking, excitement, and depression.

For these reasons, it is not surprising to find that there has been a great deal of scientific work done on color vision and color perception. Richter's two bibliographies (1952 and 1963), for example, contain well over 6000 entries even though they cover only the fifteen-year period from 1940 through 1954. Despite all this work, there are many fundamental things we still do not know about this subject. As one illustration, there is still much uncertainty about precisely how various wave lengths of radiant energy are transformed into nervous energy and differentially coded for transmission to the higher nerve centers of the brain.

Color can be studied anywhere along a broad spectrum of problems. At one extreme, some scientists are deeply interested in the microfunctioning of the retinal elements and in the photochemicals that transform electromagnetic energy into nervous energy. At the other extreme, one can find research workers concerned with responses to color as an aid to understanding the structure and dynamics of personality. The problem I wish to discuss falls somewhere near the middle of this spectrum, for I am primarily concerned with the common color names that we can all use to talk about our color sensations.

Strangely enough, in all the vast literature on color and color perception, the topic of color names is one that psychologists have largely ignored. To be sure, studies involving color naming have had a long and respectable history in psychology. In fact, one of the very earliest of these was begun by James McKeen Cattell while he was a fellow at The Johns Hopkins University in 1882–1883 (Cattell, 1885). But Cattell and the many psychologists who used color-naming tests after him were interested in these tests almost exclusively as dependent variables. Typically, investigators have used the time taken to name a set of simple colors, or the number of errors made in naming some simple colors, as a measure of other things—mental ability, the effect of drugs, the effect

* A Sigma Xi-RESA National Lecture, 1964–1965.

of fatigue, and so on (see, for example, Berdie, 1940; Cattell, 1890; Cattell and Farrand, 1896; Remoli, 1933). Rarely was anyone interested in what people meant by the color names they were using. The occasional studies which tried to establish some denotative meaning for color names confined themselves to the spectrum colors (see, for example, Beare, 1963; Dimmick and Hubbard, 1939; and Pickford, 1951), to a very few surface colors (Katzin and Murray, 1934), or to no more than five or six simple color names (see, for example, Halsey, 1959).

To focus more precisely on the present topic, let me pose the question that got me started on this problem in the first place: What is the maximum number of usable color names for all of color space?

Why Study Color Names?

Let's begin by asking, "Why should anyone study color names?" Although there are several answers to this question, one of the simplest is to say that colors and color names are important for many industrial and engineering purposes. You have heard, I know, that we are experiencing a kind of second industrial revolution—a revolution brought about by new kinds of machines and machine systems. One thing that characterizes this new class of machines is that they deal not so much with tangible products, like steel ingots, glass bottles, and washing machines, as with information—meaningful information that can be understood, handled, and transferred from man to machine and vice versa.

Let me give you an example. An air traffic control center in a large metropolitan airport doesn't produce a single concrete item that one can point to, handle, or put into a crate. Its principal product is information—information about incoming flights, outgoing flights, weather conditions, and emergencies. For any one aircraft the controller needs to know a lot of different things: the identity of the aircraft, its altitude, speed, distance, direction, origin, and destination. Some of this information changes continually; other pieces of the information are relatively static. Since controllers in a large metropolitan airport may have to keep tabs on a hundred or more aircraft at any one time, you can readily see that such a system must be capable of handling a very considerable amount of information.

Another familiar example of modern systems that deal primarily with information are the giant computers that are becoming almost as common as desk calculators and slide rules were a few decades ago.

An essential part of many of these information-handling systems is that they must portray, or display, the information they are processing so that their human counterparts can see it and interpret it quickly and correctly. When large amounts of information have to be displayed for human users, color turns out to have some extremely useful properties (see, for example, Harris, Kolesnik and Teel, 1964; Morgan, Cook,

Chapanis, and Lund, 1963; Smith, 1962; and Smith and Thomas, 1964). Because colors can be so effective for coding information, the engineer or systems designer sometimes wants to know what is the maximum number of different colors that can be used for this purpose. This is quite a complex question because, to be useful for coding information, a set of colors

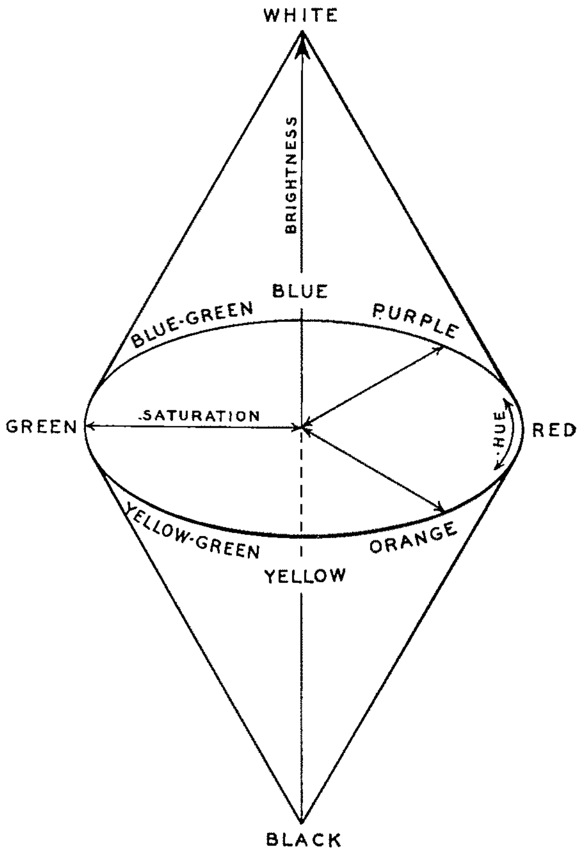

FIG. 1. A schematic representation of the psychological dimensions of color space

must satisfy some very special requirements. First, any color in the set must practically never be confused with any other color in the set. Second, every color in the set should be readily associated with a common color name. Third, some color codes should be usable by ordinary people with little or no specialized training in color. And therein lies the origin of our problem for today.

The Dimensions of Color Space

Color sensations can be classified and ordered without any reference to the characteristics of the stimuli which arouse them. Over the past

several hundred years, artists, philosophers, and scientists have devised many such classifications of colors according to their similarities and differences. Almost without exception, all such schemes agree that some sort of a three-dimensional model is needed to represent adequately the full gamut of color sensations which the normal person experiences. Figure 1 shows a diagram which is widely used by psychologists and color scientists for this purpose.

Hue: Hue is perhaps the most important of the three fundamental variables of color as a mental phenomenon. It is the main *quality* factor in color and is what the ordinary person means when he says color. Another way of saying it is that hue is the essential element which leads us to refer to colors by such distinctive names as red, yellow, green, blue, violet, and so on. Hue sensations do not, however, occur in discrete groups. Instead, they shade imperceptibly from one to another and, indeed, form a complete circle, as illustrated in Figure 1. Starting with red, for example, one can describe color sensations which become progressively yellower, that is, the red first becomes orange-red, then orange, yellow-orange, and finally yellow. From yellow one can proceed similarly by continuous gradations to green, blue, violet, and back to red again.

Brightness (lightness): The second variable of color sensation is brightness, the *quantitative* aspect of color sensation. It is easy to imagine two colors, say red, of identical hue but differing in brightness. Common terms which are used to refer to variations in brightness are *light* and *dark*. As with hue, however, the brightness dimension also forms a continuum, shading imperceptibly from very light to very dark hues. In technical work, *brightness* is used to refer to variations in the intensity of lights; *lightness* to variations in the intensity of surface colors.

Saturation: The third variable of color sensation is the most difficult to explain in words alone, without reference to actual color samples. Perhaps the best way of defining saturation is to say that it is the percentage of pure hue in a color. In this sense, it is roughly parallel to the concept of the purity of a chemical compound. In common speech, words such as *pale* or *deep*, *weak* or *strong* appear to refer to variations in saturation. Light brown, for example, is a weakly saturated yellow red of medium lightness, and moderate pink is a weakly saturated light red. In Figure 1, saturation is represented by radii originating at the center of the diagram and extending in all directions from the center. In this diagram, white, gray, and black are colors of zero saturation and thus of no hue. The white-gray-black continuum varies only in brightness, or lightness.

Color Space Defined: Now, when I say *color space* I mean that three-dimensional space which models color in all possible variations of hue, lightness, and saturation.

The Munsell Color System: Although one can study color sensations

without reference to physical stimuli, it is nonetheless true that sensations of color are most consistently and readily elicited by stimuli of the appropriate kinds. The most useful general set of surface colors available for scientific and technical work is found in the Munsell *Book of Color*. This is a large collection of carefully prepared colored chips, spaced to represent about equally noticeable color differences. Taken together, the Munsell chips cover virtually the full gamut of all colors reproducible by ordinary paints and pigments. The master colors of the Munsell system have also been deposited with the National Bureau of Standards, where they have been measured and calibrated so that they can be related to other color measurement systems. In addition to illustrating the dimensions of color space, the Munsell system is extremely useful for specifying colors in business, science, and industry. If a manufacturer specifies the color of one of his products in Munsell terms, anyone else can go to a Munsell atlas and find out precisely what color he means.

For all its advantages, the Munsell system is a collection of standardized colors useful primarily as a system of color nomenclature and specification for scientific and technical work. The language of the Munsell system is foreign and strange sounding to the uninitiated. The lightness dimension becomes *value* in Munsell terms, and saturation is called *chroma*. Chips bear names like 2.5 YR 4/6, where the 2.5 YR refers to a particular hue, the 4 to a specific lightness (or value), and the 6 to a particular saturation (or chroma). One can easily find out with great precision exactly what such a color looks like, but the terminology of the Munsell system is hardly one that could be used for ordinary color descriptions.

I shall refer to the Munsell system later when I come to some of the experimental work I want to discuss. At that time, however, my interest will not be in the Munsell system as a system, but only as a comprehensive set of stimuli for studying color names.

On the Total Number of Discriminable Colors

One approach to our problem of colors and color names is to find out how many discriminable colors there are in color space. No one really knows this. We can, however, arrive at an estimate of this number (Nickerson and Newhall, 1943) by discovering how many discriminable colors there are in the hue circle (about 200), multiplying these by the number of discriminable steps in the lightness dimension (about 450), and multiplying that product by the number of discriminable steps in saturation (this varies from about 15 to 165 in different parts of color space). The final outcome of such a series of computations is a very large number—about 7,295,000 discriminable colors!

Although this might appear to constitute an enormously useful reservoir of colors upon which to draw, it turns out to be of virtually no

use at all for the kind of practical problem we started with. Let's look at the difficulties with this approach for, in so doing, we will be able to see more clearly the complexities of this problem.

Absolute versus Comparative Judgments: The first difficulty with the computations above is that they are based on values obtained with the most sensitive methods available to the psychophysicist. Generally, this means two colors presented side-by-side, under good viewing conditions, and with the observer being required to state merely whether the two are the same or different. This involves a comparative judgment, a kind of discrimination which the human eye can make with exquisite precision.

But the situations in which colors are used for coding information do not permit such niceties of observation. In an information display, a color may appear by itself and the observer may be required to identify it as being this or that. Observations performed under these conditions are sometimes referred to as *absolute judgments* in the psychological literature (Chapanis and Halsey, 1956).

One of the fascinating things about these two kinds of situations is the tremendous shrinkage which occurs when one goes from comparative to absolute judgments. Take, for example, the visible portion of the electromagnetic spectrum, that region stretching from approximately 380 to 780 mμ. If one explores this region step-by-step using the most refined methods available to the psychophysicist, he will discover that there are upwards of 150 discriminable wave lengths in the visible spectrum (Wright, 1947). Now present a set of spectrum colors one by one and ask the observer to attach an arbitrary label to each one. Even after observers have had extended practice, the maximum number of different colors which they can identify without appreciable error is no more than 12 or 13! (Halsey and Chapanis, 1951; Chapanis and Halsey, 1956). This shrinkage from 150 to less than one-tenth that number represents a tremendous contraction in the pool of colors available for practical work.

Individual Differences: There is still a further complication that we must face in this business if we insist that colors be labeled with common names instead of arbitrary labels. This complication is that people appear to differ in what they see—just as they differ in height, weight, reaction time, or in any of the thousand or more ways we can measure them. Incidentally, let me make it absolutely clear that in this discussion I am eliminating from consideration all persons with color-vision defects. The differences I am concerned with are differences one can find among so-called color-normal individuals.

Let us return again to our visible spectrum, spread it out before an observer and ask him to locate that part of the spectrum that represents the purest orange, the purest yellow, the purest green, and so on. Let the observer take his time and ask him to repeat his determinations several times. Compare the results you get from several observers and

you are sure to discover a remarkable thing. What appears to one observer as a pure yellow looks orange to another. What one observer is willing to call a yellowish-green, another will call bluish-green. And so on. Each observer is internally consistent, that is, he can repeat his own determinations with remarkable accuracy. But the differences between individuals are disconcertingly large (see, for example, Dimmick and Hubbard, 1939; Halsey, 1959; and Pickford, 1951).

What is the source of such inconsistencies? Do they arise from genuine differences between visual systems—differences between the way your eye and mine respond to the same stimuli? Or are such discrepancies primarily due to differences in the way we have learned to attach color names to our color sensations? These are intriguing questions and, at the moment, we can only speculate about the answers to them. But, however they arise, differences in the way each of us uses color names are one of the added complications we shall clearly have to face in our search for color names that we can use in color space.

How Many Color Names Are There?

Another way of tackling our problem might be to go at it by way of color names themselves. What is the situation here? No one really knows how many different color names there are. Kelly and Judd prepared a dictionary of color terms for the National Bureau of Standards in 1955, and that compilation contains over 5000 different color names. The difficulty, however, is that the number of color names, unlike the number of discriminable colors, is not some fixed, finite number. There are people in our world of advertising and industry who dedicate themselves to the invention of new color names. Indeed, the guiding principle for people in this kind of work is "New names for old colors every year." As a result, we find ourselves being deluged with such exotic names as *afterglow, air castle blue, Aladdin's lamp, Andrinople berries, angel blue, angel red, apache, aphrodite, April sky, Arab, arabesque, atlantis, atonement, Australian pine, autumn blonde, autumn brown, autumn glory, autumn gold, autumn green, autumn leaf, autumn oak,* and *autumn tan,* just to name a few that begin with the letter "a." Rest assured that these are not my inventions. Some manufacturer, somewhere, has used each of these names to refer to some particular shade of lipstick, face powder, fabric, or tile.

There isn't even agreement in the different segments of industry about the precise definition of some of these terms. For example, in the Plochere color system used by interior decorators, *autumn brown* refers to a strong yellowish brown, a yellowish brown of high saturation. In the Standard Color Card put out by the Textile Color Card Association, however, *autumn brown* refers to a moderate brown, a brown very low in saturation.

If we eliminate all obvious inventions of the advertising arts, we can

still find many color terms which have little meaning for the average person. Indeed, it is extremely unlikely that terms such as *alabaster, amethyst, caeruleum, carmine, chartreuse, claret, cyan, ebony, fuchsia, heather, heliotrope, hemp, indigo, japonica, jasmine, madder*, and *mauve* can even be found within the vocabulary of the average American adult. Let me remind you that, according to the American Education Association, in 1960 the median adult in these United States had completed only eleven years of schooling. The very fact that you are reading this means that you are all members of a select minority of our population. Once again, when one finds people who know and use specialized color terms in their work, we often find disagreements in what they mean by these words. In the field of color photography, for example, *cyan* refers to a color between blue and green. To an artist, however, *cyan* is a much bluer color. What an artist calls *red* is *red magenta* to a color photographer. And so on.

I think you will agree that our brief excursion into the world of color names has so far been a singularly unrewarding exercise. One quickly becomes bewildered by their sheer numbers, confused by the lack of agreement about what they mean, and mystified by their exotic and esoteric qualities.

Color Names in Common Use: Let us now follow another avenue and ask what color names one finds in common use. One of the few studies I know of this problem is reported by Pauline Evans (cited in Evans, 1948). She surveyed seventeen best-selling novels and found 4416 color terms used in these books. Of that total number, 4081, or 92.4% were accounted for by only 12 terms. These, arranged in order of most to least common, are: white, black, blue, red, gray, green, brown, gold, yellow, pink, silver, and purple. Of the total of 4416 terms, 4066, or 92.1% were unmodified. Only 7.9% were modified with words like "dark," "pale," "pinkish," and so on.

The common language, in other words, appears to get along with no more than about a dozen color names. But surely there must be some middle ground. One can't help feeling that if he looks hard enough he ought to be able to find more than a dozen color names to blanket the vast domain of color we can all see.

The Denotative Meaning of Some Common Color Names

My latest approach to this problem has been a direct frontal assault on the question of what people mean by various color names. In particular, I have been looking for consistencies, and for differences, among color names in terms of how normal people use them in designating real colors.

There are at least two ways one could go about this kind of problem. One is to show an observer a large number of different colors and ask

him to name them. This technique, I think, has a number of disadvantages. If you do not limit the number of color terms you allow the observer to use, you will generally end up with such a large assortment of different names, with and without qualifiers, that it is difficult to know what to do with them. There is no easy way of quantifying the outcome of an experiment of that type.

For this reason, we did the reverse kind of experiment. We spread out a large array of colors before an observer, then gave him a color name and asked him to find that one color which, in his opinion, best exemplified that color name. In short, we asked the observer to define what each color name meant to him by pointing to a particular color.

TABLE 1 THE BASIC COLOR NAMES AND MODIFIERS USED IN THIS STUDY. THE GROUPINGS OF THE BASIC COLOR NAMES SHOW THE SIX SETS INTO WHICH THE COLOR NAMES WERE DIVIDED DURING THE ADMINISTRATION OF THE TESTS. EXCEPT AS NOTED BELOW, EVERY COLOR NAME WAS USED WITH EVERY MODIFIER

Basic Color Names		*Modifiers*
Red	Green	(No modifier)
Pink§	Yellowish green	Vivid
White*	Yellow green	Strong
Gray†	Greenish yellow	Pure
Black*	Olive	Deep
		Dark
Yellow	Blue	Light
Orange	Bluish green	Pale
Brown	Greenish blue	Grayish
		...(ish) White
Purple	Reddish purple	...(ish) Gray
Violet	Purplish red	...(ish) Black
Purplish blue	Purplish pink§	

* No modifiers were used with these color names.
† This name was used unmodified and with each of the following modifiers: *Light, Dark, Pure, Medium.*
§ The modifier *...ish Black* was not used with these color names.

The Color Names Tested: Table 1 shows the color names we have tested. As you see, the color names are made up of two parts, what we call (1) basic color names, and (2) modifiers. By and large, these basic color names and modifiers were selected from the National Bureau of Standards dictionary of color terms (Kelly and Judd, 1955), although we have introduced some additional terms of our own. With the few exceptions noted in the footnote to the table all basic color names were paired with all modifiers. Some examples of our color names are *vivid green, pale blue, deep olive, grayish purplish blue,* and *violet black.* These pairings of modifiers with names gave us a total of 233 color names, the number tested in this investigation.

General Testing Procedure: For our colors we used all the color chips in the Cabinet Edition of the Munsell Book of Color (1226 in all) and added to them 133 special colored chips generally of higher saturation than are contained in the book. We ended up, therefore, with a total of 1359 different samples of color.

These colored chips were spread out on a large table, under a carefully controlled source of illumination approximating natural daylight. The observer stood, or, if he wished, sat, before this display. He was given a color name, for example, *vivid brown*, and instructed to find that one

TABLE 2 SELECTIONS MADE FOR FOUR COLOR NAMES (ABOVE) AND SOME OF THE STATISTICS COMPUTED ON THE DATA (BELOW)

Munsell Chip Selected	*Red*	*Pure Red*	*Vivid Red*	*Strong Red*
10.0 RP 3/14				1
10.0 RP 4/10			1	
10.0 RP 5/12				1
2.5 R 3/10				3
2.5 R 4/12	1	1		1
2.5 R 4/14	3	8	6	4
2.5 R 5/14	4	3	1	1
5.0 R 3/12	1			4
5.0 R 4/12	1			1
5.0 R 4/14	17	14	15	12
5.0 R 5/14	4	1	4	2
5.0 R 6/12			1	
7.5 R 3/12				2
7.5 R 4/12			1	
7.5 R 4/14	8	10	7	8
7.5 R 5/14	1	3	4	
N	40	40	40	40
n	9	7	9	12
Modal f (M)	17	14	15	12
M'	25	24	22	20
M''	29	32	28	24
Consistency, C	0.54	0.56	0.52	0.43
Mean	5.1 R 4.2/13.8	5.1 R 4.2/14.0	5.2 R 4.3/13.8	4.8 R 3.8/1
Overlap, 0		78	76	60
I_t		0.08	0.09	0.26

color which, in his opinion, was the best vivid brown on the table. He was given unlimited time to make his selection and he was allowed to pick up the cards containing the colored samples, move them about, or handle them in any way that would make his search easier.

To reduce this task to manageable proportions, we did the testing by groups of colors (as shown in Table 1). For example, all of the color names containing *yellow*, *orange*, and *brown* were tested as a group. In this way we were able to modify the size of the display and the number

of colors through which the observer had to search. We made sure, of course, that the range of colors displayed was more than adequate to cover those he might want to search through. At any one time, however, a display contained no more than about 550 colors.

The six different sets of colors, and all the color names within a set, were tested in a different random order for each of the observers used.

The Observers: The observers were 40 young adults, 20 men and 20 women. They were required to have normal color vision, good visual acuity, and to speak English as their native tongue. The observers were housewives, students, secretaries, and teachers. As a group they were undoubtedly above the average intelligence of the population as a whole.

The Basic Form of the Data: At the conclusion of many weeks of testing we ended up with 9320 color selections—40 for each of the 233 color names. Table 2 shows the way in which the raw data were tabulated for four of them. It also shows some of the statistical measures that were computed to describe the selections.

Our first interest was in getting some measure of the consistency with which the 40 subjects could agree in their selections. To this end we first determined n, the total number of different selections made for each color name. Next we computed the frequency of the modal, or most common selection, M; the sum of the frequencies of the two most common selections, M'; and the sum of the frequencies of the three most common selections, M''. Finally, we used a rather complicated statistic which I borrowed from communication theory, although we used the measure simply as a summarizing statistic having nothing to do with communication. This measure, which I call the coefficient of consistency is defined by:

$$C = [\Sigma(f \log_2 f)] \div [40 \log_2 40]$$

where f is the number of times each of the n color chips was selected. This measure of consistency, or agreement, ranges from 0.00 to 1.00. If all 40 subjects made exactly the same choice for a color name, the value of C would be 1.00. If each of the 40 subjects made a different choice for a color name, the coefficient would be 0.00.

It turns out that all five measures of consistency (n, M, M', M'', and C) were highly correlated, but that the coefficient of consistency, C, gave the highest average intercorrelation with all the other measures (+0.95). Consequently this is the only one I shall refer to from now on.

Consistencies for All 233 Color Names: The distribution of consistencies for the 233 color names is shown in Figure 2. As you see, observers never agreed completely in their selections for any color name; on the other hand, they never disagreed completely either. Still, taken as a whole, the distribution gives the impression of low rather than high amounts of

agreement. The median value is only 0.29, and the consistencies for 219, or 94 per cent, of the color names are less than 0.50.

To some extent, this impression of instability in the color selections must be tempered with the observation that our measure of consistency tends to be biased toward the lower values. As I have already said, a value of 0.00 would result if all 40 observers had picked a different color, 40 in all, for a particular color name. On the other hand, a value of 0.29

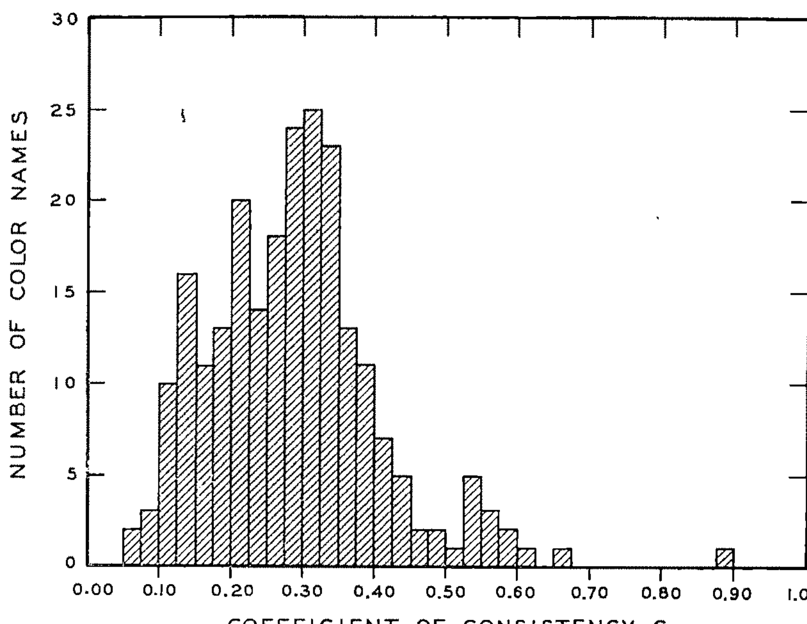

FIG. 2. Coefficients of consistency for the selections made to the 233 color names.

(the median for all color names) could occur if the 40 observers had picked only 14 different colors—three observers each agreeing on one of 13 colors and the fortieth picking a fourteenth color. A value of 0.52 could occur if the 40 observers had picked only 6 different colors— seven observers each agreeing on one of 5 colors with the remaining 5 agreeing on the sixth. To sum up, then, the agreement among color selections is better than the distribution in Figure 2 seems to suggest.

Consistency of Selections Made by the Two Sexes: Since we tested 20 men and 20 women, it is natural to ask: Were there any sex differences, and, if so, which sex was more consistent? The answer is that the women were significantly more consistent. This finding is in agreement with those of other studies which show that women or girls tend to be better than men or boys in other kinds of color naming (see, for example, DuBois, 1939, and Ligon, 1932).

Consistencies for Groups of Color Names: Figure 3 reveals some interesting differences among the 19 basic color names. Each horizontal line shows the range of four C's: that for the unmodified basic color name, and for the basic color name modified by "pure," "strong," and "vivid." The short vertical line shows the mean of these four values of C. The heavy dot shows the mean value of C for each basic color name in combination with all 12 modifiers.

The white, gray, and black color names fall into a class by itself and I shall not say anything about them except to point out that the observers were generally able to agree very well about them.

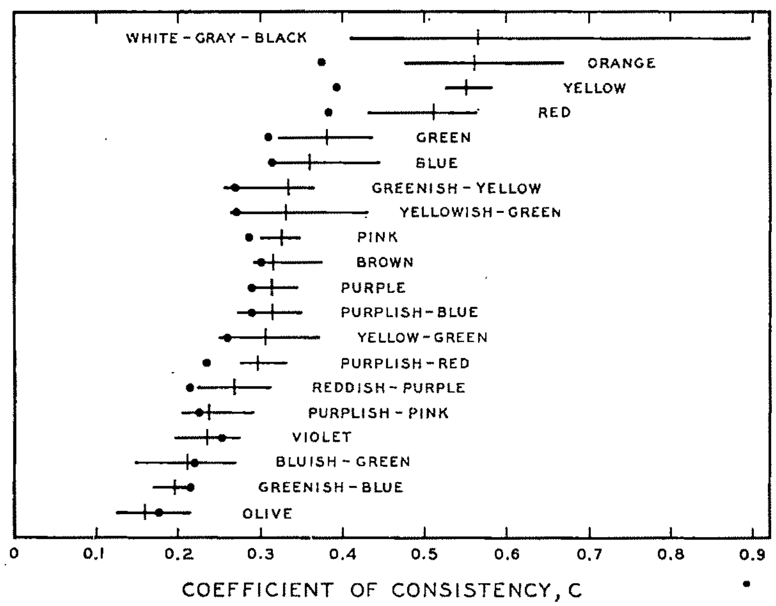

FIG. 3. Consistencies of selections made to the basic color names.

Observers tended to be highly consistent in their selections for *orange*, *yellow*, *red*, *green*, and *blue*. Selections for *purple* and *violet* showed a considerable amount of variability, and our observers found *olive* the most difficult color to define precisely. It is interesting to note, too, that observers were always less consistent in their definitions of compound color names than simple color names. The agreement for *yellow-green*, for example, is less than that for either *yellow* or for *green*.

Figure 4 shows that there were some differences between the various modifiers, although these differences do not appear to be as great as those shown in Figure 3 for the basic color names. Poorest agreement was shown for the modifiers . . . *gray* and *grayish* . . . , as, for example,

reddish gray and *grayish red.* Greatest consistencies were obtained for the color names which imply highly saturated hues, for example, *red, vivid red, strong red,* and *pure red.*

Incidentally, appropriate statistical tests show that the differences in Figures 3 and 4 are highly significant.

Mean Hue Selections for the Strong Hues: The discussion so far has been concerned only with how well the 40 observers could agree in their definitions of various color names. We calculated a mean hue, a mean value (lightness), and a mean chroma (or saturation) for the selections made to each color name (see Table 2 for examples). I shall have time here to discuss only some of the findings for the hue selections.

The mean hues for all the *strong* colors are shown in Figure 5 as arrows radiating out from the center. The scale and the symbols around the

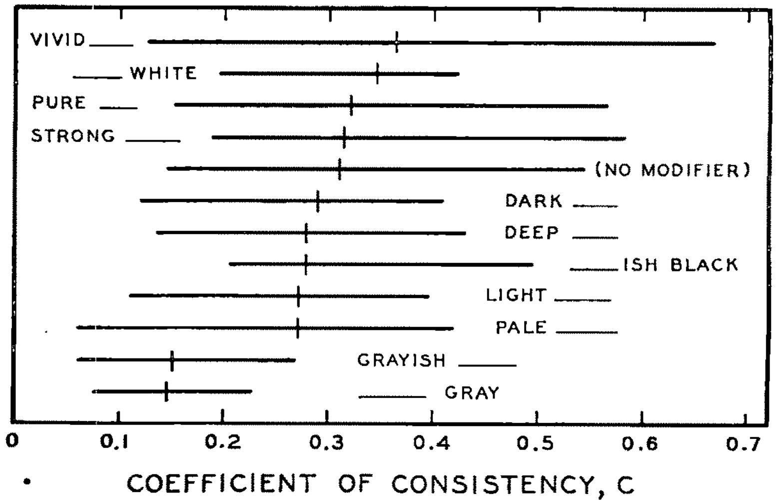

FIG. 4. Consistencies of selections according to color name modifiers.

inside of the circle show the locations of the 40 principal Munsell hues. The short arcs on the outside of the circle are ranges for *strong* hues as defined by color experts of the Inter-Society Color Council and the National Bureau of Standards (Kelly and Judd, 1955). In some cases, the agreement between the ISCC-NBS definitions and what our observers did is excellent. This is true, for example, of *strong red, strong orange,* and *strong yellow.* In other cases, the agreement is poor. Note, for example, that the mean selection for *strong green* is completely outside the range of values specified by the ISCC-NBS. A particularly interesting comparison is that between *strong purple* and *strong violet.* Color experts agree that violet should be a bluer color than purple. Our observers,

however, could make practically no distinction between these two colors. In fact, they even reversed the two!

Another highly interesting point concerns the compound colors, such as *greenish yellow*, *yellow green*, and *yellowish green*. These turn out to be particularly difficult. Although color experts think of *yellowish green* and *greenish yellow* (or *bluish green* and *greenish blue*) as being distinctly different colors, our observers scarcely differentiated between them at all.

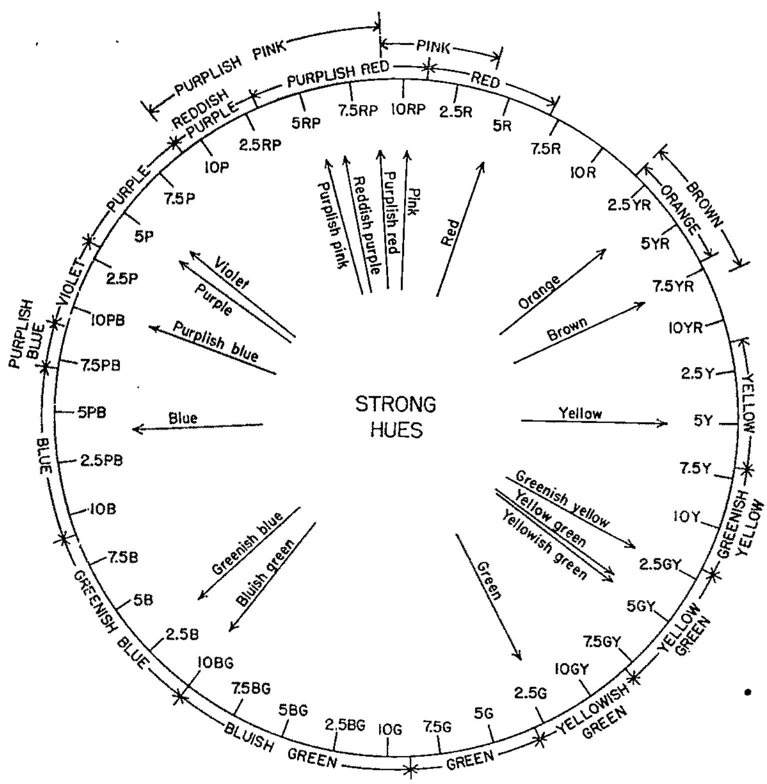

FIG. 5. Average hue selections for the *strong* hues (arrows). The scale and symbols around the inside of the circle are designations for the Munsell hues. The segments of arcs on the outside of the circle show the ranges of hues defined by the ISCC-NBS method of designating colors.

The last point of interest in Figure 5 concerns the distribution of the mean selections. Recall that the Munsell hues are spaced to correspond to equally noticeable color differences, that is, equal distances around the perimeter of the circle correspond to equally discriminable hue differences. When we first made our selection of 19 basic color names for this study (those in Table 1) we tried to pick names which would blanket the hue circle almost uniformly. Our findings show that we fell far short

of this goal. There is a large region between most of the Munsell greens and all the blue-greens that was not sampled at all by our color names. There is another large empty space between *greenish blue* and *blue* and still another between *violet* and *purplish pink*. These regions contain a great many colors which the eye can discriminate if it wants to. But apparently we have not found these colors to be sufficiently interesting to reward them with their own distinctive names.

On the Overlap between Different Color Names: The selections of color chips were sometimes nearly identical for different color names. Table 2 illustrates some of these. Note, for example, that 5.0 R 4/14 was the most common selection for *red, pure red, vivid red,* and *strong red.* In fact, only two Munsell chips (5.0 R 4/14 and 7.5 R 4/14) accounted for over half of all the color selections made for these four color names. Another way of saying this is that our observers regard *red, pure red, vivid red,* and *strong red* as being essentially the same color. This conclusion is supported by the means of the selections made to these four names (Table 2). The maximum discrepancy among the four mean hues, mean values, and mean chromas is less than half the difference between any two color chips adjacent in the Munsell system.

Two measures were computed to quantify the overlap between the selections made to pairs of colors. One measure was a simple tally of the number of times identical chips were selected for different color names. The other, again borrowed from communication theory, is a measure of the information transmitted (or, as it has also been termed, the contingent uncertainty [Garner, 1962]) between color names and color selections. Here again, think of this only as a statistical measure of association, varying, for these data, between 0.00 and 1.00. These two measures agree so highly that only the first was computed for all the data. Measures of overlap are now available for all 27,028 pairs of colors so that we can determine which color names overlap and by how much.

The results of this analysis reveal that there is almost complete synonymy between *violet* and *purple,* and between *yellowish green* and *yellow green.* This identity holds not only for the basic color names but for the basic color names used with all eleven modifiers. Although there is somewhat less overlap between *yellow green* and *yellowish green,* between *bluish green* and *greenish blue,* and between *reddish purple* and *purplish red,* it is sufficient for us to conclude that we could not expect people to make dependable discriminations between the members of each pair.

The situation with regard to modifiers is, in some respects, even more interesting. First, the modifiers *pure, strong,* and *vivid* appear to add nothing to a basic hue name. For all practical purposes, *pure blue, vivid blue,* and *strong blue* are synonymous with just plain *blue. Deep* and *dark* also turn out to be synonymous, as do *pale* and *light.* Moreover, these

generalizations hold for all nineteen basic color names. All these findings are at variance with the ISCC-NBS system of designations in which these modifiers were supposed to identify distinct variations in lightness and saturation. Unfortunately, the ordinary person does not make these distinctions.

There are also numerous other pairs of color names which overlap but these are more or less isolated instances and do not hold for entire sets of modifiers or basic color names. For example, although the *pink* and *red* colors appear to be differentiated on the whole, *pinkish white* and *reddish white* are not. Similarly, *orange* and *brown* appear to be distinctive except for *orange black* and *brownish black*.

On the Total Number of Distinctive Color Names: We turn now to what is probably the most interesting question, essentially the question we started with: How many distinctive color names are there? Imagine that we were to construct a very large table of data. Across the top of the table, we list the color chips starting with the first and continuing through the 1359th. Along the side of the table we list color names, starting with the first and ending with the 233rd. Inside the table we enter in the appropriate cells, all the 9320 color selections made by our observers. What we will have, in short, is a kind of gigantic correlation table, containing 1359 times 233 cells.

The measure of information transmitted (or contingent uncertainty), to which I have already referred, is a kind of statistic which is ideally suited for data of this type. If we calculate such a measure for this large table of data we obtain a result equal to 5.49 bits. The antilog of 5.49 bits is 45. This means that there are theoretically 45 different color names for which color selections should not overlap (Garner, 1962).

Unfortunately, when we examine our table of data we find that, in still another respect, our selection of color names was not as good as we had hoped. Of the total of 1359 chips, 246, or about 18 per cent, were never selected at all. Our theoretical number of 45 color names does not cover the entire color space of the Munsell system but only about 82 per cent of it.

We can, however, make some estimate of what the total number of color names could be and bracket it with some upper and lower limits. For the lower limit, let's assume that we managed to find some additional color names to sample the unused 246 color chips. Let's assume, further, that the agreement for these new color names is no better than the agreement for that of our most variable color name, *pale olive*. Under these assumptions we would need an additional seven color names to cover the unsampled area of the Munsell system.

Let's follow another line of reasoning to see what it will give us as an estimate. If 45 different color names can cover 1113 chips, on the

average, how many color names would we need to cover 1359 chips? That turns out to be about 55.

By putting all these computations together, we may say that the total number of different color names that could be used for color space (as represented by the Munsell system) is most probably between 52 and 55.

Postscript and Prospect

Let's stop now, take a look backwards at what I've tried to say, and then a look forwards at what this means for future research.

I started with a question: What is the maximum number of usable color names for all of color space? We saw first that the normal human eye is capable of discriminating upwards of 7,000,000 different colors, but that this number is not at all a fair estimate of the reservoir of practically usable colors. We saw then that the English language contains thousands of different color names and that our language is flexible enough, and our culture rewarding enough, to encourage the proliferation of color names, although most of us have little idea what these color names mean. On the other hand, the common English language, the language of the novel, seems to get along well with no more than about a dozen different color names.

A direct experimental attack on the denotative meaning of various color names has revealed some interesting things about what people mean by various color names. We saw that some color names which appear to be quite different are, for all practical purposes, synonymous. We saw also that people do not make distinctions between certain modifiers which appear to convey the idea of variations in saturation and lightness. And, finally, we ended up with the estimate that there are probably between 52 and 55 usable color names for all of color space.

As is so often the case with scientific research, the questions which we have uncovered are far more numerous than those we have answered. Our 52 to 55 usable color names are theoretical, rather than actual. Inspection of our data identifies a number of names which do in fact refer to different regions of color space. But what of the unsampled parts of color space? Can we find color names for them? Are there indeed simple color names which we can use for the hues which lie between green and blue-green? Or for the low-saturation hues, which were also conspicuously avoided?

How do we learn color names? Where and how in the genetic development of a child do associations get established between color names and color sensations? How does the child learn to make the discriminations that provide him with the color language he has as an adult?

Would we have obtained the same results if we had tested observers who know British English instead of American English? Would a French psychologist have discovered the same number of usable color names as

we did? Or a German psychologist? In short, have we discovered something reasonably constant about human color perception, or have we discovered something that is specific to people who speak a particular language?

These are only a few of the intriguing questions that occur to me, and I am sure, to you as well.

In conclusion, if my brief remarks today have colored your thoughts with the warm glow of *crushed strawberry*, *sunglow*, *coral blush*, or *mountain haze*, I will feel that I have, in part at least, earned the privilege of addressing you. But if, by some stroke of good fortune, I may have enticed a few of you into exploring these questions further, I will feel that I have been most richly rewarded.

Acknowledgments

I am grateful for the assistance of Dr. Aleeza C. Beare who collaborated with me in some of the research described here. The work reported in this paper was done in part under Contract Nonr-248(55) and in part under Contract Nonr-4010(03) between the Office of Naval Research and The Johns Hopkins University. It is Report No. 12 under the latter contract. Reproduction in whole or in part is permitted for any purpose of the United States Government.

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AMERICAN SCIENTIST, 53, 1965

ON THE PRESENT INCOMPLETENESS OF MATHEMATICAL ECOLOGY[1]

By L. B. SLOBODKIN

ALMOST all fields of science are growing. The number of journals published per unit time; the number of Ph.D.'s and engineers produced; the dollars spent; facts learned; institutes; seminars; interdisciplinary, international, coordinated, crash, and other programs are all increasing at a dizzying rate. Publishers, symposium organizers, committee chairmen, editors, deans, and deanlets all compete for time with students and research.

To be a scientist in 1965 is a delightfully hectic and exciting business. Scientists are presumed to have certain intellectual abilities and standards which make them of value in supporting political, social, and religious movements and institutions so that each scientist must carefully budget his time, clarify his own opinions and, at all costs, maintain an honest intellectual perspective on the relative merit and state of programs and ideas, particularly in his own research area.

However, not all of the areas of science are growing at the same rate. The most rapidly growing areas are those in which there is essential unanimity on which problems are considered important and which techniques are appropriate to their solution. The more slowly growing areas are more difficult to characterize.

In my own area, ecology, there is an intermediate growth rate, despite a general sense of the practical urgency of certain ecological problems.

Every year, new ecological programs are starting at various points around the world. In calorimetry, the first work was done less than six years ago but the number of laboratories engaged has gone from one to more than ten in that time. Six years ago, life table data for animals were almost absent, but Alex Comfort, in revising his book on senescence in 1964, had simply to drop the project of summarizing the existing literature [2]. New journals and review series are appearing on all the continents. The demand for teachers far outruns our supply.

However, while the field grows in a satisfactory fashion, the growth does not have the monolithic, Gargantuan aspect that characterizes the burgeoning of, say, cell biology. The number of good quantitative ecologists is in the thirties or forties for the entire world, rather than the thousands of biochemists and geneticists.

This relatively slow growth, combined with the lack of emergence of any one central problem which acts as a bandwagon for all of the field, implies that quantitative ecology is an unfinished or incomplete field in some different sense from genetics or nuclear physics. While all the rami-

fications of genetics and nuclear physics are not yet known, there is general consensus on the present broad theoretical format of these fields. While new facts are yet to be found and present theories may be overthrown, it is at least clear which ideas are most significant to test. Ecology, on the other hand, does not yet have any central theoretical formulation. I will try to explain, in part, the relatively slow and diffuse growth pattern of ecology and will then discuss some of the problems raised by sciences that share the incomplete character of ecology. I will then contend that there exist sciences whose present significance lies in the formulation of new thought patterns rather than in the prediction of events in terms of the more traditional mathematical and logical systems.

Initially, ecological theory had the avowed intent of quantifying natural history, but this is itself an ambiguous concept. To count all the animals on earth and then publish that number is a trivial kind of activity. It is, however, far from trivial to develop a theory which permits prediction of the total number of animals from a reasonably small group of measurements—in fact, it would be an intellectual triumph of major proportions.

The attempt to develop a quantitative theory of ecology, starting with the pioneer studies of Lotka, Voltera, D'Ancona, Kostitsyn, and others [3], was probably motivated by the conviction that underlying the endless special case descriptions of the interactions between organisms and their environments lay a relatively simple mathematical theory. Repeatedly, the analogy with one or another branch of physics appeared in their writing and still appears in current papers [4]. Some of the questions that are asked also have about them an air of physics-like generality.

What is the pattern of population growth?
Is there a constant ecological efficiency? . . . or more naively
What determines the numbers of animals? . . . and so on.

These are eminently fair questions, but in the experimental and field analyses several things became apparent. For example, the simple models of population growth, beloved of the mathematician and ideally amenable to grandiose theory formation, simply do not hold. The lovely sigmoid pattern found in certain protozoa and bacteria does not generally happen in metazoans, and if it does happen, it happens for the wrong reasons. That is, an elegantly simple mathematical expression for sigmoid growth is

$$\frac{dN}{dT} = aN - bN^2$$

For bacteria, a and b have operationally clear meanings of a simple kind and N is an enumeration. For metazoans a similar shape may, under

certain circumstances, arise, but the equation nevertheless doesn't apply since, among other things, N has no operational meaning—an old animal is not equivalent to a young one nor is a large one equivalent to a small one, etc. It is still possible to develop mathematical theories of population growth but they begin to take on the aspect of *coffer hypotheses*—long series of special constants and elaborations are introduced to make the mathematical theory fit some particular example or case—and on occasion, this, in itself, can become a fascinating exercise in the solution of practical problems, the use of computers, or in the development of applied mathematics [5]. However, while special cases can be successfully analyzed, generalizations derived from them have not been remarkably productive.

One attempted solution, in the face of this difficulty, is to abandon all effort at maintaining the correspondence between mathematical theory and field observation and to develop rather elegant mathematical formulations which provide insight rather than prediction. Typically this type of analysis is designed to make an apparently paradoxical set of events intuitively acceptable. For example, numerous cases are known in which reproductive potential seems to have decreased during the course of evolution, in apparent contradiction to certain general evolutionary theorems. Various, plausible sounding, abstract systems have been constructed to demonstrate how this might have occurred [6]. The difficulty here is in deciding how these hypothetical systems relate either to more conventional systems with predictive value or to any specific natural situation.

This is not equivalent to saying that only field observation can be used to explain field phenomena—in fact, it is almost certain at the moment that field observation by itself, in the absence of laboratory and theoretical study, is almost useless in providing explanations. The way in which the various kinds of study fit together is that theoretical and laboratory analyses can provide limits for the possible functions of various processes in the field—thereby permitting, and, in fact, making possible, decisions between various alternative hypotheses with respect to field data.

For example, populations of hydra in any particular area of a small lake tend to fluctuate rapidly. Laboratory populations of the same animal do not seem capable of increasing at the rate observed in the field, implying that the animals which appear at any point in the lake have in part originated elsewhere. Simultaneous collections from various parts of the lake show that sudden increases in one place are fairly well correlated with disappearances elsewhere, as if the hydra migrated, but the observed rate of movement of hydra in the lake is inadequate to account for the migrations [7]. Under deleterious conditions in the laboratory, hydra will secrete a gas bubble at the pedal disc, and float from the substrate, sink-

ing when the bubble is either burst or lost. This of course provides an ideal mechanism for animals to leave one place in a lake and rapidly accumulate somewhere else. Crowding, as such, triggers bubble formation [8]. In this case, the laboratory study and the field study lean on each other. Both of these grew out of a laboratory study on population dynamics in hydra which was undertaken to determine the efficiency with which hydra can transform food into flesh that can be consumed by a predator, and more particularly to see if this efficiency is the same as that of Daphnia. (Incidentally it is, and this also poses a problem in its own right [9].) The Daphnia experiments [10] in turn grew out of an attempt to see if the mathematically elegant logistic equation really applies to metazoans (which it does not).

The sequence of studies indicated above is typical of many of the various studies that have been made in quantitative ecology. We start with a high standard of mathematical elegance which generates plausible sounding statements. At some point, these statements are experimentally or observationally tested. Typically, they are refuted in a limited sense, (as for example, while Daphnia populations do not grow according to the logistic equation, I can concisely describe how they do grow by saying "It is like the logistic equation, if the logistic equation would take cognizance of time lags and differences in both needs and contributions of different kinds of animals,") but the empirical studies themselves are sufficiently fascinating to generate their own hypotheses and further empirical work. Finally, we find that we are talking about some very specific mechanism, like the bubble formation of hydra, which essentially brings us to another ecological generality. That is, on the basis of laboratory studies with Daphnia, fungus flies, mice, and other organisms, it is fair to say that every population has some mechanism of escaping from its environment as that environment begins to become crowded. The physiological basis of the escape mechanism varies from species to species but its ecological role seems constant. We could predict therefore that hydra would have some escape mechanism but we could not have predicted that it would involve making a bubble [11].

In short, we are talking about a research area that generates its own problems and generalizations and, in that sense, is a delightful thing to work with; but notice how we have departed from the original dream of a physics-like theory with high mathematical elegance. The critical incompleteness of quantitative ecology lies in its mathematical aspect, which is extremely odd since most of the subareas of ecology start from a mathematical formulation as clear as that of any physical problem. Despite the long time, the competent minds, and the plethora of data, progress in the mathematics of mathematical ecology has not been as satisfactory as one might have hoped in, say, 1945. I suspect that the explanation for this lies partly in the illegitimacy of the analogy with physics.

The world of the physicist, even with the current rash of new particles and particle-like objects, consists of not more than one hundred different entities, each one of which is characterizable by a set of state measurements of not more than 20 or 30 variables. While the possible modes of interaction of this many components may well overtax even a large computer, it is nevertheless the case that a mathematical model is in one sense feasible, even if it may not be a numerical model.

The relation between physical phenomena and the mathematics of physical theory involves the generation by experimental observational data of a kind of mathematics, or of logic, such that one-to-one correspondence can be established between the admissible statements of the mathematics and conceivable observations in nature.

The complexity and richness of this type of theory is intimately related to the relation between the number of predictions that arise, the number of manipulative operations permitted, and the number of assumptions and definitions that were required to construct the theory. In general, "better" theories are those in which a small planting of assumptions and definitions and rules yields a rich harvest of testable consequences (as Mark Twain almost said) [12]. A theoretical advance consists in demonstrating that one or more of the rules or definitions that had been used to predict a given class of phenomena are in some sense redundant. Occasionally, a theoretician is willing to abandon the attempt to predict a class of phenomena, restricting himself to a subclass, if, in return for this restriction, he can discard a fairly large share of his rules and assumptions.

A "bad" theory, in the eyes of a physicist, on the other hand, has a relatively very large number of rules and assumptions for the amount of predictive power. This criterion of the quality of a theory is essentially an aesthetic one—which has been adopted by mathematicians as almost their sole criterion of quality [13]. Ecologists who collaborate with mathematicians are familiar with the frustrating event of having the mathematicians produce elegant looking theories for slightly simpler problems than the ecologists had in mind.

There do exist respectable sciences that have essentially no theory at all in the physicists' sense. Mineralogy and, to an even greater extent, taxonomy are primarily concerned with framing assumptions—for in essence a definition is an assumption, and the naming of a species is a definition. Nevertheless, the distinction is made between good taxonomy and bad taxonomy and perhaps more significantly between good taxonomists and bad taxonomists. That is, the normal criteria of scientific quality, which we use as biologists, are not the same as those of the physicist and mathematician.

Perhaps the key to understanding the incompleteness of the more mathematical aspects of quantitative ecology is to assume that the

differences in the criteria of quality that do, in fact, exist between mathematics and physics on one hand and biology on the other are not merely a matter of the comparative tastes and personalities of biologists as opposed to those of physicists but are in some sense intrinsic. (Note the diminishing self-confidence of the biophysical converts to biology.) It is almost as if a theory can only contain a certain amount of complexity. If it is composed of simple elements it can generate complex theorems and if it is composed of intrinsically complex elements it can only generate relatively simple theorems, in the sense of being very closely related to the definitions and assumptions [14].

Is there any reason why biological systems might choose to be of the second sort rather than the first sort—why—specifically, they might "choose" to use complex special mechanisms to solve certain problems? This is a rather peculiar problem and must be examined carefully.

The physical world involves a small number of particles and their very rich family of interactions, and this fact is the key to the relative success of the physicists' standard of elegance as actually applied. The biological world incorporates these particles and their interactions in large packages and each package seems to have a relatively small number of rather specific things that it can do, which differ to a large degree from those that any other species can do.

Not all of the responses of a biological system are specific—that is, to certain classes of general environmental changes, populations respond by altering the rates of rather generalized processes, reproduce faster, change their life expectancy distributions or feeding rate, etc. It is with this class of phenomena that quantitative ecology, in its narrow sense of a quasi-physical theory, has its greatest success. For example, Hairston and his co-workers [15] have been able to demonstrate that rather slight environmental changes, producing relatively slight changes in the life expectancy of an intermediate host, can effectively produce a major reduction in schistosome populations, while direct killing of the intermediate host would not work in the absence of habitat alteration. This kind of result only arises from the theory of quantitative ecology and has in fact already shown itself of major practical and intellectual significance.

But, as the theory is developed for any population, a point is reached at which the organism plays what might best be described as a trick—pulls out a new tool of a very specific kind which has some very specific function relevant to the environment of that particular species. For example, it has recently been shown that a species of rotifer, *Brachionus calyciflorus*, when in the presence of predatory rotifers, either *Asplanchna sieboldi*, *A. girodi*, *A. brightwelli*, or *A. predenta*, as a response to a specific chemical secreted by the predator, develops enlarged spines which make them inedible! [16] No respectable mathematical theory can be expected

to forecast anything like that nor should it be expected to. It is, however, a legitimate question of ecological theory to ask: When will an organism or species evolve a trick and when will it not?

In fact a partial response to this question can be formulated in terms of an evolutionary argument, too long to recapitulate here [17]. The problem of the development of specific mechanisms to face specific problems seems to depend on the clarity with which the organism can recognize the problem, the degree to which the problem would alter biological rates if the specific mechanism were not available, and the frequency with which the problem arises—analogous to a degree to the problem of when is it worthwhile to own five or six screwdrivers, each with a specific function, rather than one with fair competence to handle a spectrum of jobs.

The incompleteness of ecology seems to be based on empirical facts, and must be explained in empirical terms. A possible general explanation is suggested by Von Neumann's discussion of the relation between brain-like digital computers and actual brains [18]. He pointed out that the number of digits carried in a number in many kinds of digital computers is well in excess of the number of significant figures in most empirical measurements. This is necessary since most computer programs involve a sequential series of steps which quickly accumulate rounding errors. He then considered the neuron as a possible element of a computer-like system and concluded that the precision of neuronal response is so low that, if neuronal systems functioned in the same fashion as these computer elements, the precision of the final response would be excessively low. He concluded that there exists some other type of theory which would properly describe events in a brain and that this type of theory is lacking in arithmetical and logical "depth." That is, it is lacking in long chains of operations or arguments. The degree to which this concept can be related to ecological events is not completely self-evident. It seems clear, however, that the response of organisms to environmental change is of the same order of precision as that of individual neurons, certainly not higher. It is also clear that accumulation of variance with time in any property of a population generally does not occur and would be disastrous if it did occur [19].

I have suggested elsewhere [17] the possibility that the process of evolutionary adaptation consists of a hierarchical system of relatively simple feedback units in which many elements in the system are assumed to have essentially direct access to the environment, thereby minimizing the length of sequential processes. This scheme may in fact be incorrect but it does demonstrate the possibility of making formulations explicitly for biological problems, without attempting to force the biology into a pre-existing mathematical mold.

This does not deny that, on occasion, insight derived from mathe-

matics may prove extremely useful, nor does it deny the likelihood that biological insights may provide raw material for new developments in mathematics. I am strongly convinced, however, that, to insist on the theory of biology conforming to aesthetic standards derived from extant mathematics is illegitimate and is, in fact, an imposition of metaphysical criteria on the empirical world. Empirical sciences must develop their own standards of quality and cannot take refuge from the necessity of thought in the shadow of Newton or Euclid.

The above statement, whose partial validity cannot be seriously questioned, has certain very important implications. Specifically, it implies that scientific research quality can not legitimately be evaluated by its degree of conformity to any preconception of its mathematical form.

I can now return to the initial question: Why do certain areas of research tend to develop more rapidly than others? Several components enter into the growth rate of a science, each to an uncertain degree. One of the most obvious and legitimate is the need of various social agencies for certain clearly visualizable information which can only be produced as the result of scientific research.

Several enlightened governments, during and since the Second World War, have apparently come to the conclusion that the specific demands that a government may want to make of its scientific establishment cannot generally be foreseen in detail. It can be predicted that, from time to time, questions will arise which will require specific scientific answers. It is therefore generally valuable to treat the scientific establishment as a resource or machine which must be kept in functional order. This seems to be most effectively done by supporting a certain amount of research which is, frankly, not related to immediate goals, but might be of consequence in the future. Either the questions asked may later prove important or the kind of intellectual and physical activity involved in the research might be applicable to other research projects which, in turn, might have practical significance.

This kind of support, like all government support, requires decisions as to the appropriate recipients of funds. As indicated above, decisions based on utility as opposed to lack of utility are straightforward. Within the class of low immediate utility projects the decision process changes its character and becomes more difficult. The goal of the supporting agencies and their referees is the laudable one of supporting "good" as opposed to "bad" science; "important" as opposed to "unimportant" science.

The idea of good science tends to become confounded with either the capacity of the field in question to generate an elegant theory in the sense that Newtonian physics or Mendelian genetics generates an elegant theory or the hope that it will do so in the near future, in the sense that modern nuclear physics may generate an elegant theory in the

near future. It seems likely, however, that the concept of elegance, narrowly defined, is irrelevant to certain areas of the empirical world. Obsession with elegance will tend to concentrate effort on only limited portions of reality. There are indications that this is now happening.

A disenchanted friend of mine described modern experimental physics as "a dance performed around a bubble chamber" and, to an equal degree, biology threatens to become a study in DNA engineering. Various individuals, schools, and agencies have been issuing proclamations defining the "important problems" of biology and the more fundamentalistic of these have actually eliminated those fields they consider unimportant from various departments, curricula, and even universities.

The differences in relative growth of scientific areas are, therefore, seen to have several components. Some of these components are completely legitimate results of social needs, or at least of what are assumed to be social needs. Others are a legitimate consequence of particular advances being to some extent self-accelerating. I have been calling attention here to a less legitimate process of differential growth in which preconceptions of the form scientific theory ought to take, by persons in authority, act to alter the growth pattern of different areas. This is a new problem which is probably not yet ineradicable; but it is a frightening and terribly twentieth century trend, perhaps ultimately even more dangerous than the strange meanings that have been recently given to words like freedom and peace.

The greatest intellectual contribution made by science to mankind is the idea that human senses, suitably extended by various stratagems and devices, are capable of providing valid information about the world. Furthermore, this information requires the construction of appropriate theoretical frameworks before predictions and insights can be gained. The theoretical framework cannot be imposed *a priori*. To say that all of biology should have an elegant mathematical framework is as much of an imposition of metaphysics on observation as it was to say that the only suitable astronomic theories must involve cycles and epicycles, or that genetic data must conform to the principles of Marxian dialectics.

Traditionally, science consisted of an "account of the present undertakings, studies and labours of the ingenious in many considerable parts of the world." This passage was published on the title page of each volume of the *Philosophical Transactions of the Royal Society of London* until 1775. It was then eliminated, as if, by then, science needed no definition. Perhaps it is significant that shortly before the Royal Society found it no longer necessary to define science it did seem vital to protect the public from persons who would use science for self-aggrandizement. Starting in 1761, an "Advertisement" was printed in each volume of the *Transactions*, in effect warning the public against considering publica-

tion of an idea in a scientific journal as providing the imprimatur of the scientific establishment for the validity of the idea. This Advertisement appeared for the last time in 1936.

Long before 1936, science had become a reasonably lucrative profession. To pretend that science is a kind of genteel intellectual game had not been possible for many years. Perhaps this is why the Advertisement was dropped from the *Transactions*, but I do not actually know.

The fact that science has become an adjunct of national policy does not, in itself, necessarily eliminate the possibility of new and radical concepts arising from science. This possibility will be eliminated, however, as soon as the central authorities of scientific administration no longer feel embarrassed in closing whole areas of research as "unimportant."

Any system of intellectual constructs which is espoused by the agents of secular power seems to generate its own metaphysical orthodoxy. Science fought its way free of government-sponsored religion only to threaten now to become the new established church.

I hope that I have demonstrated at least in part, that the world is so made that elegant systems (in one traditional sense) are in principle unable to deal with some of its more fascinating and delightful aspects. New forms of thought as well as new subjects for thought must arise in the future as they have in the past, giving rise to new standards of elegance [20]. To say that a region of the world is but poorly described by existing kinds of theory is an affirmation of the possibility of intellectual advance, not a denial.

As for the future of ecology? I can only deny the usefulness of all shibboleths. Our future lies in full utilization of our intellect. Just as astronomers will not accept a map of the cosmos which places the sun at either the center or the edge, we must act as if we are neither at the beginning nor the end of our scientific progress. All of this may have been obvious in the seventeenth century but, in the twentieth, it may be worth restating.

NOTES AND REFERENCES

1. This is adapted from the first Phillip Clark Memorial Lecture, delivered at Michigan State University, East Lansing, Michigan, February 17, 1965, in memory of Phillip Clark, Professor of Zoology at Michigan State University, who was killed by a car on December 24, 1964, at the age of 45. Professor Clark was primarily known as a biometrician and quantitative ecologist.

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12. S. L. CLEMENS, *Life on the Mississippi*, Chapter 17, p. 152 in the Hillcrest edition, Harper and Brothers, New York, 1904.
13. The use of mathematical elegance *per se* as a major criterion for a physical theory and the consequent problem of determining when a statement produced by a physicist should be called a theory has been recently discussed by J. DYSON in his article entitled "Mathematics in the Physical Sciences" in the September 1964 *Scientific American*, *211* (3): 128–146.
14. The very general idea, that empirically equivalent theories can be constructed either by combining complex assumptions and simple derivations or by a complementary shifting of the complexity from the assumptions and definitions to the derivations, is being more profoundly developed by Mr. YEHOSHAPHAT G'IVEON of the University of Michigan as part of his doctoral dissertation in Communications Science.
15. N. G. HAIRSTON and B. C. SANTOS, 1961, Ecological control of the snail host of *Schistosoma japonicum* in the Phillipines, *Bul. Wd. Health Org.*, *25*, 603–610 and the references therein.
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AMERICAN SCIENTIST, *53*, 1965

THE ORGANIC CHEMISTRY OF SEA WATER

By PETER J. WANGERSKY

To many of us, the concept of the sea as a source of wealth, unlimited but difficult to obtain, is a natural and familiar one. As a youth in a New England manufacturing and seaport town, I was well aware of the hazards and rewards of the Grand Banks longline fishery, even though that fishery had long been mechanized. Only the Portuguese fishermen still made the trip to the distant fishing grounds in sailing vessels and tended their lines from dories, and only the small Italian grocery stores still carried dried salted cod as a staple. A trip to the lobster pound, decorated with the exoskeletons of what must surely have been the ancestral, almost legendary, lobsters, was an expedition for a summer Sunday, and almost every school year included a day at a museum devoted to relics of the now-extinct whale fishery. We children stared at the great skeletons suspended from the ceiling and wondered, a little disbelieving, that any creature so large could survive on a diet of the tiny krill. By contrast, the story of Jonah seemed almost more convincing.

As an adult, taking my living from the sea the easy way, as an oceanographer, the question of marine productivity became a matter of professional as well as personal interest. However, productivity was no longer defined in terms of bushels of clams, pounds of shrimp, or number of shore dinners, but as grams of organic carbon fixed by photosynthesis in a given time period—a more basic but somehow less romantic unit.

It would seem to the casual observer that the measurement of this primary productivity should be fairly simple. All that needs to be done is to isolate a sample of water, measure its organic carbon content, let the phytoplankton grow for a given period, and then measure the increase in organic carbon. A straightforward procedure, with almost no chance for error other than the usual analytical errors, it must surely have been done on almost every body of water in the world by now. However, sea water is a medium of a complexity sufficient to dismay any right-thinking analytical chemist, and it is only recently that methods of carbon analysis simple enough for large-scale sampling programs have been devised. With the advent of the gas chromatograph and the nondispersive infrared CO_2 analyzer, both instruments capable of shipboard use on the newer oceanographic vessels, we are beginning to get a clearer picture of the distribution of organic carbon in the oceans. There are also some nonanalytical problems inherent in the definition of productivity which complicate the interpretation of what results we do have.

It has long been evident that the amount of carbon tied up in dissolved organic matter is many times greater than the amount incorporated into marine organisms. Early analyses generally established this ratio as 300:1; in more recent work the ratio has been closer to 100:1. While it is possible to determine the carbon content of plankton caught by net tows with some accuracy, the smaller plankton, which must be caught by filtration, cannot be separated from the nonliving particulate matter. The plankton carbon figure is therefore a minimum figure at best, usually including only those larger forms collected by net tows.

The dissolved organic carbon is usually determined by some form of wet oxidation, and is also a minimum figure, since not all organic compounds can be completely oxidized to CO_2 by wet methods. A great deal of effort and ingenuity have gone into devising more accurate and sensitive methods for measuring the CO_2 evolved by the oxidation; this line of research has reached the point of diminishing returns. Future effort should be concentrated on the handling of the sample before analysis and on the oxidation process itself. The older methods were troubled by interference from the chlorine freed by the acidic oxidation reagents, and sometimes avoided this interference by precipitation of the chlorides. Such precipitation may also remove organic compounds by adsorption on the inorganic particles. The agreement between the various methods of combustion, wet and dry, has not been striking; the dry methods typically give carbon values 2–10 times higher than the wet methods. These analytical differences must be resolved, at least to the point where we know we are all measuring the same things.

Within these admittedly rather severe methodological limitations, the distribution of organic carbon in the oceans is fairly well agreed upon. The dissolved fraction, as determined by wet combustion methods, normally ranges between 0.5–1.2 mg C/liter, with the higher values found in the surface waters. Below the euphotic zone the concentrations vary irregularly, with at least a suggestion, in data taken in the North Atlantic by Duursma [1], of a correlation between carbon and oxygen distributions. Measurements of particulate organic carbon, including planktonic organisms, are generally about 10% of the dissolved organic carbon values, or 100–150 μg C/liter in surface waters and perhaps 30–50 μg C/liter in deeper waters. There appears to be no marked decrease with depth after the first hundred meters.

The impossibility of separating small planktonic organisms from large pieces of detritus permits us to make only rough estimates of the detritus:living organisms ratio. Parsons and Strickland [2], on the basis of total particulate organic carbon and chlorophyll analyses, reported a ratio of 500:1. Riley, *et al.* [3], considered the phytoplankton carbon to account for something less than 10% of the particulate organic car-

bon, with the phytoplankton carbon estimated from measurements of cell volume.

We have, then, over much of the ocean, a soluble organic carbon content of around 500 μg/liter, and a total particulate organic carbon content of perhaps 50 μg/liter, all ultimately derived from a surface population of phytoplankton of some 5 μg C/liter. This is a weighty inverted pyramid for the phytoplankton population to support. If we consider the detritus to be entirely the remnants of dead phytoplankton, the constancy of its concentration with depth suggests that these materials are not used to any extent by the zooplankton and deep-water fish, and are probably composed of compounds resistant to further metabolic processes. In the same manner, the still greater concentrations of dissolved organic materials led early workers in this field to assume that the bulk of the dissolved materials must be extremely resistant to decomposition, with perhaps a lignin-like structure. In general, it was considered that the organic material in true solution took little part in the metabolic cycle in the ocean, and was probably slowly oxidized to CO_2 by chemical reaction with the dissolved oxygen of the deep water.

Pütter and Krogh: Dissolved Organic Carbon

Certainly not all biologists believed in the inertness of the dissolved fraction. In 1909, Pütter [4] advanced the hypothesis that some marine organisms might obtain much of their food by direct absorption of dissolved organic carbon. Unfortunately, he had no way of identifying the compounds present in sea water, and his method of analysis for organic carbon gave rather high values.

In a series of papers published in the early 1930's, Krogh and his coworkers [5] developed a better micro-method for dissolved organic carbon in sea water and examined the whole question of production and utilization of dissolved organic compounds. Krogh established a ratio of dissolved: organism carbon of 300:1, with lower ratios in the surface waters. He concluded that it was entirely unlikely that phytoplankton organisms should diffuse dissolved organic materials out into their surrounding medium during normal growth, and that the production of such materials in culture medium was brought about by autolysis of dying organisms. He further concluded that, although some marine animals could be shown to absorb simple organic compounds from their medium, it was unlikely that such an effect would be important in the oceans, both because of the low concentrations of organic compounds in the sea and because of the "undigestibility" of the compounds present.

The logic and finality of his conclusions were such that research along these lines almost stopped for about twenty years. The major unsolved problem seemed to be the nature of the organic compounds present in sea water, and the solution of this problem depended, at least in part, on

the development of techniques and instrumentation for micro- and submicro-analysis and upon the availability of bacteria-free pure cultures of phytoplankton organisms.

The possible validity of Pütter's hypothesis, at least for shallow-water bottom organisms, was suggested again in the early 1950's by A. W. Collier and his co-workers [6], who demonstrated that the oyster (*Crassostrea virginica Gmelin*) responded to the presence of some carbohydrate, or some compound associated with a carbohydrate, by beginning to feed when a threshold value of the carbohydrate was reached. Analyses of water before and after passage through the oyster showed that a considerable part of the carbohydrate was removed by the oyster. While the amount removed varied considerably with different oysters, as much as 50 mg/hour could be removed by a single oyster. To the best of my knowledge, this has been the only demonstration of direct absorption of dissolved organic matter by a marine animal under natural conditions, although Stephens [7] has shown that such absorption is possible with almost any soft-bodied marine invertebrate, and Johnson, *et al.* [8], have found that labeled amino acids can be taken up from solution by the conch and used in the formation of both shell carbonate and organic shell matrix.

With the second of Krogh's conclusions, that it is unlikely that planktonic organisms would diffuse out organic materials during normal growth, I can only agree. It *is* unlikely, but it seems to be quite common. Almost every phytoplankton organism studied has been shown to diffuse small organic molecules into its medium, and almost any organic molecule of biological interest has been shown to be given off by some organism.

No really systematic work on this excretion of dissolved organic material by pure cultures of phytoplankton organisms has yet been published, and the field remains in a state of chaos. The information available is scattered through a wide variety of sources. A need exists for a critical review of the field, with the information on production of specific compounds classified along taxonomic lines. Some sort of order should arise when enough of this information is gathered together. Vallentyne [9], in 1957, reviewed the field of naturally occurring organic materials in solution in great detail, but from the viewpoint of enumeration of the kinds of compounds found in nature rather than their excretion by specific organisms. While his paper might well serve as a starting point for a newer review, the field has expanded considerably in the past eight years.

The evolutionary aspects of this seemingly wasteful excretion are most puzzling. A number of possible explanations have been forwarded, ranging from the removal of toxic trace metals by chelation to the preparation of a store of organic material for later heterotrophic growth, in

the absence of light. Perhaps one of the most interesting ideas, proposed by Collier, is that the production of extracellular organic material is part of a symbiotic relationship. Many of the marine phytoplankton show a requirement for vitamins when grown in pure culture, B_{12} being most commonly required. These vitamins, seldom in short supply in the sea, are produced by marine bacteria. Collier postulates that phytoplankton produce the organic medium and the surfaces upon which the bacteria flourish, and the bacteria in turn furnish the phytoplankton with necessary vitamins and growth supplements.

While it is possible to find examples of each of the mechanisms proposed, none of the explanations seems altogether convincing, since the variety of compounds excreted is so great. Many of the compounds found are not chelators, and the completely autotrophic diatoms produce as great a variety of compounds as do the heterotrophic, B_{12}-requiring dinoflagellates. It would seem more profitable, also, to store excess carbon internally, rather than to throw it away and hope to find it again after dark. I would suggest that this excretion may be primarily a matter of cost accounting; that the construction of a system able to absorb inorganic ions from solution and at the same time retain small organic molecules might require more energy than is lost by the diffusion of organic compounds produced in excess of immediate needs; and that the many mechanisms and relationships involving the organic compounds are secondary phenomena, arising from the availability of these compounds.

The habit is not unique with marine phytoplankton. Many plant cells, such as the cambium cells of the sycamore, exhibit the same behavior in tissue culture. The evolutionary usefulness of this procedure in organisms displaying division of labor between cells is perhaps somewhat more obvious. The beginnings of the process, in the single-celled plants, seem obscure, although it has been suggested that we might consider the oceanic phytoplankton as one very large, very sloppily organized tree.

There is still some question as to the composition of the organic compounds present in the deeper water. It may be that the simpler compounds released by phytoplankton in the surface waters are quickly used by other organisms, so that only the more resistant compounds get to the deeper water. The correlation between dissolved oxygen and organic carbon suggested by Duursma's data implies that the compounds in even the deepest water can be oxidized to some extent, although the mechanism of utilization, biological or chemical, is not specified. The question of composition must be settled by isolation and identification of specific compounds, not the easiest of tasks at the submicrogram level.

Particulate Matter: Origin and Fate

The origin and fate of the particulate matter in the sea did not at first

seem so complicated. We have evidence from numberless phytoplankton culture experiments that dead cells disintegrate into a kind of featureless gunk, not too different from that which we find in suspension in the sea. Since both benthic organisms and zooplankton are adapted for gathering such amorphous particles, and since bacteria in the sea are found principally on such particles, the eventual utilization of their organic content and remineralization of any inorganic components, such as nitrate or phosphate, were assumed. At least this part of the carbon cycle in the sea seemed fairly straightforward, although the constancy of particulate carbon content with depth was annoyingly difficult to explain.

In 1961, Baylor, Sutcliffe, and Hirschfeld [10] found that inorganic phosphate could be removed from sea water by aeration. The process did not remove inorganic phosphate from artificial sea water, and the authors postulated that the removal was due to the collection of phosphate on organic matter concentrated on the surfaces of air bubbles. In a later paper, Sutcliffe, Baylor, and Menzel [11] demonstrated the formation of aggregates of organic material containing adsorbed phosphate by aeration of filtered sea water. The aggregates thus formed were of a size usable as food by zooplankton. The authors suggested that such aggregates would be accumulated in windrows by light winds, and that such windrows could be found by looking for local increases in turbidity. Such collections were found both in the Sargasso Sea and in the ocean off Woods Hole. The nutritional value of these particles was demonstrated in a later series of experiments, where the authors compared the growth of cultures of the brine shrimp, *Artemia salina*, fed only on such particles, with that of cultures fed on dried yeast. While the growth rate of the animals fed on aggregates was slower than that of the yeast-fed animals, the difference was small and may have been due simply to the smaller total food supply furnished by the aggregates. Equivalent rates of growth might have been achieved by increasing the aggregate concentration, since the water in the culture vessels was always swept clean of aggregates by the feeding animals before the next feeding. Control cultures, fed nothing at all, quickly died out.

A chain of possibilities had thus been established by this series of experiments. A portion of the organic material dissolved in sea water could be removed from solution by adsorption on the surfaces of bubbles, and subsequent incorporation into organic aggregates. These aggregates would then be capable of serving as a food source for zooplankton and benthic organisms, as well as of furnishing excellent surfaces and substrates for bacterial growth. This mechanism would permit utilization of at least part of the vast supply of organic carbon present in solution in the ocean. The next step, converting the possibilities into probabilities, required estimation of the quantities of carbon involved in these reactions in nature.

G. A. Riley had been interested for some time in the role of particulate matter in the annual cycle of productivity in the ocean. Working in their own duckpond, Long Island Sound, a group of biological and chemical oceanographers at the Bingham Oceanographic Laboratory had measured the yearly cycles of plankton and particulate abundance and of phosphate, nitrogen, and dissolved carbohydrate fluctuation. The peak of the cycle of particulate abundance coincided with the spring flowering, but a considerable concentration of particulate matter was evident throughout the late autumn and winter, when phytoplankton populations were low. If the particulate matter were solely the products of degradation of phytoplankton remains, the half-life of the particulate matter through the winter months would have to be fairly long in order to maintain such high concentrations. Also, the peak of the aggregate population was disproportionately larger than the peak of the co-occurring phytoplankton flowering. The seasonal cycle of aggregate concentration agreed only in part with that of the phytoplankton.

The work on the formation of aggregates by bubbling suggested that the high particulate level during late autumn and winter might be supported by the conversion of dissolved organic carbon to aggregates by this mechanism. Samples of Long Island Sound water were accordingly filtered and bubbled, and the resulting aggregates examined. The aggregates formed in this manner looked remarkably like the unbacterized aggregates found in the Sound during the spring bloom. After the material had been sitting around a while, and had been colonized by bacteria, the resemblence to naturally occurring aggregates was even stronger.

A few words about the appearance of the aggregates might be in order at this point. Aggregates newly formed from filtered sea water commonly look like bits of cellophane. They are transparent, almost colorless, thin and plate-like, with definite, sharp edges. As bacteria begin to colonize the particles, the sharp edges become fuzzy and dark spots appear on the hitherto transparent aggregate. However, at no time do the particles approach the flocculent fuzziness of the materials found in dying plankton cultures.

Since much of the dissolved organic carbon in sea water may be a result of phytoplankton metabolism, the media in which pure cultures of these organisms had been grown should also furnish aggregates when bubbled. Therefore, culture medium taken from pure cultures of the locally important phytoplankton species was filtered and bubbled. Organic aggregates were formed in every case, with diatom cultures furnishing somewhat greater quantities than dinoflagellate cultures. The amounts of organic carbon in the aggregates ranged from 2–75% of the amounts found in the organisms.

Long Island Sound must be considered to be a special situation in many respects; it is relatively shallow, relatively well-mixed, and rel-

atively rich in nutrients, the result of run-off from the surrounding land masses. What with the drainage from New York and the Connecticut manufacturing areas, it is sometimes difficult to convince oceanographers that extrapolations from Long Island Sound to the open ocean are altogether valid. Therefore, information on the distribution of particulate organic carbon in blue water was needed.

At this time I was presented with an opportunity to make a cruise to the west coast of Africa on the *RV Trident*, research vessel of the Narragansett Marine Laboratory. I used this opportunity to take an ex-

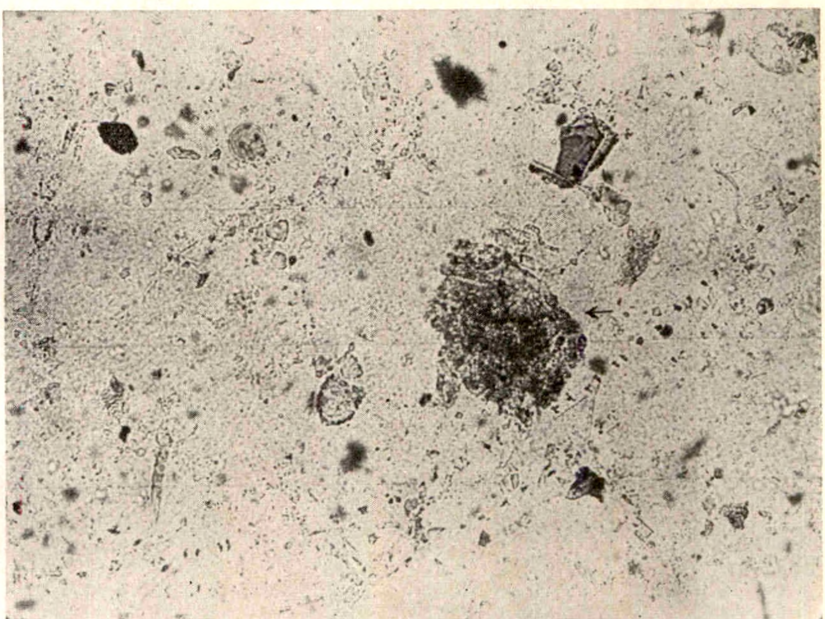

FIG. 1. Bacterized organic aggregate from Long Island Sound. 40×

tended series of surface and deep samples, from Bermuda to Africa and back, collecting particulate organic carbon for chemical and microscopic examination. The distribution of particulate carbon in the surface waters closely paralleled the distribution of phytoplankton, although less than 10% of the particulate organic carbon was present in the form of living organisms.

The constancy of the particulate carbon content with depth was particularly interesting, in view of the speed with which bacteria could find the particles on a microscope slide or in a culture flask. In order for this constancy to be maintained, some kind of adsorption onto particles must be taking place at depth to offset losses in carbon due to utilization by bacteria or zooplankton. Experiments were set up by Dr. Riley to test this hypothesis; sea water taken at depth was filtered and then

seeded with aggregates formed by bubbling filtered surface water. The sample bottles were inverted from time to time to keep the particles in suspension. The yields of particulate organic carbon from this process were almost the same as the yields obtained by bubbling water from immediately adjacent levels in the water column.

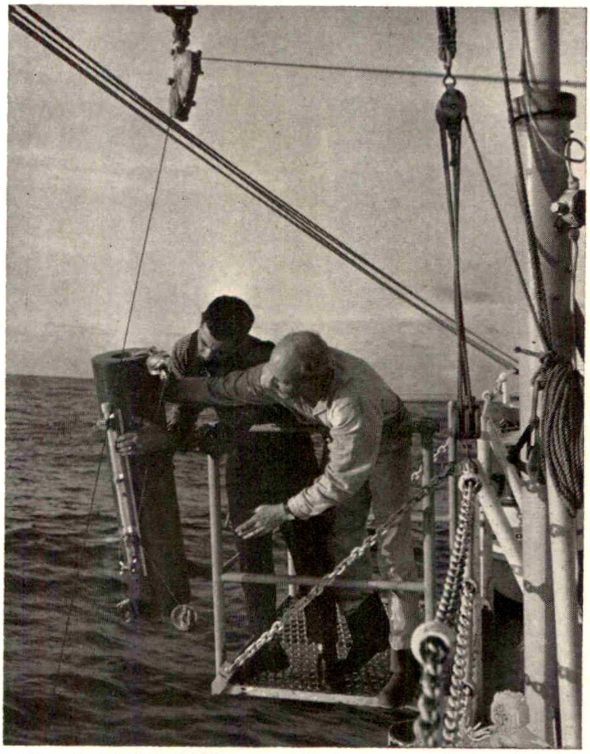

FIG. 2. Niskin 30-liter sampler being attached to the hydrographic wire by Dr. G. A. Riley and Thomas D'Ambro. Photograph courtesy of Dr. Theodore Napora.

This might seem to be reasonably clear-cut evidence for adsorption of organic matter onto pre-existing particles in the absence of bubbles, and perhaps the experiments should have stopped at this point. Unfortunately for the peace of mind of the experimenters, controls of filtered unseeded sea water showed roughly equivalent gains in particulate organic carbon on standing. It is possible that the process of filtration breaks some of the particles into extremely fine pieces, small enough to pass through the filter, and that these pieces serve as nuclei for further adsorption and agglomeration. This increase in particulate organic carbon in the unbubbled samples not only complicates the study of the adsorption process, it also makes the running of controls

very difficult for all experiments involving inclusion of materials, organic or inorganic, in the aggregates. The question of the role of adsorption in the transition of organic carbon from dissolved to particulate form will probably only be decided when we are able to start with an artificial sea water with known amounts of pure organic compounds in true solution.

Another problem waiting upon the identification of the compounds involved is the question of the mechanism of formation of the aggregates. The organic compounds taking part in the aggregation are originally in solution. Once the aggregates are formed, they are extremely resistant to re-solution. Some sort of chemical change takes place during the collection of materials. All of the aggregates analyzed to date have contained proteins or peptides, and it may be that the reactions involved are simply denaturations of these proteins. However, the reactions on bubbles take place on or above the surface layer of the ocean, where there is a fairly high energy input in the form of ultraviolet light. We must not discount the possibility of other reactions, even some which seem thermodynamically unfavorable, taking place.

The processes of excretion of dissolved organic carbon and incorporation of this carbon into aggregates force us to reconsider carefully our definition of primary productivity. Should we consider the excreted organic compounds as part of the primary productivity, even though some of them may not be readily available for further use? If we do decide to include them, how do we measure them? The standard C^{14} productivity method measures only the carbon incorporated into particulate matter. If we decide to exclude the excreted materials, how do we separate the organic carbon in the aggregates from that in the organisms? Until now we have considered that incorporation of C^{14} from bicarbonate in solution into organic particles necessarily implied incorporation into an organism. However, since C^{14} picked up as an inorganic ion can be excreted quickly in the form of dissolved organic carbon, it may well be included in aggregates formed from the dissolved organic compounds. It would seem to me that our best alternative at the moment is to try to measure the net change in organic carbon, both dissolved and particulate, in a given time period, recognizing at the same time that we are missing a great deal of information on the details of production and utilization. If we can learn enough about the mechanisms of aggregate formation to enable us to suppress such formation entirely, we may be able to work with the separate sections of the carbon system.

Whether or not we accept aggregate formation as an important step in the carbon cycle in the sea, we are forced to accept certain parts of the hypothesis. The presence of large numbers of aggregates in the surface layers of the ocean, and their rapid depletion in the first hundred meters, points out both their origin and their food value. In water taken

from any depth it is possible to form new aggregates by filtering and bubbling, which suggests that the process does not proceed to total depletion of dissolved organic matter at the surface. The distribution of particulate carbon with depth is another case in point. In every deep station for which we have carbon data, the particulate organic carbon values below 500 meters are distributed in a random fashion with depth, and are usually essentially constant. If we do not accept the constant addition of organic material to the particulate phase, we must then hold that the particulate material in the deeper water is not used either by bacteria or by zooplankton during its passage through the water column. This postulate is difficult to accept, since it allows no way for the midwater organisms to make a living other than by eating each other, in the long run a self-defeating process. We must also hold that the same inedible material is quite delicious to benthic organisms and sedimentary bacteria, since there is no large accumulation of organic materials in the bottom sediments. It seems unlikely that material usable by bottom forms would be quite unusable by zooplankton and midwater bacteria.

The organic carbon cycle in the sea has thus become considerably more complicated in the last few years, but quantitatively somewhat easier to understand. The amounts of food available for bacteria, zooplankton, and benthic organisms seem much more reasonable if at least part of the reservoir of dissolved organic matter can be transformed into easily used particulate matter. Any adaptation which would facilitate the finding of these particles should be greatly favored; we should perhaps examine zooplankton organisms for mechanisms capable of sensing changes in turbidity or turbulence which might be associated with accumulations of particles.

Geochemical Effects

A problem associated with the formation of aggregates is their role in the geochemistry of minor and trace elements in the ocean. The aggregates are called organic only by courtesy and precedent, since their organic fraction seldom exceeds 30%. In Long Island Sound the remainder of the aggregate is composed of the kinds of inorganic debris one would expect to find in the Sound—clay particles, mineral fragments, old empty diatom frustules, bits of manila from frayed ropes, wood fibers, and so forth. If the aggregates were larger, one would almost expect to find rusty beer cans and lead sinkers from party fishing boats. In the open ocean, the inorganic fraction is a little harder to identify. We have found that both the aggregates formed by bubbling filtered sea water and those formed by bubbling synthetic sea water medium in which phytoplankton organisms have been grown contain about 2.5% calcium carbonate. In particulate matter taken from the open ocean the calcium

carbonate content is somewhat higher, perhaps as much as 10%. However, this includes any calcareous organisms present in the sample. Chave [12] has shown that naturally occurring organic aggregates can contain discrete crystals of calcium carbonate, usually in the form of calcite, and that the calcium carbonate present in sea water in this form is not in equilibrium with the sea water. The organic outer coating effectively protects it from re-solution, even in water grossly undersaturated with respect to calcium carbonate.

The contribution of calcium carbonate to the sediment by this mechanism is large enough to be important in geochemical terms. If a particulate organic carbon content of 35 μg/liter is estimated for the deep water, the carbonate attributable to this source would be about 1 μg/liter, a small amount indeed. However, Riley *et al.* [13] calculated a sinking rate for the aggregates of 2 meters/day, primarily on the grounds of biological oxygen consumption, resulting in a sedimentation rate of 0.7 mg cm^{-2} yr^{-1} for fine calcium carbonate included in the particles. This is an appreciable fraction of the measured rates of fine calcium carbonate accumulation, which range from 0.3–1.8 mg cm^{-2} yr^{-1}.

Manganese is another cation of great interest to geochemists. The distribution of manganese in the ocean has always been a puzzle, since the valence and oxidation states predicted by the pH and Eh of sea water are seldom found. Manganese (II) has been found in the calcite of foraminiferal tests and in the fine carbonate fraction of sediments in a distribution difficult to explain in terms of simple inorganic reactions. The manganese (II) concentration is very low in the surface carbonates, but increases linearly with depth in the sediment. Conversely, insoluble manganese is high in the surface samples and decreases with depth in sediment. A mechanism for putting insoluble manganese into solution after sedimentation, linear with depth or time, is needed. It seems likely that change in redox potential is not involved, since the distributions of other cations are not affected.

It occurred to us that a possible mechanism might involve the incorporation of Mn(II) into organic aggregates, with sedimentation to the sea floor taking place in this form. Utilization of the organic outer coating of the aggregates as food by bacteria and other benthic organisms would free the Mn(II) into solution in the interstitial water and in the water layer just above the sea floor. Collections of naturally occurring aggregates were analyzed for Mn(II), and amounts of the order of 0.03 μg/liter were found. This small quantity of Mn(II) produces a sedimentation rate of 2 μg cm^{-2} yr^{-1} of manganese, enough to account for most of the manganese present in carbonate sediments. Details of this research can be found in a paper by Wangersky and Gordon [14].

This mechanism is probably akin to the process of ion flotation described by Sebba [15]. He found that bubbling air through a solution

containing surface-active organic materials resulted in the concentration of certain ions in the particulate matter formed in the foam. The degree of concentration depended upon the charge and structure of the surface-active agent and the ionic radius and charge of the ion involved. It is at least theoretically possible to design an agent which will remove a specific ion from solution, leaving behind other ions with similar chemical properties but differing ionic radii. Sebba demonstrated the collection of aluminum, uranium, the alkaline earths, iron, nickel, and traces of other metals from sea water. He suggested this technique as a method of economic significance for the collection of aluminum and possibly magnesium.

A concentration of ions by this mechanism might also be responsible for some of the other anomalous distributions found in carbonate sediments. The biological calcite formers of importance in marine sediments, principally the foraminifera and coccolithophores, discriminate against strontium in shell formation, maintaining a fairly constant Sr/Ca ratio. The Sr/Ca ratio of the fine fraction of carbonate sediments is quite variable, however, and always higher than that found in the forams. If strontium were incorporated into the calcium carbonate of the aggregates strictly in proportion to its concentration in sea water, the presence of varying amounts of aggregates in the sediment could account for the peculiarities of the strontium distribution. Any slight selectivity for strontium over calcium would be still more helpful. Similarly, the features of magnesium and barium distribution in carbonate sediments, difficult to understand on strictly biological or strictly chemical grounds, could be explained readily by the incorporation of these cations into organic aggregates. Many of the trace elements in sea water are present in amounts far below those permitted by the solubility products of the compounds they should form in sea water. It may be that control of trace element concentration in the sea is a function of aggregate formation in the surface waters.

Model Systems and the Organic Chemistry of Distilled Water

The proportion of inorganic material in the organic aggregates was surprising at first, and experiments were devised to determine which ions could take part in aggregate formation. Our first experimental bubblings were made in synthetic sea water systems to which small amounts of protein had been added. As a control, we also bubbled synthetic sea water with no organic material added. To our dismay, the yields of particulate matter in the controls nearly equalled those in the experimental runs. We then bubbled solutions of each of the components of the synthetic sea water, and found that a small but measurable amount of particulate matter could be removed from the surface layer of the water in the bubbling chamber no matter which salt was

used. Apparently, the inorganic material was carried into the air above the water surface in the form of an aerosol, and fell back on the surface as a collection of fine crystals after evaporation of the water in the aerosol. The crystals then rested upon the surface layer until the turbulence associated with bubbling carried them below the surface and put them back into solution. The yield of particulate matter from these strictly inorganic systems was thus proportional to the area of relatively undisturbed surface. Since the particles carried an electrical charge, the degree to which they could be crowded together on the surface was limited.

All through this series of experiments our yields of inorganic materials had been small, and our organic carbon values indistinguishable from the blanks run on the dry reagents alone. In our second series of bubblings, we suddenly began to get yields of inorganic material three and four times greater than in our previous runs, together with appreciable yields of organic carbon. Since this second series was primarily a repetition of our earlier work, the discrepancies were most dismaying. The reason for the increased organic carbon, and thus for the higher inorganic yield, occurred to me while I was driving past Lake Whitney, one of the New Haven reservoirs, one evening. The ice had gone off the lake, and the spring phytoplankton bloom was in progress.

We immediately ran organic carbon analyses on our distilled water, and found much more carbon than we had been adding to our experimental runs. The carbon compounds in the water had survived triple distillation and distillation from alkaline permanganate. Apparently we will have to resort either to combustion in oxygen or to treatment with intense ultraviolet light to remove these impurities.

The presence of this amazingly stable organic material in our distilled water, and its correlation with algal growth in the city reservoir system, raises an interesting point. Many people who have grown phytoplankton organisms in synthetic medium have noticed that with some species there seems to be an annual rhythm of growth in the laboratory, even when the organisms have been kept under laboratory conditions on synthetic medium for years. The cultures do well in the early spring, die down through the summer, and sometimes begin to prosper again in early autumn. In short, they seem to be responding to the seasons in the same manner as their relatives in the wild. I have been assured several times that there must be an innate annual growth rhythm in these organisms, since obviously they received no seasonal clues from their environment. It would seem, however, that the organisms have been given some fairly strong seasonal clues from the presence of organic materials in the distilled water used to make up their media. I would suggest that the organic carbon content of the distilled water is one more variable which must be considered and controlled by anyone

growing organisms in aqueous medium, whether the organisms be bacteria, phytoplankton, or still larger organisms. There may also be a connection between the level of these compounds in the distilled water supply and the sudden onset of sexuality which sometimes overtakes colonies of Hydra grown in synthetic media.

Bubbles and the Origin of Life

The ability of the bubbling process to scavenge organic compounds from dilute solution and to collect the aggregates thus formed on the surface layer of the sea may have been of major importance in the origin of life. The creation of simple organic compounds out of inorganic starting materials by the input of almost any kind of energy into a "primeval" reducing atmosphere has been demonstrated by many scientists. A review of this work can be found in the recent symposium volume edited by Fox [16]. Some experiments have shown that even a reducing atmosphere is not really necessary, and that at least the smaller polymers, the simple peptides, can be produced from dilute solutions if dicyanamide is present in solution. The difficult step has been the production of larger molecules, the ordered organic polymers. The mechanisms which have been successful so far have involved high concentrations, in some cases dry mixtures, of the simple starting compounds, and relatively large energy inputs. While many of the reactions leading to the formation of biologically significant compounds are catalyzed by ultraviolet light, the penetration of these low wave lengths into water is decidedly limited. Some mechanism is needed to concentrate the simpler compounds and to hold them within the range of an energy source, the most probable and ubiquitous being ultraviolet light.

Bernal [17] has suggested that such collection took place on the surfaces of clays, and that the ordered clay structure aided in the ordering of the polymer structures. This method of concentration limits the origin of life to inshore areas, where the clay particles might be cast on shore after collecting organic materials from the bay or estuary. The volume of water swept for organic materials would be rather small, compared to the oceans, and the exposure time of the clay particles to sunlight rather short, perhaps of the order of one or a few tidal cycles. Furthermore, once the clay particles carrying the adsorbed organic matter had been buried even a few centimeters in the sediment on the beaches, the organic material would have been relatively inaccessible to further reactions.

I would suggest instead that the surfaces of inorganic particles formed by the action of air bubbles in sea water would serve as more suitable sites for the formation of the larger organic polymers. The simple organic materials would be concentrated in a relatively small area, with a low water content, and held above the sea surface, subject to the full in-

tensity of the sunlight. If the particles were carried below the surface by turbulence and the organic compounds put back into solution, the compounds would be in an environment favorable to further chemical reaction. Fortner, *et al.* [18], have shown that the irradiation of solutions of halides with ultraviolet light gives rise to halogen atoms and solvated electrons. The chlorine atoms thus formed are able to dehydrogenate ethanol and methanol. It is to be expected that, in the upper centimeter of the ocean, this kind of reaction is taking place, with resultant changes in the composition of the organic compounds in solution.

It has already been shown by Sutcliffe, *et al.* [11], that the bubbling process can concentrate both organic and inorganic phosphates on the aggregates. The conditions necessary for the formation of biologically active macromolecules—concentration of smaller molecules, large energy input, presence of high-energy phosphate compounds, continued recollection of the organic products, and perhaps even the ordered surface of the inorganic crystal—seem to be met in the environment of the organic aggregate on the surface layer of the sea. Perhaps the next cycle of "origin of life" experiments should involve bubbling chambers and artificial sea water, as well as primitive atmospheres.

Acknowledgments

Much of the subject matter of this paper was developed in conversations with Drs. K. E. Chave, D. J. Nelson, B. C. Patten, G. A. Riley, and W. H. Sutcliffe, Jr. Responsibility for the wilder extrapolations, however, rests entirely with me. My own research mentioned in this paper was carried out at the Bingham Oceanographic Laboratory of Yale University, and was financed by Contract AT(30-1)2882 with the Atomic Energy Commission.

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AMERICAN SCIENTIST, 53, 1965

SYSTEMS RELIABILITY AND ENGINEERING STATISTICAL ASPECTS

By G. J. LEVENBACH

AROUND the year 1800, the French mathematician Pierre Simon, Marquis de Laplace, once calculated the probability that the sun would rise the next day, utilizing the observation that it had done so each morning for the last 1,826,230 days since creation [1]. Using similar arguments, we can ascertain with very high probability that many centuries from now an archaeologist will become interested in *reliability* publications from the twentieth century. His digging up the past will result in a huge collection of proceedings, transactions, and programs of annual, national, regional, and local meetings, seminars, colloquia, training sessions, and panel discussions about reliability. On the basis of these findings he might advance an alternative to the prevailing hypothesis that twentieth century civilization disappeared in an atomic holocaust and suggest that suffocation by mountains of paper is more likely. He might be led to the assumption that an extremely large number of people were concerned with reliability and conclude that the world must have been a very unreliable place to warrant such an effort. When our archaeologist finally gets to deciphering and compiling the information, he would find not only that *redundancy* was a very popular subject in reliability dissertations, this one included, but also that most of the information itself was highly redundant, and, again, this story is no exception.

Despite all these considerations, however, this paper has been written to try and convey some of the flavor of reliability engineering to the nonstatistical reader of this magazine.

It is safe to say that the tools developed by mathematical statistics are having an ever increasing impact on science and technology. Statistical models can help very much in understanding and analyzing complex phenomena. Discovery of hidden and unknown causes can be greatly aided by a statistical analysis of the collected data. The flood of data resulting from automatic measuring and recording devices can be reduced to manageable dimensions by suitable statistical processes. On the other hand, if the model is substituted for the real world, we are in danger. As the Scottish poet Andrew Lang remarked a century ago: "They use statistics as a drunkard uses lamp posts, for support rather than illumination." Furthermore, by uncritical use of the electronic computer, it is not too difficult to end up with more "reduced" data than one had original observations. If these new numbers do not add insight into the experimental situations, the effort is wasted. After

all, so far, more discoveries have been made through the intuition of the skilled scientist, and more technological improvements have been created through the skill of the engineers, than by statisticians pushing models and numbers around.

Let me repeat: Statistics provides a powerful tool [2], and a brief look at some "reliability history" might help us to evaluate the impact of statistics on reliability.

Incidentally, the word "reliable," according to the Oxford English dictionary, came into current use as recently as 1850 in this country. This distressed the British guardians of the language, witness numerous letters in the learned journals of the day. A book was even published entitled *On English Adjectives Ending in -Able, with Special Reference to Reliable* [3]. The distress was caused not so much by our being first to use it, but by our using the word reliable at all: it was irregular to use the suffix "-able."

Why has the interest in reliability increased so much in recent years? People have always been interested in the reliability of their cars, their appliances, airplanes, telephones, the electric power systems, etc. In the early 1950's, the Armed Forces became aware that the maintenance problems on their equipment had gotten out of hand. The pinch was felt both in costs and in skilled manpower. Numbers are quoted about a system that required ten times its first cost for maintenance over the life of the system. An important additional factor is that the equipment was often not available when called upon and missions were jeopardized. The public utilities had learned to cope with this over the years. If they had not, maintenance expenses or lack of customer enthusiasm would have put them out of business long ago.

One of the basic causes of unreliability became readily apparent when a few simple statistical models, applicable to electronic equipment, were developed. These showed that the increased number of component parts required to make up the systems that had to meet the largely increased performance demands of modern warfare were not of a sufficiently high quality to keep the systems working. This put the onus on the component manufacturer. If he would only make good components, the country would be saved. The model, about which more will be said later, allows quantification of reliability, statistical testing, and estimation. In addition, it helps in developing specifications with reliability levels and acceptance tests written into them. However, it turned out that more was involved. Even the best parts, when misapplied in a circuit, will fail. Circuit conditions like voltages and currents, environmental conditions such as vibration, shock, temperature, etc., all come into play and require more sophisticated models. However, the way was shown, and a new breed of engineers was needed—the reliability engineers, who required support by mathematicians and other specialists. These

reliability engineers are typically associated with the designers of the equipment. Many organizations have found out, and are still finding out, that there is more to reliability than putting numbers into a statistical model and writing numerical reliability requirements into a specification. Reliability is, as it always has been, an engineering function.

Before illustrating this with a few examples, let me point out that the design of electrical and electronic systems has from the start (i.e., the

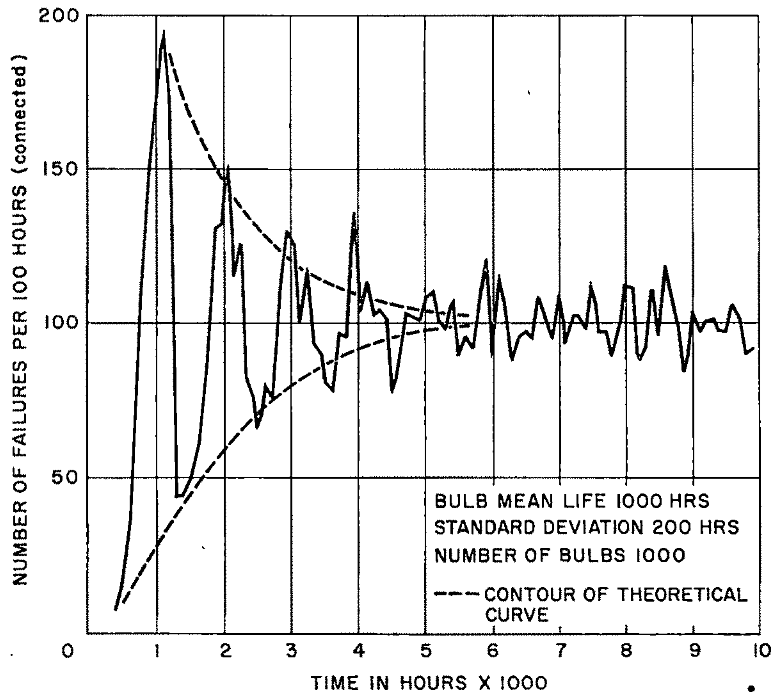

FIG. 1.

end of the last century) made an extensive use of mathematical models, mostly on a deterministic basis. The new aspect that has been added by the events described is mainly the accelerated use of statistical models and techniques.

Turn for a moment to Times Square, New York, and watch the lighted news sign as an example of an active system. It contains approximately 13,000 light bulbs. Everyone knows that the life of incandescent lamps is finite. A reasonable assumption is that the life is normally distributed around a mean value of say 1000 hr, with a standard deviation on the order of 100 or 200 hr.

The next assumption is that the system, after renovation of the *Times* Tower which is in process now, will be stocked with new bulbs and that,

by some magic maintenance facility, each burned-out bulb will be replaced instantaneously. Watching the system as a whole, what pattern of failures will we observe? Almost no failures will be observed in the beginning, perhaps not until the bulbs reach 500 to 700 hr life. An increasing number will fail as we reach the mean life of the bulbs, i.e., 1000 hr. Thereafter there will be a gradual decay in the observed failure rate until around 1500 hr. A second maximum, due to the second generation of bulbs, can be expected after 2000 hr. However, the curve of failure rate against time will show some broadening, and lowering of its peak, as the replacements of early casualties in the first generation might sometimes fall between the two generation peaks. In subsequent generations, the peaks will get lower and lower and the valleys will be filled up. A picture of the results of a simulated experiment is shown in Figure 1, which demonstrates that, after a number of mean lives, the number of failures will fluctuate slightly around a constant value. It is of interest to note at this point that Drenick [4] has shown that, under very weak constraints on the type of life distributions, complex systems containing many components tend asympotically toward a constant number of failures per unit time, a situation which can be handled by means of a model using the Poisson distribution.

This property is used with much enthusiasm by reliability people: a complex system with constant failure rate, where failure rate is defined as the number of failures per unit time, is not too difficult to handle mathematically.

At this point we can introduce a very popular definition of reliability: "Reliability is the probability that an equipment will perform its intended function for a specified period of time when used in the manner and for the purpose intended."

Let us test this definition against the performance of the light bulb system. Instantaneous replacement implies that the probability that the system is in operating condition is 1.0 (certainty). A more meaningful definition of reliability for this case might be the probability that the system will operate for a time t, without a light bulb failure. Knowledge of this reliability function as a function of time might interest the man responsible for replacing the bulbs.

One additional aspect should be mentioned. Suppose the maintenance man for the light bulb sign is absent. Failure of bulbs would degrade the performance of the news sign, but quite a number could be lost before the sign became unusable for the purpose intended, i.e., the understanding of the flashed message. This indicates that a single measure of reliability is not necessarily adequate to describe the reliability of the system.

In the case of the light bulb news sign, redundancy is presented by the combination of sign, reader, and language: several bulbs or even letters and words could be missing from the intended display and the reader

would still be able to understand the message. (I shall touch on other aspects of redundancy a little later.) On the other hand, if we consider a missile or a satellite, there might be a set of critical components such that, if one fails, the whole system is dead.

In this case one can use the model for the system reliability $R(t)$ as

$$R(t) = R_1(t)R_2(t) \dots R_n(t)$$

where an individual component reliability $R_j(t)$ is the probability that component j has not failed before time t.

For systems enjoying instantaneous maintenance, the probability $u(t)$ of failure at time t is given by the renewal equation:

$$u(t) = f(t) + \int_0^t u(t - y)f(y)dy$$

It is not necessary to elaborate upon these equations as they have been belabored by numerous people for the last ten years. However, I will have to refer to them in one of my next examples.

Before leaving the light bulbs, I should observe that it does take time to change light bulbs or, in general, to maintain a complex system, and that the time in which a system is out of order should be taken into account. The concept of "availability" of a system can be introduced. Generally a system is not available when it is being maintained. To illustrate this with our news sign, we assume that the message becomes unintelligible when one bulb fails. Then in the steady state the availability is given by

$$A = \frac{\theta}{\theta + \theta_r}$$

where θ = mean time to failure of the components (all assumed to be described by the same frequency density function), and

θ_r = mean time of detection and repair.

For an evaluation of availability in general situations, we can consider that the failure times and repair times have distinct statistical distributions and that the failure distribution of the system is represented by a generalized renewal equation incorporating these two distributions. The problem has also been attacked in other ways, e.g., by modeling it according to a semi-Markov process with two states: the operating state and the repair state, with certain transition probabilities and distributions of the time spent in each state.

A second example illustrating reliability aspects, differing from the ones discussed so far, is provided by the transatlantic telephone cable. There are now several telephone cables linking Europe and the North American continent. The oldest telephone cable of this group was put into service in 1956 and has operated so far without a failure in the underwater electronic equipment.

One complete cable connection consists of two cables: one for west to east and the other for the return transmission. The length is about 2000 miles. To transmit a frequency band sufficient to accommodate the 36 speech channels, for which the system was originally designed, amplifiers are required at approximately 40-mile intervals. This adds up to 51 amplifiers in each direction or 102 amplifiers in the total system. Each amplifier has 60-odd components such as resistors, vacuum tubes, etc.

For reliability considerations, as a first approximation, the model for the system can be very simple: a series chain of elements. If any one of the about 6000 components fails, all 36 telephone conversations are dead.

Adding to this series model the reasonable assumption that the failure rate of the components is constant in time, we can easily show from the equation discussed before that the failure rate of the system is the sum of failure rates of the components. Thus the system has a constant failure rate (Poisson process), which is equivalent to saying that the times to failure are exponentially distributed.

Now the reliability design objective for the system was set at one failure in 20 years of continuous service, which requires for each component a failure rate of roughly one in 10^6 per 1000 hr.

To demonstrate ahead of time, with 90% confidence, that such a failure rate has been achieved in the population from which the components for the cable are to be taken would require about 200,000 components tested for one year without the occurrence of a single failure. It is not surprising that such a test has not been run. To show what has been done, one has to switch from statistics to engineering.

To achieve the reliability objective, the following principles were adhered to:

(*a*) Use materials and components proved by experience.
(*b*) Design specifically for the application and environment.
(*c*) Manufacture to rigid specifications, under close engineering control using tightly controlled raw materials.
(*d*) Screen and inspect thoroughly, rejecting any product which behaves abnormally.

I could discuss these points in technological detail to show that there is more to them than just pious words, but this would lead beyond the scope of this paper. Statistics played a role but not often a very prominent one. In the first place, "proved by experience" does not imply determination of failure rates or time to failure, rather it indicates five to ten years of engineering experience under all kinds of environmental and service conditions. New materials, though promising in their performance, were not used. The design of the vacuum tubes and paper capacitors, both difficult items, started in the 1930's, more than 15 years before the first cable was constructed.

In the inspection operation during manufacture, a strong emphasis was placed on "being in statistical control." In one instance, the observed distribution of a certain parameter of a component, when plotted on normal probability paper, had an elongated S shape indicating a mixture of two populations. Even though the over-all outside tolerances were met, the components were not used in the amplifiers until the cause of the population shift (in this case a shift in mean) was explained and proved to be harmless for life expectancy. That this cause could be traced was mainly due to the complete pedigree which was maintained about each part from raw material to final product—another tool to promote the integrity of the manufacturing operation.

Stringent restrictions on the size and number of components, imposed mainly by the requirement that the repeater should be laid by the cable ship as part of the cable, precluded any of the well-known techniques to reduce the impact of an individual failure on the system performance (e.g., redundancy). Where these space limitations do not exist, reliable operation of a system can be achieved by using appropriate circuit design techniques, without demanding the ultimate in component reliability. The concept of redundancy has already appeared in the lighted news sign example, where, even if some bulbs did not light, the reader might infer the message. There the redundancy is in the language. A simple way to introduce redundancy in a circuit is to use two components in parallel so that if one fails the circuit is still operational.

Simple algebra [5] enables one to evaluate the theoretical probability of failure of a structure consisting of any series-parallel combination of elements. Whether these structures can be designed so that they meet the performance requirements is another matter, but they can be made extremely reliable [6].

In a recently laid transatlantic cable, redundancy is applied in circuits using vacuum tubes, these being very critical elements: 2 strings of 3 tubes each are used instead of one.

A good mechanical analogue of this redundancy situation is the split into two independent hydraulic systems some car manufacturers are now using for brake systems. Anybody who has experienced a sudden loss of brake pressure will realize the reliability implications.

Ignoring again the complications introduced by redundancy in the simple series model, we return to the submarine cable represented by this model to illuminate another aspect of systems reliability.

One of the important parameters of the amplifier is the gain in speech energy it supplies to offset the cable losses. These cable losses increase with the frequency to be transmitted. At the highest usable frequency, each amplifier is required to supply a gain in energy of one million times. Over the total circuit the gain must be maintained within about $\pm 40\%$ of the nominal level 95% of the time. This requires not only an appro-

priate accuracy for the nominal gain values for each amplifier (everything adds up over the 2000 miles) but also a close check on the unavoidable statistical fluctuations of the component parameters in each amplifier. Extensive statistical studies were necessary in the design stage to relate the expected fluctuations in the actual values of the component parameters to the variation in the amplifier gain and, hence, in the system's performance. These statistical studies can be roughly characterized as "propagation of error" studies. Working backwards from the over-all

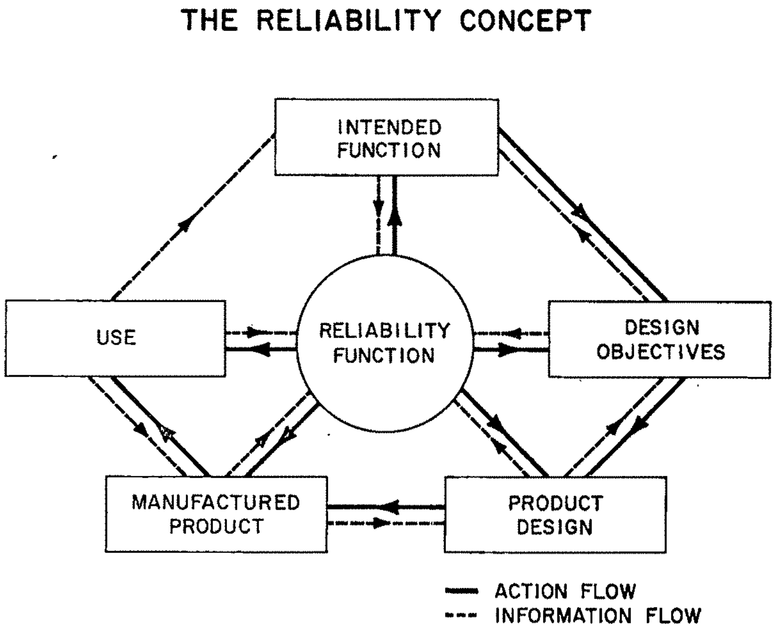

FIG. 2.

systems requirements, the tolerances for the individual components were specified not only for their initial values but also with respect to their aging characteristics over a period of 20 years. From the user's standpoint it makes very little difference whether a telephone circuit is out of service because it has failed completely or because it is out of tolerance.

In a similar situation, presented by a long landborne system, the performance of the amplifiers in the early hardware stages showed too large variations of the amplifier gain compared to the specified values. To pinpoint the subsystem responsible for these discrepancies, a series of experiments was laid out in a 7 × 7 hypergraeco-latin square. This turned out to be an efficient tool for locating the significant contributor to the variations.

I could continue citing examples of statistical tools used in the devel-

opment of systems contributing to their reliability. What I hope to have indicated sufficiently is that for a complex system it is unlikely that one can produce a single number *adequately* and *completely* describing the "reliability" of a system. The user is interested in a system that functions for a minimum total cost, including *maintenance* and *operation*, for the time period for which it is designed.

To conclude this general review, I shall discuss briefly the general flow of events from inception to completion of a system as seen from the reliability viewpoint (Fig. 2).

In both military and nonmilitary systems, the starting point is a specification by the user-to-be of the INTENDED FUNCTION. In a telephone system this would include: number of channels, type of service, points to be connected, etc. A military defense system would define the threat to be countered, the size of attack to be handled, the frequency of attacks to be expected, etc. Also, the reliability considerations, which include availability, maintenance, etc., must be defined.

With these specifications spelled out, the DESIGN OBJECTIVES have to be derived. The end product of this phase should be a break-down of the system into organic units, black boxes each of which would have a fully defined function. To make the transition from the intended function to the design objectives, several alternatives usually have to be explored, e.g., the present submarine cable systems cannot transmit TV—the experimental communications satellites can. The exploration of alternatives requires mathematical models, which for the reliability aspect will have their statistical character. Queueing theory often plays a major role. Maintenance activities, different for different possible solutions, involve cost and logistic considerations. Markov chain models and renewal theory are often used as well as developments from first principles in probability theory. The systems outlook must be maintained, as each organic unit goes its own way into the hardware stage.

The PRODUCT DESIGN involves the detailed steps necessary to prepare drawings for manufacture. Circuits must be designed. The choice of component parts suitable for the circuit conditions envisaged is of extreme importance. The failure and drift characteristics must be evaluated with respect to the circuit requirements. For reliability purposes, statistics provide: design of experiments, distribution theory in connection with life testing, time series analysis for environmental testing, etc.

In MANUFACTURING, the designs must be materialized. Statistical quality control is obviously required to maintain the reliability designed into the system. Sampling theory for inspection and possibly appropriate acceptance tests for units and subsystems must be designed to be used on a routine basis. It might not be economically feasible to achieve a reliability guarantee at a predetermined high confidence level, but the operating characteristic curves must at least be well defined.

The USE phase provides the eating of the pudding. As indicated by the arrows all around, every action is connected with a feedback. Nature has to be fought all the way, and numerous adjustments are to be expected. In the use phase we can see whether the design intent is met.

The important and difficult task in creating a reliable system is that the systems outlook is maintained all around the circle. Otherwise, as experience has shown, the components might be fine, but the system will not work.

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7. Acknowledgments: I am indebted to several colleagues at Bell Telephone Laboratories for their useful comments and corrections.

HERE AND THERE

By the Board of Editors and the Membership of The Sigma Xi-RESA Societies

In May 1965, the M.I.T. Press, Cambridge, Mass., published a volume entitled *Tizard* by Ronald W. Clark. A reprint of an earlier publication in England, the book "is more than a well-written biography of a dedicated scientist, for in describing the bitter political infighting that accompanied the birth of radar, Ronald Clark unfolds the story of a new kind of *scientific administrator*, working in government.... Tizard's opponent on matters of both policy and technology was F. A. Lindemann, later Lord Cherwell." C. P. Snow, in the Godkin Lectures at Harvard University, dealt with this same theme from Lindemann's point of view, also presented by Lord Birkenhead in his biography of Lindemann, *The Prof. in Two Worlds.*

Through the courtesy of the M.I.T. Press we are permitted to present the Foreword to the book, by the English scientist, Sir Solly Zuckerman; the Foreword to the American Edition written by Vannevar Bush; and, lastly, the concluding paragraphs of Ronald Clark's rich biography.

TIZARD

By RONALD W. CLARK

Foreword

By SIR SOLLY ZUCKERMAN

It is only five years since Sir Henry Tizard died. But to many his name already seems to conjure up little more than a picture of some kind of personal struggle for power in places where scientific advice bears on the formulation of national policy. Sir Charles Snow, who dramatized the clash of Sir Henry Tizard with Lord Cherwell, could hardly have anticipated this outcome of his colourful account of the relations and characters of these two distinguished scientists—even if he correctly foresaw the debate which it stimulated about the channels whereby informed advice should be directed towards those who are responsible for government in a scientific and technological age.

Ronald Clark has now given us a complete picture of Tizard, based upon a study of some five hundred files of private papers which he left, and which had not been consulted before, as well as the diaries and unfinished autobiography which Sir Charles Snow studied. In addition Ronald Clark has had access to Lord Cherwell's archives, which had already been used by Lord Birkenhead in the biography he wrote. On top of all this he was able to consult a mass of private letters and documents belonging to various people whose paths had crossed Sir Henry's. The result is a volume which undoubtedly becomes part of the history of our times.

Tizard's greatest single achievement was the encouragement he gave to the development of the chain of radar stations which assured the R.A.F.'s victory in the Battle of Britain. None of the scientific ideas behind radar was his, but without his support, backed by the prestige gained through many years of intimate and successful co-operation with members of the Air Staff, it is doubtful if our defences would have held in those vital days of 1940. Tizard's own place in history is secure for this contribution alone.

But there were other contributions—all admirably told by Ronald Clark. He describes the events leading to the setting-up in 1934 of the famous Committee of Air Defence, in whose deliberations distinguished scientists, serving officers, and politicians played a part, and which foundered in 1936 because of the clash between Lindemann, later to become Lord Cherwell, and the rest of the Committee, led by Tizard. Fortunately, this first phase of the Committee's existence had not passed before the foundations of radar were securely laid. Clark describes the later reconstitution of the Committee without Lindemann, and the next eighteen months during which a radar system proper was built. He then tells of the eclipse of Tizard and his committees, and of the increasing dominance of Lord Cherwell, Sir Winston Churchill's chief wartime adviser on scientific and technical matters. Ronald Clark also gives us a clear account of Tizard's return to Whitehall after the war, and of many other events which uniquely illustrate phases in the evolving system within which science and politics converge.

The picture which is painted of Tizard will prove eminently true to those who knew him well. Tizard's achievements were great. He was a scientist who immediately won the confidence of the scientific establishment. He commanded loyalty even when his judgment was at fault. For Tizard was not always right. He was sceptical about the likelihood that scientific

knowledge of the atom could be harnessed for military purposes; he felt that 'unnecessary excitement' was being generated by the belief—correct though it proved to be—that the navigation of German aircraft was being assisted by radio beams. There is the suggestion that he held that the invasion of Normandy in the summer of 1944 would be bound to prove a disaster. Events have also told against his vision of the institutional ways whereby science can best help the State. And as Ronald Clark shows, some of his views, as well as those of his staunchest allies, were in the latter part of his public career too often coloured by a violent antipathy to Cherwell, who emerges from Ronald Clark's book not quite as black as on other occasions when his name has been mentioned in the same breath as Tizard's.

In spite of all this Tizard is correctly revealed by Ronald Clark's book as a beacon of a particular kind in the history of England—as a man who more often than not pointed the right way, and as a person who could be relied upon to fight to keep it open.

5th October 1964

Foreword to the American Edition

By VANNEVAR BUSH

Much has been written about the disagreements between allies during a great war. Little has been written about the deep friendships which appear among comrades in arms of different nations, even among comrades whose efforts, behind the lines, are devoted to placing advanced weapons in the hands of fighting men. It is especially fitting that an edition of a book on Henry Tizard should appear in the United States for he exemplified strongly the mutual respect and friendship which developed between scientists and engineers of the United States and their counterparts in the United Kingdom.

It was primarily due to the skill, patience, and geniality of Tizard that interchange between Britain and the United States, on the development of weapons, was put into effect and made fully operative, long before the United States entered the war. There is no doubt that this collaboration resulted in a more effective war effort and contributed significantly to ultimate victory. In so doing he made a host of American friends who will welcome this volume.

It was not easy to do. There were stuffy admirals and generals on both sides of the ocean. There was a widespread conviction

on both sides of technical superiority, and there was doubt as to the others guardianship of secrets. The partnership between scientists and military men, which later in the war produced striking results, had not then developed. The civilian scientific effort in the United States had barely begun. The maze of complex loose organization on both sides had not crystallized into effective form.

Through this confusion, Henry Tizard threaded his way with a sure step. He fully attained his objective, and left no bitterness behind. We, in the United States, hail his memory with gratitude and affection.

8 September, 1964

Concluding Paragraphs

By the autumn of 1959 Tizard had shed a number of business responsibilities. He was beginning to "take things carefully," and there are some indications that he felt more than normal concern for his health. Yet there was little specific warning when he collapsed suddenly at Keston on the evening of 8 October. The cause was a cerebral haemorrhage, and he died early the following morning.

It was probably as he would have wished. When his old friend Sir Frank Heath died in 1946, Tizard wrote to Heath's son. His father, Tizard pointed out, had "lived a long life and did great work, and never lost interest in men and things, and I gather that he died suddenly and without any suffering. So what more can one ask for?"

His ashes were buried in the floor of the Oriel College Antechapel, below a stone bearing his initials and the dates 1885–1959. On 19 November, a Memorial Service was held in the Henry VII Chapel, Westminster Abbey. It was a Service whose setting Tizard would have appreciated and about whose congregation he would no doubt have had perceptive comments to make. It included a fair cross section of those among whom his life had been spent—not only relations and representatives from the various bodies he had helped to direct. It included, also, such people as Bill Carpenter, the former head-porter from Imperial College, now very old himself and racked with arthritis, who had come from his home in south-east London. Tizard would have liked that.

He had been one of those men of ability and charm who can do most of the things they wish to do. They are limited only by their self-imposed objectives and by the price which they are willing to pay for success. Tizard's personal objectives had

been modest. He wished to see science used as he felt it should be used to help the nation—and other nations for that matter; he wished to leave the world a better place than he found it. But for himself he wanted little more than did his old friend Sir Richard Threlfall, whose "ambitions were of the homely friendly sort," as Tizard himself put it. The wonder is not that he failed to reach the Lords or to become Britain's first Minister of Science, but that his own aims carried him so far, reined in as they were by his own rigid principles. For no man was quicker to pounce on any attempt to smudge the line between right and wrong; no man more suspicious that when science or scientists entered the political area the fight would rarely be on equal terms. Thus in his attempts to achieve what were really political ends he was handicapped by an unwillingness to use anything that fell within his own clear definition of intrigue, a definition including much that passes as the common coin of political life. There is not the slightest indication that he ever regretted this; indeed, there was no reason why he should. He had gone far. He was much loved. He knew that with the passage of the years it would become ever clearer that the battle of "the Few" had been won with the weapon of radar, handed to that bright company almost as the chocks were pulled away. He had, moreover, walked not merely with crowds but also with politicians and yet kept his virtue; in itself, perhaps, a considerable enough achievement for any man.

NOTICE

In accordance with Article III, Section 1 of the Constitution, notice is hereby given that the 66th Annual Convention of The Society of the Sigma Xi will be held on December 29, 1965, at 9:00 a.m. in association with the AAAS meetings at the Hotel Claremont in Berkeley, California.

The December issue of AMERICAN SCIENTIST will contain the complete agenda for the following scheduled sessions:

8:00–9:00 a.m. —Registration

9:00–Noon —First Session

Noon–1:30 p.m.—Luncheon

1:30–3:30 p.m. —Second Session

Following the Convention, at 8:30 p.m. in the Harmon Gymnasium of the University of California, Dr. Jacob Bronowski, Deputy Director, The Salk Institute for Biological Studies, will deliver the Annual Phi Beta Kappa–Sigma Xi Address on the theme of "Science and the Other Humanities."

Kodak advertises:

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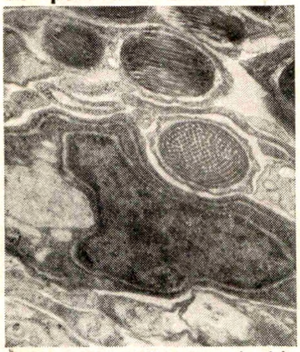

Delbert E. Philpott took this beautiful electron micrograph on a KODAK Projector Slide Plate, Contrast. It shows the ultimate source of the sticky secretion on the tube feet of starfish that gives them a grip powerful enough to open oysters. The sieve-like object is a cross-section of one of the secretory packets that Dr. Philpott discovered. Two more secretory packets are seen in longitudinal section above it. The large object beneath the packets is the cell nucleus.

How much easier life would be for biology students without the long tale spilled out on KODAK Projector Slide Plates by electron microscopes over the past quarter century! Used to be KODAK Lantern Slide Plates. No difference. (Only the name was updated. The old name was redolent of coal oil.) In addition to "Contrast," they also come as "Medium." Very little difference in electron-exposure behavior. ("Not so," argue some customers. We don't argue back. The customer is always right.)

What, then, has happened to the spirit of progress in materials for electron micrography? This is a very difficult question on which we have been working for a long time. About all we have to show for this work is an article entitled "Some Things Every Electron Microscopist Ought to Know." It's free from Publications Service, Eastman Kodak Company, Rochester, N. Y. 14650. It chats a little theory (very, very lightly) about electron image structure. Dr. Philpott had never read it before making the electron micrograph that revealed the starfish's secretory secret.

Operations in the π-clouds

The U. S. Air Force has been kind enough to develop a new electron acceptor and kind enough to tell the world about it, including us, who have promptly made and marketed it as EASTMAN 9724, *9-(Dicyanomethylene) - 2, 4, 7 - trinitrofluorene*. This is a fine thing for an air force to do.

A Ph.D. candidate a decade ago acquired a large number of our benzene derivatives and watched them under the microscope to see which would and which wouldn't form brightly colored addition compounds with *2,4,7-Trinitro-9-fluorenone* (EASTMAN 7135) from a fusion mixture. Hoping at the outset for a touchstone to tell the mononuclears from the polynuclears, he wound up instead with a set of selection rules that his successors in interest have succinctly summarized in the π-bonding concept. Now all aromatics have been fitted out by the theoreticians with halos composed of the pooled left-over electrons. Unless electron-loving substituents have drained off the π-cloud or the halo has been otherwise bent somehow, a new variety of stable complexes will be formed with the trinitrofluorenone structure. Except that if the Air Force is right and you do it with EASTMAN 9724 instead, the complexes will be *more* stable. The absorption maxima of the charge-transfer bands—whence comes the bright color—occur at longer wavelengths that do not confuse with the absorption bands of the aromatic hydrocarbon donors. Want a list of the maxima as seen in *Dichloromethane* (EASTMAN S342 is the Spectro Grade)? They're beautifully spaced.

This EASTMAN *Organic Chemicals business is handled by Distillation Products Industries, Rochester, N. Y. 14603 (Division of Eastman Kodak Company).*

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THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

SEE INDEX AT END OF THIS SECTION

The Scientific Endeavor. Centennial Celebration of the National Academy of Sciences; 332 pages; 90 illustrations; $2.50 paper; The Rockefeller Institute Press, 1965.

The Past President of the National Academy of Sciences, Detlev W. Bronk, and the President of the Academy, Frederick Seitz, in the Foreword to this volume write:

"The Centennial of the National Academy of Sciences was celebrated in the Autumn of 1963. Many of the 650 members of the Academy gathered in Washington throughout four festive days of social events and brilliant scientific discourse. They were honored by the National Government [which] their predecessors and they had served for a century. Representatives of sister academies, learned societies, and universities throughout the world brought greetings which stressed the world-wide scope and unity of the scientific endeavor—especially appropriate to our Academy in which one-quarter of the members were born in foreign countries. . . .

"The President of the United States and twenty-three distinguished members of the Academy delivered memorable addresses in which the History of the Universe, the Nature of Matter, the Determinants and Evolution of Life, and the spirit of the Scientific Endeavor were described in a score of remarkably relevant and coherent accounts.

> Day after day the mysteries of life were laid bare, and antecedent to life the structure of matter, and indeed of the universe, were presented in dramatic and fascinating clarity . . . the juxtaposition of topics did a very great deal to show the essential unity of scientific disciplines however different their techniques. . . . We listened to great wisdom. . . .

said Professor I. I. Rabi in the final discourse.

"Because the addresses contained so much wisdom, were presented with clarity, and revealed the unity of science, it was agreed that they should be made available to a larger audience than could assemble for our Centennial. That is the purpose of this book."

Each member of Sigma Xi and RESA could read this collection of addresses with great profit and with gratitude to The Rockefeller Institute Press for providing the opportunity for each purchaser to share in the Centennial feast at an outstandingly bargain price.

General College Geology by A. J. Eardley; 499 pages; $9.25; Harper & Row, 1965.

In *General College Geology*, Dr. Eardley has attempted to provide the basics of elementary geology in one text. The author's concise writing and the many truly magnificent illustrations and diagrams make this book one of excellent clarity. Generally the text includes the most recent scientific knowledge, and chapters concerning astrogeology and the geology of the ocean floor clearly coordinate modern ideas of related sciences.

The book suffers, however, by trying to cover too many topics in too little depth. It has been written on the debatable premise that unreasonable overlap exists in contemporary textbooks treating physical and historical geology. The result is a combination, under one cover, of diluted versions of these subjects. Many fundamental geologic concepts are completely omitted or hastily

covered in a few paragraphs. Among these are: mass wastage, isostasy, cycle of erosion, geosynclines, uniformitarianism, causes of mountain building, crystallization order of a basaltic magma (Bowen's Reaction Series), etc. Metamorphic rocks and metamorphism are treated in three pages of text and the entire physical evolution of North America from the Cambrian to the Pleistocene in five short pages.

Dr. Eardley suggests that *General College Geology* may be suitable for use in either a one- or two-term course. A one-term course may consist of a broad survey of the available subject matter including discussions of organic forms and development. This book can be used profitably in such a course.

There seems, however, to be no obvious advantage to using this text for a two-term course. Indeed, the deletion of important concepts may be considered by many to be a distinct disadvantage.—*Dale F. Ritter*

Astronomy Highlights: "Apollo and the Moon" by F. M. BRANLEY; *"Birth and Death of the Stars"* and *"Space Age Astronomy"* by K. L. FRANKLIN; *"Captives of the Sun"* by J. S. PICKERING; *"Design of the Universe"* by S. I. GALE; *"Man in Space"* by F. C. HESS; *"The Sun in Action"* by T. D. NICHOLSON; *"Time and the Stars"* by J. M. CHAMBERLAIN; 32 pages each; \$0.50 each, paper; 8 booklets boxed. \$4; Natural History Press of Doubleday & Co. for the American Museum-Hayden Planetarium, 1964.

These eight little booklets, each only 32 pages long, provide at a popular level a first look at some highlights of modern astronomy. Each booklet is self-contained, so that it can be read independently of the rest, but consequently there is a fairly large amount of repetition among them. The short length, and illustrations on every page, make for high readability. A glossary and reading list at the end of each are useful items.

As in almost any collection written by several authors, the level and presentation are not uniform. Kenneth Franklin's contribution on stellar evolution is perhaps the best written, and contains some very good diagrams, but is more advanced than the rest. The three on space astronomy and space flight are also well done, in spite of the fact that telescopes in space rate only two pages of description. "The Sun in Action" suffers only from the lack of a diagram to explain limb darkening.

"Design of the Universe" however is often confusing. For example, the terms "galaxy" and "Milky Way" are not clearly distinguished, and there is one mystifying statement, without further explanation, that the apparent brightness of a star is *usually* less than its absolute brightness. There are similar misleading statements in the other two of the series, but the major criticism can be levelled at the editing. Some diagrams do not contain the same example as used in the text, and there is at least one which is placed four pages away from the relevant section.

In spite of these few shortcomings, these booklets can be recommended to the person who wants a brief introduction to a few simple ideas of astronomy. They will hopefully inspire him to learn more.—*John E. Gaustad*

Seurat & the Science of Painting by W. I. HOMER; 327 pages; \$12.50; M. I. T. Press, 1964.

Georges-Pierre Seurat died in 1891 at the age of 31. His natural bent was that of conferring a stillness in his paintings that at his finest moments recall the ennobled imperturbability of Piero della Francesca and Velazquez. Also, Seurat reflected a geniality that is preserved in his images of his place and time. Upon these qualities as an artist, Seurat chose to impress contemporary theories of light and color. He had a keen sense for the best theories by the best scientists of the time, and he skillfully selected those theories that could be best applied to his artistic development. And that is what this book is about.

Homer has organized his book around six of the major paintings of Seurat. He has dissected Seurat's manner of selection of spectral colors and the way the colors were applied. The book traces the

scientific origins for the ideas that Seurat used in each of the paintings. This is followed by an analysis of the paintings themselves to see if the ideas were deliberately used and to discern if the desired effects were achieved. They usually were.

Homer has done a beautiful job. There is space here to distill only an essence of Seurat and his sources from the aromatic mixture that Homer has brewed. The scientist whose writings on color were most thoroughly assimilated by Seurat was Ogden N. Rood, a physicist at Columbia University and a member of the National Academy of Sciences. In turn, Rood's synthesis derived from the researches of Maxwell, Helmholtz, and Dove; thus, Seurat was close to the origins of contemporary studies on vision, luminosity, after-images, and color-mixing. By juxtaposing pure colors, first by cross-stroking (balayé), then by painting in thin parallel lines, and finally by a true "divisionism" or "pointillism," Seurat sought to present a visual mixing of colors along the lines originated artistically by Delacroix, propounded by the chemist Chevreul and the critic Blanc, and quantitatively analyzed by Rood. Seurat developed his technical capabilities to such a high degree that he could even use visual mixing persuasively to give the impression that his paintings are illuminated from behind.

Having brought coloristic methodology as far as he could take it, Seurat addressed himself to the problem of conveying emotions by the direction of lines. For this he followed the precepts laid down by Charles Henry, his contemporary, and a kind of bizarre Renaissance figure who worked with proficiency in mathematics, physics, physiology, esthetics, psychology, chemistry, and the history of science. Still, Seurat was careful, in his selection of the ideas of Henry, to use only the formulations that seemed reasonable and capable of being projected onto the canvas. Homer analyzes with precision the means that Seurat used to convey gaiety in his last two major paintings, particularly *Le Cirque*, painted shortly before Seurat died.

This scholarly book by Homer is a definitive examination of a great painter who was one of the progenitors of the modern era of painting. It sets an imposing standard for all historians who wish to demonstrate a relation of science to their especial field of inquiry. The book could be read profitably by the scientist, the historian, and the artist, and each would learn a different lesson. A lesson that all should learn can be taken from the permutations from Seurat's exact regime that occurred after Seurat died. When Rood reflected on his own influence on impressionistic painting after seeing the paintings of Pissarro and Monet, he stated "I always knew that a painter could see anything he wanted to in nature, but I never before knew that he could see anything he chose in a book."—*R. C. von Borstel*

Human Genetics by M. Whittinghill; 431 pages; $8.95; Reinhold Publishing Co., 1965.

This book is designed for people with little or no previous training in genetics as well as for those whose training is more advanced. Consequently, the author has had to blend a text on the elements of genetics with a more specialized treatise on human genetics. He has succeeded in doing this in an interesting and informative and readable way, for the most part, but has been caught in the obvious pitfall of being too brief to be clear in a few places (e.g., the section on the control of genic activity in Chapter 15).

The book is divided into four sections. The first of these, entitled "Monohybrid Genic Segregation," has chapters covering among other things the concepts of genotype and phenotype; meiosis and mitosis; segregation in a monohybrid cross; basic statistics including the binomial distribution and chi-squared tests; and the elements of population genetics. The second section, "Regular Chromosome Behavior," is a potpourri of independent assortment; linkage and sex linkage; and quantitative inheritance. The third section, "Biological Interactions," deals with the factors governing the expres-

sion of the phenotype. The last section, "Mutation and Evolution," considers spontaneous and induced mutation; changes in chromosome number; chromosome aberrations; and mosaics.

The book is profusely illustrated with original diagrams and figures whose inspiration is derived from Roger Tory Peterson's "Field Guide to the Birds," and whose purpose is to present various concepts in simple visual terms. Unfortunately, this approach is not always successful and some of the diagrams are more confusing than helpful. For example, the Hardy-Weinberg law would be clearer if explained in algebraic terms than in terms of the diagram given in Chapter 6 (Figure 6-1).

Each chapter is concluded with a selection of useful references on human genetics and a problem set.—*Nicholas W. Gillham*

Introduction to General Relativity by R. ADLER, *et al.;* 451 pages; $12.50; McGraw-Hill Book Co., 1965.

This volume is a welcome addition to the still quite limited list of textbooks in a field which is today regaining importance. It gives a careful, well-balanced, up-to-date account of general relativity, pre-supposing an undergraduate knowledge of special relativity.

The differential geometry needed for Einstein's theory occupies roughly half of the text, but much of it is interspersed with the applications so that the reader does not lose sight of the physics. It is treated in the classical formalism still used in most contemporary research in relativity, and includes a discussion of differential forms. Of primary interest is, of course, the case of four-dimensional Lorentz manifolds with Euclidean topology, and some of the theorems (e.g., on p. 84, "Every closed tensor is exact") require modification in such fields as geometrodynamics, where different topologies are considered.

The physical chapters include the classical topics—gravitational field equations, Schwarzschild solution and consequences, linearized field equations, equations of motion and conservation laws—and a well-chosen selection of more advanced and special topics. Such subjects as the initial value problem, certain cosmological questions, and the Rainich-Misner-Wheeler "already unified" field theory are here treated for the first time in a textbook. Other valuable features are discussion of relevant experiments, and extensive bibliographies at the end of each chapter.

The authors use very successful didactic methods of motivation, heuristic explanations, and summaries to keep the reader aware of the final aim of involved arguments. In a few instances these may become misleading; for example, nearly a whole section (6.5) is devoted to a heuristic derivation of the light deflection formula which leaves the erroneous impression that the correct formula can be derived from the principle of equivalence alone. Finally, the nature of the Schwarzschild "singularity" could have been clarified by including the Fronsdal-Kruskal analysis in the extensive chapter on the Schwarzschild solution.

The extensive and clear treatment of general relativity in this book will make it a fine text for the serious student of Einstein's theory.—*Dieter Brill*

Olduvai Gorge 1951–61, Vol. I—*A preliminary report on the geology & fauna* by L. S. B. LEAKEY, *et al.;* 118 pages; $14.50; New York: Cambridge University Press, 1965.

Since 1931 Dr. and Mrs. L. S. B. Leakey, following up the pioneer work of the late Dr. Hans Reck, have been exploring and excavating in Olduvai Gorge. The surveying phase of their activities, from 1931 to 1947, was reported in Leakey's *Olduvai Gorge*, published in 1951. At that time the emphasis switched to large-scale excavating, which has been accelerating as financial support has increased. Seldom has money been better spent. The fascinating discoveries bearing on the evolution of man that have already been reported need no retelling here. Anyone with even a passing interest in human history will join with the Leakeys in expressing thanks to those granting bodies, especially the National Geographic So-

ciety, that have made the work possible.

This is the first of a projected series of volumes that will cover the work done during 1951–61. It deals briefly with the general geology of the gorge (Chapter 1) and sets forth the essentials of the rock sequence. Climatic problems are briefly discussed in Chapter 7. Hay's preliminary study of the stratigraphy of Beds I–IV that appeared in *Science* is reprinted as an appendix. Certain papers dealing with potassium-argon dating and published in *Nature* are reprinted as Chapter 8. Parenthetically, it may be said that a date of between 1.75 and 2 million years for the lowest hominid remains now seems reasonably secure.

The bulk of the volume (Chapters 2–6) deals with the faunas. The large collections and much better specimens that have come from the excavations permit extensive revision of the faunal lists published in 1951. It is now set forth that Bed I is late Villafranchian in age, that there is no real faunal break between Bed I and the lower part of Bed II (this terminology is an inheritance from Reck; "bed" as used by him had a very different meaning from that now applied to the word in modern stratigraphic parlance), which is regarded as latest Villafranchian and essentially contemporaneous with Omo. Within Bed II a major faunal and stratigraphic break is reported. Faunas from the upper part of Bed II through Bed IV are of mid-Pleistocene type. Thereafter, the fauna, so far as known, appears to be essentially that of modern Africa. Treatment of the various groups is perforce uneven. Only the suids and bovids are discussed by Leakey in any detail. Accounts of the others are really progress reports, ranging from brief to cursory.

Publication of the projected volumes will obviously take some time. Meanwhile, work at Olduvai continues, new discoveries and new interpretations are being made. Inevitably, the volumes, limited to 1951–61, will be partially out of date before they are published. Is publication now worth while? The answer, of course, is an emphatic yes. In any crucial locality, such as Olduvai, periodic stocktaking is essential if the undertaking is to continue serving as a standard of comparison and not break down under a mass of specimens and raw data. As Simpson has well put it in his Introduction: "For work of less complexity, of less urgent importance, and of less worldwide interest to anthropologists, archaeologists, palaeontologists, and geologists, we might be willing to await more nearly definitive monographic publication. We are not willing to wait for available information on Olduvai, and indeed cannot do so if we are to continue our related studies effectively." May there be many stocktakings before the tale is done.—*Bryan Patterson*

Technology & Uses of Liquid Hydrogen, edited by R. B. Scott, *et al.;* 415 pages; 17.50; The Macmillan Company, Pergamon Press, 1964.

From the first liquefaction of hydrogen in 1898 (by Sir James Dewar) to 1952, only small quantities were produced and this was done in scientific laboratories for experimental purposes. From 1952 until the present, various processes have created a demand which has been estimated as approaching 48,000 tons per year by 1966. The largest plant alone should be now able to produce about 60 tons per day. Space flight needs use the major share of this production.

These facts, and many others, can be culled from R. B. Scott's introductory chapter which serves to summarize the chapters and supply background, history and supplementary highlights. The numerous authors present chapters covering most, if not all, of the field(s) delineated by the title, from the thermodynamics of liquefaction and large scale industrial operations to safety measures in the lab and industrial plant. Other chapters cover deuterium distillation, liquid hydrogen bubble chambers, neutron thermalizers and liquid hydrogen as a coolant/propellant for normal and nuclear rocket engines.

All told, there are 15 separate chapters and subchapters and 16 separate authors. Under these conditions the editors have done an excellent job of maintaining a fairly uniform level of

technical discussion and style of writing. There are only a minor number of typographical errors, there is a fairly complete and, in some cases, exhaustive list of references following each chapter.

Most of the chapters had been written in mid or late 1962 and so the delay in publication seems strange but is of minor importance in terms of the material under discussion. In conclusion, one might say that a fitting subtitle would be a Handbook of Liquid Hydrogen. It is to be recommended for inclusion in any technical library and for the personal library of anyone connected with the use and study of liquid hydrogen. —*Peter Grosewald*

Theoretical & Mathematical Biology, edited by T. H. WATERMAN & H. J. MOROWITZ; 426 pages; $12.50; Ginn & Co., Blaisdell Publishing Co., 1965.

This is a collective work; apart from three chapters by the editors, it contains fourteen diverse chapters by as many different authors. The value of a collective work is best judged by its prospective useful half-life, which, for similar books, is frequently of the order of two or three years; in this particular instance we would guess that it is larger by, at the least, an order of magnitude. The style of practically all the contributors is crisp, precise, and highly disciplined, and the bibliographies sufficiently selective to be of genuine value. Humble thanks are due the editors for restricting the vast area of molecular biology to just two chapters; anybody who buys this book is virtually sure to have on his shelf already some excellent presentations relating to this fashionable subject. The volume has one glaring and quite readily remedied fault: It should be called "*Methods* of Theoretical and Mathematical Biology." Assuming this change of title, the book is as excellent as such a work can possibly be. There are, on the one hand, no discussions on the general nature of the life process; on the other hand, such biological detail as appears has invariably been subordinated to the aim of developing generally applicable theoretical methods.

The reader who approaches this book in the forlorn hope that it will provide him with new insight into the highly controversial subject of general biological theorizing will not find his meat here, and this might be most fortunate. On the other hand, all those numerous workers who are in earnest about applying meaningful and non-redundant theoretical reasoning and mathematical, especially system-theoretical and statistical methods to their own procedures and research results will hardly be able henceforth to do without this work.—*Walter M. Elsasser*

An Introduction to Prehistoric Archeology by F. HOLE & R. F. HEIZER; 306 pages; $7.00; Holt, Rinehart and Winston, 1965.

The junior author of this book is the editor of the standard basic manual on archaeological field techniques in this country (*A Guide to Archaeological Field Methods*, 3rd rev. ed., National Press, 1959), which instructs students on how to survey and excavate archaeological sites in the field. The book here reviewed, is an attempt to explain to laymen and students of anthropology, without a bent for field work or the practical problems of archaeology, how this discipline operates. The authors, quite rightly, maintain that no general account of archaeological methodology has been written, hence this work is a welcome addition to archaeological literature. To this account additional perspective is provided by the inclusion of sections dealing with the definitions of the field and historical background. Little is mentioned of the content of prehistoric archaeology. A more precise title for this book would be *An Introduction to Prehistoric Archeological Method,* but such a title would have less appeal for the reading public to which it is addressed.

The book is written in 17 chapters grouped into 4 parts. Part one is introductory, two deals with excavation, three with dating, and four with interpretation. The coverage is comprehensive, but brief. The writers have an extensive background—the senior author focusing on the Near East Neolithic

and the junior author specializing in California and western North America—which produces a complementary and balanced coverage. Referencing is thorough, and an extensive bibliography and detailed index are provided.

Due to the subject matter the reviewer believes this book will have a limited use as a text despite its introductory presentation. Archaeology content books fill this market and are no substitute for a field manual. The higher price suggests that the publisher is aware of this fact. The book would be of great value to the college student majoring in prehistory, and the bringing together of material on terminology, excavation techniques, and interpretation makes it a useful addition to the professional archaeologist's library.—*B. K. Swartz, Jr.*

Experimental Methods in Gas Reactions by SIR HARRY MELVILLE & B. G. GOWENLOCK; 464 pages; $17; St Martin's Press, 1964.

The first edition of this book was written by Farkas and Melville in 1939 and the book has been out of print since 1946. In this second edition the book has been drastically revised by Gowenlock under the guidance of one of the original authors. The resulting book provides a wealth of valuable information for graduate students and physical chemists studying gas reactions. Useful reference data are provided in tabular form throughout the book, each of the seven chapters having an average of eight such tables. The experimental techniques are profusely illustrated by clear and instructive line diagrams—an average chapter has forty such drawings. The references to the original literature (over 1500 in all) are grouped at the end of each chapter and are up-to-date and comprehensive.

The first chapter summarizes the kinetic theory of gases and is followed by two chapters on the measurement and control of pressure and temperature, respectively. The next two chapters describe the preparation of gases and volatile compounds and the analysis of gases. The older chemical methods of analysis are particularly well described—these methods can often be combined most successfully with the newer physicochemical methods of mass spectrometry and gas chromatography which are briefly reviewed. A chapter is devoted to photochemical techniques and the final chapter describes practical methods for the study of gas reactions by static and flow techniques in both homogeneous and heterogeneous systems. A brief description of ultra high vacuum systems is also incorporated.

This book is most successful—its weakest point is in the description of modern analytical techniques. In particular, the sections on ultraviolet and infrared spectral analysis do not do justice to the power of these techniques. No mention is made of nuclear magnetic resonance methods which have recently been applied to the measurement of atom concentrations.

This book can be recommended strongly to all practical physical chemists and it also contains much that will be of value to others interested in vacuum technology. The price is somewhat high.—*Graham S. Pearson*

The Development of Weak Interaction Theory edited by P. K. KABIR; 286 pages; $4.95; Gordon and Breach, 1964. International Science Review Series, Vol. 5.

This volume is another in a series of review or reprint volumes. It is a collection of reprints of fundamental and not quite so fundamental papers on the theory of weak interactions. Included are papers like the original paper of Fermi on β decay, the paper of Dalitz on τ decay and the classic paper of Lee and Yang on parity violation, along with thirty-seven other papers.

There is no motivation to criticize the editor for including some nonfundamental or speculative papers. In fact, additional papers only enhance the completeness of the volume. We should remember, also, that choice of what to include is one of the ways in which the editor of a reprint volume expresses his own opinion on the subject matter, as long as there are no significant omis-

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sions, and in this volume there are none.

Reprint volumes like this one are so extremely useful to the researcher working in a field or desiring to review or begin work in a field, that everything possible should be done which would encourage people to edit, and publishers to publish such volumes.

Although most journal pages were shrunk photographically for inclusion, there is no difficulty in reading any of the pages. The volume has a pleasant and durable appearance.

This collection of reprints will interest elementary particle physicists most. However, Professor Kabir has included an historical preface which should help the nonspecialist and student.—*Frank Chilton*

The Physiology of Diurnal Rhythms by J. E. HARKER; 114 pages; $3.95; New York, Cambridge University Press, 1964.

This book is number 13 of The Cambridge Monographs in Experimental Biology. Like the others in the series, it deals with a rather specialized area of biology and it can be reviewed from the point of view of two types of reader. The specialist in the field may be most interested in those sections of the book in which the author discusses her own work, some of which is previously unpublished. The nonspecialist, or perhaps one who is just entering the field, will wish to know whether the book serves as a good general introduction to the subject.

There are quite a few errors, only a few of which will be mentioned here. Several of the figures which have been copied or modified from other authors are either incorrect or incomplete. For example, the ordinates in Figures 11 and 20 are incorrect and inaccuracy in redrawing some of the figures may lead to misinterpretations (Figures 17 and 25). Other errors which involve quite erroneous inferences include the incorrect statement that the ultraviolet induced phaseshift in the *Gonyaulax* rhythm is photoreversible (p. 57) and the description of a rhythm in nucleic acid metabolism in liver tissue by Halberg and coworkers (p. 59). The liver metabolism rhythm was studied in intact animals rather than in liver slices *in vitro* as suggested by Harker.

There are also inaccuracies of a quite different character, some of which will certainly not be obvious even to the specialist in the field. For example, in Figure 12 of the book, Miss Harker presents results of her own (unpublished?) experiments with the cockroach *Blaberus* illustrating an "after-effect" on the free-running period length. On the same figure, she has plotted the results of calculations which she has made from the unpublished text figures of another worker's Ph.D. thesis (Roberts, S. K., 1959, unpublished Ph.D. thesis, Princeton University) in which another cockroach, *Leucophaea* was used. These calculated points fit her own experimental curve beautifully and would seem to be confirmation of her results. Roberts, however (personal communication), has pointed out that when he plots the results from his thesis in a corresponding way he gets somewhat different results. Other previously unpublished work by Miss Harker, which is shown in Figures 21, 22 and 23, would be of interest to the specialist if they were presented more completely. Unfortunately, the experimental results which are shown are incomplete. For example, no results are given for the time taken for *Drosophila* pupae to develop from the time of costal vein pigmentation to the time of eclosion. The absence of this information, as well as the absence of a description of her methods makes it impossible to evaluate these results. These examples, which are only two out of several, illustrate the limited usefulness of the book to the specialist.

The book is not intended to be comprehensive and no attempt is made to discuss the relationship which diurnal rhythms have to photoperiodic phenomena or the orientation of animals by celestial clues. Plant rhythms are also rather inadequately covered.

Other books are available which are more complete with respect to coverage of material and this book makes no claim to be a reference source. Neither is it a discussion of the problems of the

field. It is rather a summary of some of the characteristic features of diurnal rhythms. After a short introduction, chapters are devoted to a description of the environmental control of phase-timing, free-running rhythms, and phase perturbations. Two other chapters deal with physiological processes and with abnormalities in rhythmical systems. A short chapter is devoted to a discussion of unknown external time signals and the work of F. A. Brown and the final chapter is a discussion of the author's views.

This monograph does not have the scholastic merit which would make it useful as a reference work nor does it satisfy the need for a book to introduce the subject to the nonspecialist. If it will stimulate others to do useful work, it will perhaps have served a useful purpose.—*Victor G. Bruce*

Practical Handbook on Spectral Analysis by V. S. BURAKOV & A. A. YANKOVSKII; 190 pages; $12; The Macmillan Co., Pergamon, 1964.

The purpose of this book is to encourage the use of emission spectroscopy as an analytical tool in Soviet industrial laboratories, as the Foreword makes clear. In this task the authors ought to have a fair chance of success as the book is written concisely and straightforwardly with emphasis on concrete applications.

Following a six page introduction of elementary theory, the first three chapters cover light sources, visual methods, and photographic methods of spectral analysis. In each case there is a description of Soviet instruments available, instructions on their selection and use, and descriptions of calibration and analytical procedures. There follow chapters on the analysis of metals and alloys, and of powders and solutions. Finally, there is a chapter on setting up a spectral analysis laboratory, with costs in roubles of the instruments and specifications for various sizes of laboratory. The bibliography lists ninety books and articles on the subject, all of them published in the Soviet Union.

The Russian analyst is thus provided with a useful guide to emission spectroscopy. Its utility for his Western counterpart will be understandably less. One is left wondering what audience the editors of Pergamon Press had in mind in publishing the book. It should be of greatest value to people interested in the state of the art of Soviet spectroscopy. These will however be somewhat disappointed to find that the book is over four years out of date, the Russian edition having been published in 1960.—*Thomas G. Spiro*

The Structure of Atmospheric Turbulence by J. L. LUMLEY & H. A. PANOFSKY; 239 pages; $9.50; John Wiley & Sons, Interscience, 1964. Vol. 12 of Monographs & Texts in Physics & Astronomy.

The theory of turbulence in its simplest form—homogeneous, isotropic turbulence—is a subject which has frustrated, and continues to challenge, an imposing list of investigators. In the much more complicated form of atmospheric turbulence, it presents problems which, though well-nigh intractable at present, cannot be ignored by serious students of atmospheric dynamics. *The Structure of Atmospheric Turbulence* by John L. Lumley and Hans A. Panofsky, which combines expert knowledge of both of these forms of turbulence, presents a well-balanced treatment of theoretical and experimental studies of turbulence in the atmosphere, which is quite welcome at this time.

The book is divided into two parts: Part I is an introduction to turbulence theory, and Part II gives a description of atmospheric turbulence, based on measurements but placed within the theoretical framework outlined in Part I. In turn, Part I contains two chapters entitled "Statistical Description of Turbulence" and "Mechanics of Turbulence." The first of these gives a very well-thought-out presentation of the basic descriptive tools of turbulence theory and should be useful as a dictionary of turbulence for those who brave the literature for the first time Part II contains three chapters; one on the mean winds and temperatures near

the ground, one on the fluctuating quantities themselves, and one on the spectra and scales of various quantities. It brings out concisely what is known of these subjects but never lets the reader forget how complicated the problems are, nor how much remains uncertain.

The book is short (under 250 pages) and it undertakes a large task. It certainly could not pretend to be complete. Yet, I was surprised that modern treatments of turbulence theory, such as the cumulant discard and direct interaction theories, were omitted, and disappointed to find the newly developing studies of turbulence in the upper atmosphere neglected. Also, though the writing is very careful, it certainly is not lively; and though details are carefully presented, the overall picture is not stressed. But, in spite of these objections, the book remains a useful and competent introduction to the study of turbulence in the atmosphere.—*E. A. Spiegel*

Radiation & Immune Mechanisms by W. H. Taliaferro, *et al.*: 152 pages; $5.95 cloth; $3.45 paper; Academic Press, 1964.

This attractively produced little monograph by three distinguished immunologist-microbiologists is one of a series sponsored by the American Institute of Biological Sciences and the U.S. Atomic Energy Commission with the specific purpose of focusing attention upon biologists' increasing utilization of radiation and radioisotopes as tools for studying living systems.

Although the book includes brief introductory accounts of the principles of innate and acquired immune mechanisms, and their humoral and cellular phases, it deals principally with three important aspects of the influence of ionizing radiation on immune mechanisms: (1) The capacity of X-rays to inhibit antibody formation and heighten the susceptibility of individuals to infection under certain conditions; (2) To enhance antibody formation under other conditions; and (3) To act as a useful tool to study the fascinating process of the antibody response at the biochemical level.

Some of the 13 chapters relate radiation-induced changes in the primary antibody response to biochemical and cellular phases; to different species; and to the dosages of antigen and irradiation involved. Others deal with such interesting and important topics as the means of protecting or restoring the capacity for immunological response in irradiated animals; the influence of irradiation on the phenomenon of immunological tolerance; and the action of radiomimetic substances on antibody formation. The text is well illustrated with original research observations. Despite its compactness, the authors have taken pains to make this book self-contained. It includes, for example, succinct up-to-date accounts of the function of connective tissue cells, of the phylogeny of the cells involved in immunity, and theories of antibody formation.

There is an excellent glossary, an author index, and a bibliography containing more than 300 references, mainly to publications that have appeared since 1951. Apart from being extremely useful to advanced students and scientists in other fields, this book is prescribed reading for immunologists unfamiliar with radiobiological aspects of their field.—*Rupert E. Billingham*

Atomic Migration in Crystals by L. A. Girifalco; 162 pages; $3.75; Ginn & Co., Blaisdell, 1964.

The study of atomic migration through crystals is a well established though somewhat specialized field. It is vital to metallurgists who study the transformation or oxidation of alloys, and it is a well established research topic for solid-state physicists and physical chemists. A decade ago it was studied only at the graduate level. However, with the rapid growth of interest in solid-state electronics and the science of materials, a much broader segment of the technically educated populace must have some knowledge of how, and how fast, atoms migrate in solids.

Traditional developments of diffusion in solids are long on mathematics and relatively short on the geometry of the crystals involved. This book is dedicated

to the proposition that there is a market for a book which "presents a descriptive account of atomic migration in terms of interatomic forces and the geometric arrangement of atoms in crystals"; one that is devoid of references and essentially free of mathematical derivations. The audience aimed at is the "intelligent layman" or "non-specialist," and the book gives a consistent and clear development that a non-specialist can follow.

The author begins by assuming only that the reader believes in atoms and then proceeds to discuss the forces between atoms, and to develop the various crystal structures that are found in monatomic solids. This leads easily into point defects in crystals, their concentration, and various mechanisms by which diffusion can occur. Some equations enter, since they provide the only concise way to state the basic differential equations of diffusion and the equation for the diffusion coefficient obtained from random walk theory. However, the meaning of the symbols in each equation is explained along with the physical model leading to equation.

Having progressed from atoms to activation energies in 55 pages, the next 80 pages are devoted to diffusion in metals, in ionic crystals, in covalent crystals, and along grain boundaries. The book closes with a discussion of the effect of a temperature gradient, or an electric field, on diffusion. A more rigorous (and mathematical) development of certain basic equations, along with references for further reading are to be found in appendices.

The book is clearly written throughout, and is outstanding in its development of the interplay between crystalline structure and diffusion. It is highly recommended to the technical graduate of a few years ago who is interested in learning about how atoms move in crystals, and, it should be in any library which caters to this group.—*Paul G. Shewmon*

Progress in Nuclear Physics, Volume 9, edited by O. R. Frisch; 310 pages; $15.00; The Macmillan Company, Pergamon, 1964.

Annual review volumes on a variety of subjects are a valuable tradition in physics. The present very good volume is a representative member of the series edited by Prof. O. R. Frisch, of Cambridge University. Although each volume of this series presents a mixture of articles that includes both theoretical and experimental subjects, the greater emphasis tends to be given to detailed discussions of new experimental developments. The articles on experimental subjects seem to be directed toward readers who are, in fact, interested in working in those fields, and who wish to become experts. By contrast, the theoretical articles seem to be directed toward a more general reader, one with a good background for the subject in question, but one who is not himself likely to become a participant.

Three articles of the present volume are devoted to experimental devices. These are by Rutherglen on spark chambers, by Dearnaley on the semiconductor counters that have become so valuable for low-energy nuclear physics, and by King on high-energy beam design. All three articles have been written with great clarity.

An article by Eden describes "structure analysis" of collision amplitudes. It is a pedagogic presentation of the line of thought that lies behind dispersion relations, the Mandelstam representation, Regge poles, and allied subjects. The author largely succeeds in his attempt at explaining the standpoint of such theoretical work.

Phenomena of "high-energy physics" are treated in two articles. Thus, Burhop, Davis, and Zakrzewski treat the processes that occur when strange particles interact with complex nuclei. Not only have these investigations yielded valuable information about the strange particles, but they are likely to yield interesting information about ordinary nuclei. The present article summarizes experiments using nuclear emulsion techniques. Future work is expected to use more powerful techniques.

The article by Farley is a very clear sketch of the beautiful experimental investigations of the interactions of mμ mesons with the electromagnetic field.

It is these investigations that show that, aside from questions of production and decay, mμ mesons have all the properties of heavy electrons.—*Norman Austern*

Carbene Chemistry, Vol. I of *Organic Chemistry* by W. KIRMSE, *et al.*; 302 pages; $9.50; Academic Press, 1964.

Of all the developments which have taken place in organic chemistry in the past fifteen years, probably none has brought more fascination than the discovery and exploitation of divalent carbon intermediates, designated (with the zest that chemists have for assigning catchy names) "carbenes." The initial surprise occasioned by the first evidence of their existence has given way to a great deal of research aimed at investigating their stability, structure, energies, and reactions. With the time ripe for a balanced review and seasoned appraisal of carbene chemistry, this excellent book of Kirmse is particularly welcome.

"Carbene Chemistry" could almost serve as a model for a scientific monograph. It is organized in a logical manner and written in a clear and straightforward (if somewhat dry) style. As any good book of this type should, it reviews the literature with a critical eye and suggests further experiments. It is thoroughly documented and includes references through most of 1963. As befits the many facets of carbene investigation, thorough coverage has been given to mechanistic studies, kinetics, methods of preparation, reactions, and synthetic applications. Two additional chapters contributed by H. M. Frey, P. P. Gaspar, and G. S. Hammond go into more detail on the energetics of carbene reactions and the question of spin states.

This book will inevitably be compared with Hines' monograph, "Divalent Carbon," which appeared at about the same time. Neither suffers by the comparison, however, since the main differences are in emphasis and style.

Dr. Kirmse has packed a lot of information into this compact book, and it is a welcome addition to the chemists' bookshelf.—*Richard K. Hill*

Taxonomic Biochemistry & Serology, edited by C. A. LEONE; 728 pages; $16.50; The Ronald Press Co., 1964.

The use of biochemical and serological methods as aids to taxonomy has recently been expanding. This book is a collection of 8 mostly excellent reviews and 39 representative papers on specific research work or reviews of specific areas.

Despite certain exceptions, mentioned below, I think it is fair to say that in most taxa biochemical (including serological and molecularly biological) methods have been about as useful as the study of a previously neglected morphological character or group of characters. Any additional character is helpful in taxonomy if well studied; the methods of biochemistry have not yet contributed importantly to either theory or major taxonomic revisions except in the bacteria.

This is not to say that such will always be the case. There are many possible biochemical characters that are unstudied (and also many in the behavior, gross anatomy, histology, and cytology of the adults and immatures of most groups). Some microorganisms, especially bacteria and those epiphenomena, the viruses, have few morphological characters, and it is here that both the greatest achievement and the greatest promise of biochemical taxonomy seem to lie. Transitional ("hybridizing") populations also frequently need biochemical study. And it is very possible that, when its methodology is stabilized, serology will make contributions in many taxa out of proportion to the number of tests made. Serology may indeed become as important to large areas of taxonomy as all of morphology; I cannot conceive of this happening in the next century with other biochemical methods.

Some biochemical taxonomists, like some others, overlook basic taxonomic principles. Demonstrably false assumptions in one or more papers of this book are the following: taxa classified as primitive are primitive in all important respects; recency of latest common ancestry is the only criterion in taxon-

omy; intraspecific variation is unimportant; convergence does not occur at the biochemical level; and different biochemical criteria will necessarily give the same classification. Most or all these assumptions are however rejected as assumptions, and sometimes tested, in other papers; biochemical knowledge is both compatible and frequently found with taxonomic competence.—*Leigh Van Valen*

Approaches to Paleoecology, edited by J. IMBRIE & N. D. NEWELL; 432 pages; $12.50; John Wiley & Sons, 1964.

Paleoecology is gradually emerging from its stagnant preoccupation with reconstructing the original environments of single fossil samples. This reconstruction is necessary as background and for establishment of the often narrow limits on available data, but in itself is of little significance for the advance of ecological or other major generalizations. Although paleoecological data bear on many or most of the current problems of population and community ecology above the descriptive level (sometimes in ways different from recent data), probably less than two dozen papers in the world literature have been mainly concerned with these areas.

The present volume is the best available summary of the major areas of paleoecology. Most of the 19 papers, each by a different author, are concerned with diagenesis (considered broadly) and the limitations imposed by the physical environment on the distribution of marine animals. Some treatment is however given to most of the important aspects of paleoecology, new as well as old, and there is a moderate amount of original material. It is perhaps significant of the state of paleoecology that, although the replacement of faunas in time represents one of the two greatest advantages that paleoecological data enjoy over recent data (the other is accumulation of specimens over a relatively long period, which is frequently a disadvantage), only one paper (by Shotwell) utilizes this advantage.—*Leigh Van Valen*

Paleomagnetism & Its Application to Geological & Geophysical Problems by E. IRVING; 399 pages; $19.50; John Wiley & Sons, 1964.

The essential simplicity of the principles underlying the application of fossil magnetism to such problems as polar-wander and continental drift have inclined many earth scientists to take the subject for granted. The data are either considered generally to support continental drift or rejected outright, commonly for that very reason. For the most part, however, neither the acceptance nor rejection are founded on any real up-to-date understanding of the subject.

Because of the critical importance of the evidence favorable to continental drift the assumptions and techniques employed in these studies have undergone rigorous appraisal by workers in the field. Although the assumptions have not been entirely proved, they have been clearly defined; furthermore, the requirements of the techniques, from sampling through measurement and experiment to statistical analysis, are now of high standard. Paleomagnetic evidence today commands serious attention and Irving's book carefully and systematically demonstrates why.

Following a brief introduction to the magnetic properties of rocks, a subject treated fully in Nagata's book, *Rock Magnetism*, chapters are devoted to the general features of the geomagnetic field, and to techniques of sampling, statistical analysis and presentation of data.

Chapter five, on "The Reliability of Paleomagnetic Observations," vividly illustrates the uncertainties inherent in Natural Remanent Magnetism, but also how these can often be clarified and undesirable components eliminated. It is in this field, the refinement of paleomagnetic data, that some of the greatest progress has been made in recent years. This chapter, in particular, should be read by all to whom the implications of paleomagnetic data are of concern.

Then follows a review of all paleomag-

netic data available up to the end of 1963, and an evaluation in their light of hypotheses regarding the past history of the earth's field.

Chapters seven and eight in turn consider reversals of magnetization and changes in intensity of the earth's field.

Chapter nine, of more general interest, compares paleomagnetic latitudes with latitudes inferred from paleoclimatic indicators. The correlation in favor of paleomagnetic latitudes rather than present day latitudes is impressive.

Chapter ten, by means of case histories, gives an interesting illustration of the diverse and important applications of paleomagnetism in various branches of geology. The specific application to paleogeographic reconstruction is included here.

A comprehensive reference list of paleomagnetic results and an exhaustive up-to-date bibliography are included in the Appendix.

Profusely illustrated, the volume is most attractive in its makeup and binding, accounting, perhaps, for the cost of $19.50.

The book constitutes a review of all the underlying principles, techniques developed, and results which have been obtained in paleomagnetism to date. It is appropriate that the current status of paleomagnetism should be documented in detail, and the author is to be congratulated on doing it so well.—*Robert Hargraves*

Reference Groups (Exploration into Conformity & Deviation of Adolescents) by M. SHERIF & C. W. SHERIF; 370 pages; $6; Harper & Row, 1964.

In this 370-page volume, Sherif and Sherif attempt the systematic study of adolescents in the informal groups of their voluntary association. They seek to understand "*who* the people are whose appraisals count for the individual, *what* defines a deed as conforming or deviating, and *why* the individual does or does not conform to it" (p. 4). Their study focuses on adolescent groups from lower-, middle-, and more upper-class neighborhoods. It is refreshing to note that the authors assume and empirically

find that all such groups of adolescents, regardless of their particular neighborhood, function in accordance with similar principles of group formation and change. The pejorative term, "gang," typically used with reference to adolescent groups in lower-class neighborhoods, is seen to be, in fact, only a term which conceals the basic similarities of group functioning and purpose which cross neighborhood and class boundaries. "Their values and goals earmark them all as youth exposed to the American ideology of success and wanting the tangible symbols of that success" (p. 199). It is also refreshing to note the general orientation which the authors attempt to bring to this work: "Our attempt in this project has been to place the individual in the context of the small group that counts in his scheme of things. The attempt has also been to place the small group in question within the context of its physical and social setting, including the formal groups and institutions in it" (p. 41). This orientation is best exemplified in their statement that, "In order to understand why the boys steal cars . . . the following information is surely pertinent: the enormous success of the automobile industry in this country; the constant reminders on radio and TV that everyone needs a car . . . ; the system of streets and highways; the mobility and many activities a boy can have with a car . . . " (p. 76).

For the most part, their specific method of research involves observation and interview, with the administration of some questionnaires. Of their many conclusions, those most significant to this reviewer are embodied in the following quote and in its implications: " . . . the importance of group formations in socially undesirable behaviors by youth almost precludes great importance to individual pathology as a causative factor. Being a responsible, reliable member of a group, who can be counted upon by others even in secret and dangerous activities, is simply not possible for any period of time if one is severely disturbed emotionally, subject to acute anxieties, depression, persecution, or other such symptoms" (p. 279).

This reviewer finds it unfortunate that, in spite of the aims of this study and its general orientation, the product given to us to read is only mediocre in its overall scope and poor in certain specifics. The book reads more like a preliminary report of an early stage of work than a more polished, final product. It is excessively repetitive and could be edited to less than half its size without damage. One likes to assume that there must be a richness of data, given the samples studied and the methods of observation, yet the book leaves one with the feeling that this "clinical" richness has not been tapped. Only the barest minimum of the surface of these groups has been examined, at least in this report of their work.

This reviewer does not enjoy harping at methodological issues; however, the questions of observer reliability, more sophisticated statistical analyses of the data, and other such matters are for the most part ignored. This can be forgiven in part, but only if the authors can provide us with a rich resource pool of data, or possibly with a sophisticated, tightly knit theoretical framework. As has already been indicated, the richness of data is absent. We are given only one chapter containing anything on the order of a descriptive protocol of these groups. The work itself is almost totally lacking in any theoretical framework other than of the most general, loose, "hunchy" nature. All of this leads this reviewer to wonder why this work has appeared prematurely in book form. It is better seen as a preliminary report of an interesting, potentially profitable psychological study.—*Edward E. Sampson*

Organic Semiconductors by Y. OKAMOTO & W. BRENNER; 184 pages; $9; Reinhold Publishing Corporation, 1964.

The field of organic semiconductors or conduction in organic solids has grown rapidly in the last few years and a good review might be appropriate. This book does not fill the bill, however, as the title seems to suggest. As the authors point out in the preface, they have

limited their discussion to an account of research on the dark-conductivity characteristics of organic solids. This was a particularly unfortunate choice since this aspect of the work on organic solids has been relatively unproductive.

The book consists essentially of a compilation of measurements of the conductivity of various kinds of organic solids. There are chapters on monomeric organic compounds, charge transfer complexes, polymers, and biological systems and a chapter contributed by A. F. Armington on electronic carbons. Essentially all experimental results are reported with no obvious attempt to select or emphasize the most significant results.

The second chapter entitled "Electronic Conduction Mechanism in Organic Materials" is surprisingly inadequate. It consists essentially of an extremely elementary discussion of conductivity in semiconductors. There is a chapter on measurement techniques and the final chapter is an attempt by the authors to predict what is in store in the future for organic semiconductors.

The book would probably be useful to an organic chemist who is attempting to prepare organic solids which exhibit unusual electrical properties and this is presumably the audience to whom the authors were writing. I would not recommend the book to anyone interested in understanding the electronic properties of organic solids or in investigating the status of the field.—*R. G. Kepler*

Elements of Cytology by N. S. COHN; 368 pages; $8.95; Harcourt, Brace & World, 1964.

The field of cytology keeps expanding beyond its classical and neoclassical confines as any healthy active branch of science should. As a result, there is felt by some teachers of the subject and by most commercial publishers a need for the impossible: a modern comprehensive text of the calibre of Wilson's *Cell in Development and Heredity*. While the volume reviewed here has apparently satisfied a publisher, it is not likely to please very many serious teachers of the subjects nor their students.

Aside from a reasonable organization of the material into chapters, an abundance of additional references and a reasonably lucid and instructive chapter on chromosomal aberrations, there is little to recommend this book. The work is so heavily burdened by mistakes in fact and obscure or meaningless statements that the nonexpert student is likely to be confused if not misled.

As specific examples of the latter, the potential user of the book as a text for his students might look at Chapter 2 on methodology, particularly at the discussion of various microscopic techniques, at the discussion of the Feulgen reaction on pages 160 and 161 (also at the top of page 147 where it is equated with basic staining), and at the verbal and diagrammatic descriptions of meiosis in Chapter 12. In this chapter, the discussion of crossing over mechanisms is particularly disturbing both because of its incompleteness and its confusion.

In this reviewer's opinion, Cohn's *Elements of Cytology* does not well serve its intended purpose as "a comprehensive balanced survey of the field of cytology for the undergraduate or graduate student . . . "; nor is it likely that even a more critical and sophisticated treatment of the subject matter by a single author can provide as good a background as is available through a judicious selection of authoritative monographs on the different topics which constitute the field of cytology.—*W. Plaut*

Metal-Ammonia Solutions, Physiocochemical Properties, edited by G. Lepoutre & M. J. Sienko; 315 pages; $10.50; W. A. Benjamin, Inc., 1964.

While many excellent review articles have been written concerning the complex metal-ammonia system, this book has to rank as the current, prime reference source.

It consists of a series of papers presented at the Weyl Colloquium in Lille, France, in June 1963 to celebrate the centenary of Weyl's discovery of the solubility of alkali metals in liquid ammonia. This colloquium listed numerous specialists in the physiochemical properties of metal-ammonia solution as participants.

Beginning with a foreword by Professor Charles A. Kraus, a pioneer investigator of this system, many excellent articles are presented and the discussions held after each paper are also printed. These discussions are very interesting and elucidating.

The most notable articles are those of the following: Dr. R. Catterall, the mechanical properties of metal-ammonia solutions; Professor K. S. Pitzer, the nuclear and electron resonance spectra of metal-ammonia solutions; and an excellent theoretical article by Professors J. Jortner, S. Rice, and E. G. Wilson, on the theories and models of electron binding in solution.

Anyone who has ever performed investigations on metal-ammonia systems will consider this book a requisite for his bookshelf. Indeed, anyone who is interested in a specialized and fascinating field of chemistry, will find many enjoyable hours of fruitful learning in this book.—*William H. Brendley*

Polyonimoes (The Fascinating New Recreation in Mathematics) by S. W. Golomb; 182 pages; $5.95; Charles Scribner's Sons, 1965.

The work "polyomino" is an extension of the word domino. Just as dominoes are plane geometrical figures, composed of two equal sized squares joined along a common edge, so polyominoes are plane geometrical figures composed of *groups* of squares of equal size, all joined along common edges. A single square is called a monomino, three squares are called a tromino, four squares a tetromino, and so on. Any of these plane figures can be referred to as a polyomino.

This book, written by a professor of mathematics at the University of Southern California, considers the problem of arranging a specific group of polyominoes to form certain rectangles, or to cover specific parts of an 8 by 8 array of squares, such as, for example, a checkerboard. (Calling it a checkerboard almost gives away the answer to one of the first problems Dr. Golomb

considers—namely, can 31 dominoes be used to cover all the squares of a checkerboard except the two at opposite ends of one of the main diagonals. The answer is "No," and a beautifully simple argument based on the color of the squares is given in this book.)

The range of problems considered is very large indeed. One chapter is concerned with the construction of square arrays, with a given set of polyominoes. Another chapter is concerned with a proof that certain configurations can not be formed using certain polyominoes. The author goes into detail in all of his proofs, and all of them seem to be very convincing. He digresses from polyominoes in Chapter 5, which is concerned with various theorems of combinatorial analysis, but this digression is pardonable in such a thorough presentation. Later chapters are concerned with bigger polyominoes, solid polyominoes, and other generalization of polyominoes. The subtitle of this book is "The Fascinating New Recreation in Mathematics." In spite of the recreational implications, there is a set of exercises in one place, and a problem compendium in another. These make the book read much more like a text book than a recreational book. But, in either case, the book is a good one.—*M. H. Greenblatt*

Principles & Applications in Aquatic Microbiology, edited by H. HEUKELEKIAN & N. C. DONDERO; 452 pages; $10; John Wiley & Sons, 1964.

This book, consisting of papers presented at the 1963 Rudolfs Research Conference held at Rutgers University, is a hodgepodge, but an interesting one. While nominally concerned with aquatic microbiology, the range of chapters is great: physiology of photosynthetic bacteria, protists, rumen organisms, and iron and manganese bacteria; the persistence of pesticides in soil; succinct reviews of the *Sphaerotilus-Leptothrix* and *Arthrobacter* genera and of actinomycetes; hydrocarbon utilization by microorganisms; various aspects of water and waste treatment; and deterioration of plastics and fibers in a marine environment.

The major criticism of the book would be that many of the subjects have been dealt with recently in more extended form. For instance, Ehrlich has a chapter on microbial transformation of minerals and he has an article in the most recent volume of "Advances in Applied Microbiology" on the same subject. Similarly, Mulder's chapters on *Sphaerotilus-Leptothrix* and *Arthrobacter* appeared in recent issues of "Antonie van Leeuwenhoek" and "Annales de l'Institut Pasteur," respectively. I can understand the value of discussing them publicly at a meeting, but I don't feel that they need be published more than once. Some chapters did present new material, among the more interesting of which were Starkey's on transformation of organic sulfur compounds and Baars' on the use of infiltration ponds to purify polluted water.

In summary, the book does illustrate the variety of problems which involve the microbiology of aqueous environments, but I wonder whether the specialist will find enough new material here to make it worth while for individual purchase. The individual who wishes an introduction to the area will in many instances find this book a useful point of departure.—*Richard I. Mateles*

Archimedes in the Middle Ages, Vol. I—*The Arabo-Latin Tradition* by M. CLAGETT; 720 pages; $12; The University of Wisconsin Press, 1964.

The character, the problems both actual and historical, and the romance of the medieval history of the western end of the Old World—comprising the areas where Greek, Arabic, and Latin were the learned languages—have all in large measure been generated from the peculiar circumstance that the Middle Ages were at the same time dominated by the recovery and transference of ancient learning, and almost committed to some sort of originality by the new conditions of society and outlook. The immensely complicated problem of the literary transference of knowledge from one part to another of this medieval world, through translations and education, has been the subject of almost a century of systematic scholarship. For equally as long, scholars have also broached the even more complicated problem of the motives, opportunities, and actual achievements of the medieval peoples using Arabic and Latin as their learned languages in their use of ancient scientific knowledge. In both kinds of problem, those of recovery and those of use, the difficulties for the historian are the most intractable when dealing with an author such as Aristotle, relevant to almost every aspect of thought from theology to mechanics, or a subject such as the growth of experimental science involving a wide range of intellectual and practical habits from those of the academic philosopher to those of the unlettered technologist. Just as Galileo achieved scientific success by carefully limiting his problems to those of the measurement of motion, so the historian may do the same by the choice of problems readily soluble by available historical material. Mr. Clagett in this volume has solved once and for all a set of very important historical problems, and when the second volume of this study is published he will, if not actually have removed the question of Archimedes in the Middle Ages from the realm of historical research, at any rate have gone a long way towards having done so.

Archimedes ranks with Newton as a generator and model of scientific inquiry. How did his writings survive? Modern texts of Archimedes are based for the most part on three Byzantine Greek manuscripts. The first contained all his extant works except ON FLOATING BODIES and ON THE METHOD and some smaller items. This is the source of all Renaissance copies of Archimedes, but it is now lost. The second contained the mechanical works, including ON FLOATING BODIES, and is also lost. Both of these manuscripts were available to the 13th-century Flemish translator, William of Moerbeke. The third Byzantine manuscript was identified by Heiberg in 1906 in Constantinople, and added to our knowledge of Archimedes's writings the marvellous work ON THE METHOD which had been unknown since ancient times. No doubt because of this seem-

ingly strange lack of Greek manuscripts of so famous a scientific author, Arabic texts of Archimedes are patchy. The main treatises which they knew are ON THE SPHERE AND THE CYLINDER, THE MEASUREMENT OF THE CIRCLE, a fragment of ON FLOATING BODIES, and some indirect material from ON THE EQUILIBRIUM OF PLANES in addition to some smaller items. One important difference between the reception of Archimedes in the Arabic and the Latin medieval worlds was that in the former his techniques were mastered in a way in which in the latter they were not. This reflects a contrast in the mastery of mathematics which is one of the main differences between medieval Arabic and Latin science.

Mr. Clagett's book is in content mainly a collection of texts with English translations and commentaries. It is the product of many years of study of manuscripts in European libraries and he believes that he has overlooked no major text. He has published elsewhere the results of his researches into the transmission, to the Latin west, of knowledge of one of Archimedes's two main themes, mechanics. The present volume deals with the transmission, through Arabic mainly, of knowledge of the other main theme, mathematics. The principal works of Archimedes that came to the Latin world through the Arabic route were the MEASUREMENT OF THE CIRCLE and ON THE SPHERE AND THE CYLINDER. Texts of these and of Arabic works based on them as well as on some of Archimedes's other mathematical writings form the most important items in this volume. Latin translations made in the 12th century introduced Western mathematicians to such important problems as those of finding two mean proportionals between two given quantities and the Greek method of "exhaustion." In the striking leap forward in mathematical competence that becomes apparent in the 13th century, Mr. Clagett has shown that these 12th century translations of Archimedes played their part side by side with the well-known influence of those of Euclid. Outstanding examples of the new mathematical sophistication of the 13th century can be

seen in the work of Gerard of Brussels, Leonardo Pisano and Jordanus de Nemore.

Mr. Clagett has not only produced a fundamental work of reference for scholars mainly concerned with textual problems. He has provided the only possible means of answering a number of essential historical questions to which he addresses himself. Direct knowledge of Archimedes's texts came first through the Arabic route but later through the fuller translations made from the Greek by William of Moerbeke in 1269. The second volume of this book will deal with these versions from the Greek, and when published will make possible a detailed comparison between the two traditions. Besides these direct sources, knowledge of Archimedean problems and methods also became available to the West through various indirect sources: these were mainly concerned with statics and hydrostatics. The most important historical problem concerns the use made of these new sources of mathematical and physical knowledge. Mr. Clagett provides evidence for the popularity of Archimedes from the wide range of references made to him in the 13th, 14th, and 15th centuries as well as from the use made of his methods by such eminent mathematicians as Bradwardine, Albert of Saxony and Nicole Oresme. But, surprisingly, the incomplete Arabic tradition was not only the more thoroughly mastered but remained better known even after 1269. The Moerbeke translation was known at least in 14th century Paris, although its survival in only three known manuscripts (one complete and two in part) indicates a rather restricted popularity, but the Greek tradition really did not come into its own until the 16th century. Then, the translations from Arabic of Archimedes, like those of Galen, Hippocrates, Aristotle, and other scientific and medical writers, were consciously displaced by translations made from the Greek. But the first translations of Archimedes to be published in print were in fact those by Moerbeke, which were known among others to Leonardo da Vinci, and were pilfered by Tartaglia for his edition. Of the other two principal 16th century translators of Archimedes, there is evidence that Commandino may have known the Moerbeke translation, and that Maurolico knew both the Moerbeke translation and the 12th century translation from Arabic by Gerard of Cremona of at least one important work of Archimedes. Mr. Clagett's book at last makes it possible to discuss the influence of Archimedes on the growth of science in the Middle Ages with real knowledge. Scholars will look forward eargerly to the completion of his book with the second volume.—*Alistair Crombie*

Principles of Fluid Mechanics by W. H. Li & S. H. Lam; 374 pages; $9.75; Addison-Wesley, 1964.

This is an unusual introductory text, likely to find enthusiastic reception in some quarters and dismayed rejection in others.

Its scope is accurately indicated by the headings of 17 chapters of roughly equal length: Introduction, Dimensional Analysis, Fluid Statics, Kinematics of Fluids, Dynamics of Frictionless Incompressible Flow, Irrotational Flow, Streamlines and Stream Functions, Vorticity, The Momentum Theorem, Flow with Gravity, Flow of Viscous Fluids, Two-Dimensional Laminar Boundary Layers, Turbulent Flow, Thermodynamics and Fluid Flows, One-Dimensional Steady Compressible Flow, Shock Waves and Expansion Fans, Similarity Laws in Compressible Flows.

The level of presentation and discussion is quite sophisticated, and obvious care is taken to insure that qualitative statements of cause and effect, as well as formal derivations, are accurate and up-to-date. The pace of the first twelve chapters is unhurried, important derivations are given with full manipulative details, and mathematical aids such as vectors and Cartesian tensors are introduced with exceptional clarity and ease. The next four chapters seemed to me to be not so uniformly polished, but the last chapter is again excellent.

Two striking features of the book, which may stimulate its mixed reception, are the following:

1. Only 7 references to published literature are given, thus providing only a feeble hint as to past sources and future directions of the theory.

2. Only two photographs of actual fluid flows, and one example of experimental confirmation of theory are included.

For confidence in the theory, the (presumably beginning) student has recourse only to his appreciation of logical and mathematical consistency, and his faith in the authors' grasp of physical reality.

These features would certainly weaken the book as a source for self-study, but, in a regular college course, they might be regarded as virtues by the professor, in whose hands the authors leave those aspects of the subject which are better exhibited in nature than in print, and to whom is reserved the pleasant task of suggesting the sense of challenge and fascination which fluid motions have provoked in past generations and with which they beckon to the future scholar, scientist, or engineer.—*Frederick S. Sherman*

Orchids of the Western Great Lakes Region by F. W. CASE, JR.; 148 pages; $7; Cranbrook Institute of Science (Bloomfield Hills, Michigan), Bulletin 48, 1964.

This book is intended for the amateur field botanist who will find the accurate but nontechnical descriptions, the illustrated keys, and especially the photographs showing each species in its natural setting helpful in identifying orchids in the field. The author's familiarity with each of the 52 different kinds of orchids that he describes and illustrates, gained from 20 years of experience with them, is clearly demonstrated by his lucid, informative, and accurate accounts of their habitat and geographical distribution. The distribution maps show graphically not only the geographic range but also the relative rarity or abundance of each species. Some species, as *Malaxis paludosa*, are in general excessively rare, perhaps because they are also exceedingly inconspicuous, whereas others appear rare

only because they barely enter the region considered, such as *Tipularia discolor* of the southeastern U.S. and *Cypripedium passerinum*, an arctic species very recently discovered on the north shore of Lake Superior, and whose description as the 53rd species was added to this book while in press. The chapter on growing native orchids will appeal to conservation-minded people, and it appears at a particularly appropriate time, when interest in native plant gardens is rapidly growing.—*William C. Steere*

Order-Disorder Phenomena by H. S. GREEN & C. A. HURST; 363 pages; $15; John Wiley & Sons, Interscience, 1964. Vol. V of *Monographs in Statistical Physics* edited by I. PRIGOGINE.

This is a thorough and well written account of the methods used in solving the two-dimensional Ising model. The emphasis is on the method of Pfafians which the authors have themselves introduced into the subject, but other methods including the original one of Onsager are included. The book is to be highly recommended not only to physicists who would like to become acquainted with the applications of Pfafians to certain problems in statistical mechanics but also to mathematicians who are usually quite unaware of this complex of beautiful and deep combinatorial problems.—*M. Kac*

Analysis, Vol. I by E. HILLE; 626 pages; $10; Ginn & Co., Blaisdell Publishing Co., 1964.

This text is the first of two volumes directed to accompany college students through an undergraduate calculus curriculum. Material generally presented as Calculus I is presented with more sophistication than may usually be expected in an early undergraduate program. Yet the author's style of presentation is particularly pleasant, and not in the least reminiscent of the often dry and staccato writing so characteristic of calculus texts. By treating the requisite mathematics, Professor Hille does not omit the historical context of the subject, and thus discloses insights into the development of serious and successful mathematical thinking. More than 2000 problems help the student illuminate the points in the text. Solutions to odd-numbered problems are included in the book.

Hille prefaces the calculus with the language and precepts of set theory and the principle of induction. The notions of equality, inequality, vectors, limits, and functions complete the introductory material. The reader's attention is then channeled into the mechanics of differentiation, integration, infinite series, and an ample number of applications of these topics. Volume II will examine functions of several variables. Thus, topics such as partial differentiation and multiple integrals will appear in that volume.

This reviewer is pleased to characterize Professor Hille's work as thorough, stimulating, and capable of developing mature mathematical thought and practical ability early in the student's training. The text is a very creditable addition to the mathematical literature.—*R. W. Wyndrum, Jr.*

Tobacco Alkaloids & Related Compounds, edited by U. S. VON EULER; 346 pages; $15; The Macmillan Co., 1964.

These Proceedings of the Fourth International Symposium held at the Wenner-Gren Center, Stockholm, February 1964, afford a wide-ranging survey of the physiological, biochemical, and psychological effects of the tobacco alkaloids and certain related compounds upon mammals. An index of the magnitude of the problem may be obtained from the Report of the Advisory Committee to the Surgeon General of the (U.S.) Public Health Service entitled "Smoking and Health." It seems that the intake of nicotine alkaloid from cigarette smoke alone by United States citizens amounts to between 800 and 1000 metric tons yearly. This is equivalent to a little more than 6 grams per year per person aged 15 years or over, or approximately 150 times the single

(oral) dose reported to be lethal for a man.

Such figures do not tell the complete story. The Wenner-Gren Symposium volume is a remarkable and fascinating account of the ability of the mammalian body to absorb, distribute, metabolize and eliminate nicotine and its degradation products without suffering serious disturbance of normal functions. In general, the conclusions of the symposium seem to coincide with the conclusions of the Surgeon General's Committee. These are, "... the chronic toxicity of nicotine ... is very low and does not represent a significant health problem."

There are 26 technical papers included in this volume classified as medical usage 1, chemistry 2, smoke 1, metabolism 3, distribution in body organs 1, physiological effects 14, and behavior effects 4. Certain of these stand out for their clarity and general

interest. Schmiterlöw and Hansson used whole-body radioautography to demonstrate an accumulation of intravenously injected nicotine-C^{14} within a very short time in the brain. Other sites of significant accumulation were the adrenal medulla, the superior cervical ganglion, blood vessel walls, the gastric mucosa, and the kidneys. The kidney is stated to be the main excretory pathway of nicotine and its metabolites.

The beautiful work of McKennis on nicotine degradation in the mammalian body is summarized. Hansson and Schmiterlöw extend the general approach taken by McKennis to specific body organs wherein they note differences in metabolic products. Bovet, Bovet-Nitti, and Domino present interesting evidence that nicotine may facilitate certain types of visual discrimination and learning in rats. There are four papers on aminophenol release in mammals stimulated by nicotine, three papers on circulatory effects, three on the nervous system, and one on the gastric secretion response to nicotine. The symposium closes with a provocative view of the role of nicotine in smoking pleasure by Ejrup.

Despite the fact that nicotine is not now officially a health hazard of consequence, the sponsors of and the participants in this Symposium have performed a notable service for the educated reader by bringing together into a single volume so many brief but informative papers on a subject of such perennial interest.—*R. F. Dawson*

Man & Nature Or, Physical Geography As Modified by Human Action by G. P. MARSH (1864), edited by D. LOWENTHAL; 472 pages; $7.95; Harvard University Press (Belknap), A John Harvard Library Book, 1964.

To review a book published a century ago would seem to be a task of supererogation, except to comment on the editing of its reprint. And indeed this editing, as a John Harvard Library Book, deserves more than casual mention. For David Lowenthal, author of GEORGE PERKINS MARSH: VERSATILE VERMONTER, is thoroughly familiar with the life and works of Marsh.

He has chosen to edit the first edition of *Man and Nature*, complete with footnotes and his own bracketed comments. These latter include significant changes made in the several later editions issued during Marsh's lifetime as well as references to recent scientific literature that illuminates the text. The result is a document that should be in the hands of geographer, ecologist, historian, and land-use planner, as well as the citizen with a thoughtful concern for the future.

The new edition of this great classic comes at a time of sharp division between those who talk of the control of nature and those concerned to control human behavior in the light of our experience with the operation of nature. We have solved two ancient problems—the mass production of goods and of knowledge, but are far from adjusting ourselves to this new facility.

Marsh's thesis, it will be recalled, is that man has become a major natural force, often altering environment to his own great disadvantage. The evidence supporting this idea was accumulated by a scholar and linguist, widely read and travelled, a firm believer in the potential of applied science, and a man whose experience in business had led him to respect the practical.

Two quotations, from the first and last chapters, respectively, will serve to show the effect of his inquiry: "We are, even now, breaking up the floor and wainscoting and doors and window frames of our dwelling, for fuel to warm our bodies and seethe our pottage, and the world cannot afford to wait till the slow and sure progress of exact science has taught it a better economy."

And again: "The collection of phenomena must precede the analysis of them, and every new fact, illustrative of the action and reaction between humanity and the material world around it, is a step toward the determination of the great question, whether man is of nature or above her."

With his advantage of an added century of accumulated knowledge, today's reader will profit by his scrutiny of the discussion, both the prescient and the tentative, of forest influences, hydrology, and geological process. He need be

warned only that this is a book to be savored and not a roadside sign to be absorbed in a passing flash.—*Paul B. Sears*

Frameworks for Dating Fossil Man by K. OAKLEY; 355 pages; $8.75; Aldine Publishing Co., 1964.

Dr. Oakley's book, first conceived in 1958, was shelved in 1961 because of the author's ill health, and was completed last year. In it he reviews and evaluates the considerable amount of research that has been conducted during recent years in the closely allied fields of Pleistocene geology (Part I) and Palaeolithic/Mesolithic archaeology (Part II) in the Old World. Each of these parts is followed by an exceedingly useful list of references to works cited in the text and for additional reading. In an appendix, all the human fossil remains are listed with the dates of discovery, stratigraphical and archaeological datings, and absolute ages where known. Such a book has long been needed and this one is well done.

In his introduction the author defines his two main classes of dating: *relative* and *chronometric*, neither of which calls for special comment. It is with his notion that the *archaeological evidence* can be used for determining the relative ages of Pleistocene deposits that the present reviewer takes issue. Dr. Oakley (p. 9) considers that this approach "has some advantage over the paleontological," and he goes on to state "The method of dating based on the spread of early human cultures is really an extension of paleontological dating, for early industries (assemblages of artifacts) may be regarded as fossilized patterns of behavior which changed ('evolved') at varying rates and which were acquired and transmitted by tradition." Now the fundamental fallacy underlying this viewpoint is that it fails to distinguish between biological or natural factors, on the one hand, and cultural processes, on the other. Furthermore, the cultural

assemblages in question have in most instances been relatively dated on the basis of geological (or paleontological) evidence; therefore, it is illogical to turn around and apply these results to establishing the age of a given human fossil or of a geological deposit. In point of fact, however, this "method" is not often used, as a glance at the fossil hominid dating tables at the end of the book (pp. 291–334) will reveal.

Part I, entitled "Stratigraphical Dating," is extremely well organized and clearly expressed. Herein, the author has presented the salient facts to meet the needs of archaeologists wishing to learn the stratigraphical frameworks now applied to Pleistocene sequences on a regional basis. But in his Part II, intended to do the same for the geologist who wants to learn the terminology of Palaeolithic and Mesolithic cultures, Dr. Oakley exposes himself to a certain amount of criticism. In the first place, the Central European materials are scarcely mentioned; secondly, the Near Eastern succession, instead of being treated as a regional sequence in its own right, is considered as an appendage of Europe; and thirdly, no mention is made of the numerous and very prolific Palaeolithic sites in the territories of the U.S.S.R., notwithstanding the fact that several comprehensive Russian monographs have been translated into Western languages. Chapters 4 and 5 on Africa, as well as 6 and 7 on Southern and Eastern Asia, constitute valuable and very useful summaries. Certainly they tend to counterbalance the several shortcomings manifest elsewhere in the archaeological section.

This book is certainly not superficial, and one would hope for an expanded edition in the future. In the meantime, the present version meets a very great need in the anthropological literature. This reviewer heartily congratulates Dr. Oakley on his successful achievement. The publication of this book places all who are required to teach the subject of Early Man in the Old World eternally in his debt.—*Hallam L. Movius, Jr.*

Plant Growth & Development by A. C. Leopold; 466 pages; $12.50; McGraw-Hill Book Company, 1964.

Experimental botany has made remarkable strides in the last two decades. This book is a competent and welcome summary, at a level suited for advanced undergraduates and beginning graduate students, of selected aspects of that progress. While it breaks no new ground, intellectually or organizationally, it will serve as a useful supplement to existing textbooks of elementary plant physiology in updating and extending information, and in directing and stimulating the interest of the student.

It begins, as so many recent textbooks in experimental botany do, with an exposition of the fine structure of the plant cell and its organelles, and the function of each organelle in biochemical and developmental terms. Part I of the book concerns *Assimilation*, under the chapter headings of Photosynthesis, Organic Translocation, Inorganic Translocation, and Mobilization. The last brief chapter is relatively novel in texts of this type, and nicely introduces the concept of hormone-directed metabolic events.

Part II deals with *Growth*, under the chapter headings Auxins, Gibberellins, Kinins, Inhibitors, and Differential Growth. Again the last chapter represents some innovation in organization, serving to introduce such concepts as polarity, organ regeneration, differentiation, tropisms, and control of abscission.

Part III, entitled *Development*, begins with interesting chapters on Juvenility and Senescence, then proceeds to discussions of Flowering, Flower Physiology, Fruit Set, Fruit Growth, Fruit Ripening, Tuber and Bulb Formation, and Dormancy. The treatment in these chapters is straightforward and thorough. Part IV, *Environmental Physiology*, considers Light, Radiation (other than visible light), Temperature, and Water. The book closes with a chapter (Part V) entitled Applications of Chemicals to Plants.

In each chapter, the author uses a narrative style which increases readability. This is further enhanced by the

double column format and frequent graphs, line drawings, and tables, which provide pleasant punctuation of the text; also, their placement and content are convenient and informative. The frequent reference to names of investigators and the dates of their publications hampers the narrative pace, but will increase the familiarity of advanced students with the names and temporal context of modern plant physiologists. The author and subject indices, and the detailed citations of references at the end of chapters further enhance this beneficial feature of the book.

All in all, Professor Leopold has displayed broad and balanced knowledge of his subject, good taste in selection of topics and supporting data, and good sense in organization. He and the publishers are to be congratulated on producing an aesthetically pleasing, as well as an eminently useful book. To non-plant physiologists, I would remark that the book continues the recent (since 1952) practice of de-emphasizing water relations, which used to constitute more than half of all the subject matter in many text books. This "desiccation," paradoxically, produces a much "juicier" product. Also, unlike the older books, which crept up on the subject through atoms, solutions, and colloids, it plunges boldly into the subject, and weaves the assumed chemistry and physics skilfully into the narrative. As a teacher who has previously used this direct approach, both in books and in the classroom, I applaud the skillful manner in which Professor Leopold has carried off his latest writing venture.—*Arthur W. Galston*

Macromolecular Structure of Ribonucleic Acids by A. S. SPIRIN; Translation Editor, J. A. STEKOL; 210 pages; $10; Reinhold Publishing Corporation, 1964.

Two monographs, previously published in Russian, comprise the book, *Macromolecular Structure of Ribonucleic Acids*, by A. S. Spirin. The work summarizes the large body of data that

has accumulated in this field and attempts to correlate macrostructure with biological function. An extensive bibliography accompanies each monograph.

The first monograph deals with the molecular weight and solution conformation of virus, ribosomal, soluble, and messenger RNA. The use of various hydrodynamic and optical tools for the physicochemical characterization of RNA is well illustrated. The second monograph, which is initially repetitious of the first (even identical pictures appear in the two parts), contains a detailed description of work on the structure of ribosomes. With the structural data as background, the biological role of the ribonucleic acids is discussed.

The book gives a well-documented and up-to-date, review of the pioneering work of Dr. Spirin and colleagues, who have been concerned with ribosomes and ribosomal RNA. However, considering for instance s-RNA, the discussion of which is already scanty, one is left with a picture as was prevalent many biochemical years ago (1960–62), and this period has been fairly well documented in other reviews, even by Dr. Spirin. To some degree a time lag is unavoidable. However, in the light of data available in the summer of 1964 (when the manuscript was last edited), a more critical analysis of previous work, and the insertion of current concepts, should have been possible.

The bibliography serves as a good summary of Russian work in this field, although it is by no means exhaustive in this respect.

The monographs are clearly written; however, the translation is often literal. It is apparent that the translator is not versed in the physical and polymer terminology of nucleic acid biochemistry.

The book is expensive. In such a field, where "facts" are being continually revised, and books rapidly become only historical documents, less expensive (≪\$10) editions (paperbacks) are necessary.

In spite of these shortcomings, the volume gives a concise summary of the steps which have led to our understanding of ribonucleic acids, and thus can serve as a good introduction to this area of research.—*David Henley*

Progress in Inorganic Chemistry, edited by F. A. Cotton; Vol. 6; 350 pages; \$14; John Wiley and Sons, Interscience Publishers, 1964.

The sixth volume of this series continues to provide critical, high-quality reviews of important topics in the expanding field of inorganic chemistry. Of the four chapters in the present volume, that by B. N. Figgis and J. Lewis on The Magnetic Properties of Transition Metal Complexes is by far the most extensive—203 pages and 670 references. The authors survey modern theory of magnetic properties of transition metal ions, including effects of ligand fields, spin-orbit coupling and anisotropy, and then critically evaluate published magnetic data for a wide variety of transition metal compounds. The latter section is particularly valuable for its interpretative coverage of the less-familiar 4d and 5d elements.

The other three chapters cover more restricted topics equally well. G. W. A. Fowles briefly (39 pages) reviews Reaction of Metal Halides with Ammonia and Aliphatic Amines, covering mainly solvolysis and addition reactions of halides of representative group IV(B) elements and of the transition metals of the 4th, 5th, and 6th groups. J. H. Holloway, in Reactions of the Noble Gases, succinctly (29 pages) summarizes the status of the exciting compounds of the one-time "inert gases" from their discovery in mid-1962 up to early 1964, mainly the fluorides, oxides and aqueous chemistry of xenon. In the final chapter, The Coordination Model for Non-Aqueous Solvent Behavior, R. S. Drago and K. F. Purcell present a substantial collection of evidence supporting a general applicability of the coordination model for nonaqueous solvents, emphasizing the donor and solvation characteristics instead of the autoionization process of the older, solvent system model. Such a "progress" article is particularly valuable in these

circumstances. (However, ϵ in the abscissa of Fig. 7 is not dielectric constant, as labelled, but rather an absorption coefficient related to ion association.)

This series should be included in the reading material of all inorganic chemists and many in related areas as well.—*John D. Corbett*

Handbook of Applied Hydrology, A Compendium of Water-resources Technology, Ven Te Chow, Editor-in-chief; 1467 pages; $39.50; McGraw-Hill Book Co., 1964.

Never before has so much hydrological information been placed in a single volume. The editor has been successful in enlisting contributions from 45 authors, most of whom are well established as authorities in the general field of hydrology. Multiple authorship does present problems with regard to continuity, level of presentation, and organization; however, the editor has done a remarkable job in minimizing these distractions.

Almost by definition, if not intent, a handbook is apt to be out-of-date for teaching and research purposes at its time of publication. In contrast, the *Handbook of Applied Hydrology* has many chapters which appear to be of a more lasting quality. Some of the stronger sections of the handbook include the 4-part section (97 pages) on Statistical and Probability Analysis of Hydrologic Data, the 5-part section (124 pages) on the Hydrology of Flow Control, the section (55 pages) on Groundwater, the section (54 pages) on Runoff, and many others. Reference in most of the sections is extensive and up-to-date on important topics. Handbooks are not to be used for digging deeply into a problem of a research nature, but this one can be used for

initiating literature searches without fear of major omissions.

A few weaknesses are evident. Some of the initial sections such as Oceanography, Meteorology, and Geology are much too brief. Little mention is made of fluvial morphology. Water pollution and water supply problems could stand more extensive coverage. Culverts and associated design problems are hardly mentioned. Several sections are textbook condensations with little attempt made to show applications. The role of hydraulic and hydrologic experimental models is not adequately covered. No doubt many of these deficiencies occurred as a result of attempts to keep the size of the book within reason—an end not actually obtained.

Because of its price, the *Handbook of Applied Hydrology*, will be slow in reaching the bookshelves of individual students and scientists, but library copies will see considerable use during the next decade. In summary, the book is a real contribution to the field and should be well received by hydrologists throughout the world.—*Lucien M. Brush, Jr.*

Leisure in America: A Social Inquiry by M. KAPLAN; 350 pages; $3.45; John Wiley & Sons, 1964, paper.

Part of the trouble with this book is its subject, leisure being such a huge, amorphous and overdiscussed topic, especially the leisure of Americans. It is in fact one of those topics in the process of depletion by social scientists and professional social critics. For this reason, saying anything new about it requires either new research or unusual brilliance and originality. True enough, the author warns in the preface that "neither its tone, nor its conclusions [of the book] are startling." And so it is. Besides the lack of substantive originality—particularly noticeable in a book of this length—little is gained by a merging of a popular-social-scientific approach with the social-philosophizing one. It is probably the result of this popularizing proclivity that the book all too often indulges in stating the obvious. For example: "Ultimately the difference between philosophy, biology, psychology, chemistry, history, anthropology, and sociology is that, although all may be interested in one or another aspect of man, each emphasizes a particular kind of problem. Each formulates its own approach" (pp. 14–15). Or: "The burden of our analysis has been that the tendencies, values, and interpretations of American life are so diverse and multidirectional that the last consideration—goals taken by our leadership—must consciously and strategically isolate the assets about us and build upon them with realistic faith" (italicized, p. 300). One more example: "The purpose of the analysis that follows is to free us of self-made prisons of thought, easy definitions, and gratuitous exhortations. From such freedom we may explore issues of leisure unfettered by the need to defend, uphold, argue, or plea" (p. 13). Actually the author does take a stand. He is an egalitarian optimist in the American tradition—closely following the positions taken by Riesman and Shils—who believes that the fate of leisure in America is not as bleak as most social critics, haunted by the vision of mass culture, think. He is critical of the implicitly aristocratic standards these critics use. Addressing himself to the problem of leisure at large there is no other central position or thread but the aforementioned. Rather, this is a survey of the available literature on leisure fortified by a very strong awareness of all ramifications and problems of leisure and its interdependence with everything under the sun. This almost encyclopedic orientation becomes one of the weaknesses of the book, and the effort to take up every possible aspect and interrelationship of leisure and the social-physical environment leads to a degree of superficiality. Examining leisure from different angles in turn leads to repetition also expressed in some of the chapter headings (e.g., Ch. 4, Leisure and personality; Ch. 19, Personality and social roles in leisure).

At the same time this "encyclopedic" approach makes it possible for the author to raise all important problems that pertain to leisure in America and he brings to bear on his discussion much

of the relevant literature. Mr. Kaplan made a valiant effort to survey a difficult, overworked and—in its entirety—almost intractable topic and succeeded in writing what might be called a useful introductory text.—*Paul Hollander*

Gene Action by P. E. HARTMAN & S. R. SUSKIND; 158 pages; $4.95 cloth; $2.95 paper; Prentice-Hall, Inc., 1965.

This book made enjoyable and refreshing reading. The chemical basis of genetics and various biological implications are presented in straightforward and understandable prose. The authors have succeeded in giving a clear, up-to-date picture of a rapidly developing field.

As part of the Foundations of Modern Genetics Series, *Gene Action* begins with an account of base pairing and other properties of DNA. Four of the chapters deal with the structure and synthesis of proteins, mutant proteins, and complementation. A chapter on secondary consequences of gene mutations covers a variety of significant physiological phenomena. Messenger synthesis, ribosomes, and regulatory processes are discussed next, followed by a thoughtful treatment of the genetic code. In the last chapter, Perspectives and Horizons, gene action is considered in relation to growth, development, and evolution.

The sequence of topics flows naturally and is pleasing. There are numerous well-chosen illustrations. Each chapter ends with a list of references to reviews, books, or experimental papers, and with a set of very useful questions. A nine-page index is provided.

Gene Action is warmly recommended to all interested in molecular biology. This volume should prove valuable in undergraduate as well as graduate instruction. Research workers will discover a number of intriguing nuggets. In the words of the editors of the series, the reader "will find here the seed of more than one enigma." It is to the credit of the authors that the puzzles, the speculations, and the firm results all

emerge in an informative and stimulating manner.—*Henry J. Vogel*

Multivariate Statistical Analysis for Biologists by H. SEAL; 207 pages; $7.75; John Wiley & Sons, 1965.

This book is primarily an expository treatment of advanced statistical methods that are based on the multivariate normal distribution. The reader is introduced to determinants, vectors, and matrices in the first chapter and to additional material on these topics in later chapters as the need arises. This technique may serve to keep the mathematically unsophisticated reader from being overwhelmed by unfamiliar material, but if the reader wishes to refresh his memory it may take considerable time to locate the particular result sought. This reviewer is of the opinion that readers who have not had a previous introduction to matrix algebra will tend to become discouraged and those who have already had an introduction would find the work more convenient to use if the mathematics needed to follow the development were summarized in one place. However, it must be admitted that, in the end, this is a matter of personal taste.

The presentation is moderately advanced in some ways. For example, matrix notation is used to present models and the formulas needed to perform computation, and variances and covariances of estimators of linear functions are derived using matrix notation. In other ways, the book is more elementary. The distributions of test statistics are not derived and many statements are made without proof. This is not intended as a criticism, for the scientists who are most likely to use the book can get along quite well without many of the proofs, and the author has taken some pains to explain the physical meaning of many of the procedures presented.

The book is divided into two main sections. Section A covers linear models when there is one dependent variable, and Section B introduces the reader to the analysis of several dependent variables. The topics covered in Section B include the p-variate linear model, principal components, canonical analysis and factor analysis; all are illustrated with examples drawn from the biological sciences. Appendices on useful computer routines, general block designs, and missing observations are included.

Examples are worked in detail in each chapter. The computations involved are shown at each step in a way which makes them easy to follow. The figures used to illustrate the text are well drawn and are invariably helpful.—*James E. Grizzle*

The Natural Geography of Plants by H. A. GLEASON and A. CRONQUIST; 420 pages; $10; Columbia University Press, 1964.

The Natural Geography of Plants is a unique book. For the first time, the facts and ideas of plant geographers are presented in an appealing form. It is written for travellers who enjoy the variety of landscapes in the United States, for the naturalist who has wondered about the distribution of Spanish moss, sagebrush, sugar maples, hemlocks, and the hundreds of other plant species that are our heritage.

I found it difficult to put the book down, because it has a compelling logic. From the basic premises of plant dispersal and requirements for survival, Dr. Gleason progresses to the present-day causes of plant distribution patterns. Much emphasis is given to climatic relations, succession and other aspects of plant ecology. The authors carefully avoid speculations about the sweeping events of the Tertiary, in spite of the considerable body of facts now available about the retreat of the tropical flora, the differentiation of deciduous forests, and the spread of desert vegetation. The primary concern is with the floristic makeup of present vegetation. The logic of Dr. Gleason leads the reader from the individual species distribution to the establishment of joint ranges, and the recognition of ten floristic provinces within the continental United States. The integrity of these provinces is not diminished by the fact that transition zones exist; the nature of relics and

advancing outposts is put in lucid perspective.

It is rare in modern day publishing to find a scientific work so well illustrated with full-page photographs of such rich texture and contrast. It was a disappointment to find that no references are listed to guide the interested reader further into the field of plant geography.

Dr. Gleason is to be congratulated for a clear summing up of the science of plant geography; Dr. Cronquist for his expert diagnosis of the ten floristic provinces—the capstone of the book, and for his care in producing an engaging and polished book.—*K. Lems*

Atlas of North American Astragalus by R. C. Barneby; 2 parts, 1188 pages; $35 (cloth), $30 (paper). Memoirs of the New York Botanical Garden, Vol. 13, available through Stechert-Hafner Service Agency, Inc., 1964.

Among the 500 to 600 genera of the legume family, *Astragalus* has by far the largest number of species, variously estimated at 1200 to 2000, and has long been known as taxonomically one of the most "difficult." Mr. Barneby, an amateur in the purest sense, has been brave, indeed, to commit himself to the task of revising even a portion of the genus.

His treatment of the group is sensible and practical. He places the number of North American species of *Astragalus* at 368, many with two or more varieties, in contrast to P. A. Rydberg's comparable monograph published in 1929 that included 564 species, with no varieties, assigned to 28 smaller segregate genera. In reviewing the work of his predecessors he takes a kindly and understanding attitude. He does not condemn nor underestimate their efforts but, rather, considers them in historical perspective.

The literary style of the introductory portion in Part I is clear and very readable. The "Systematic Treatment" includes a synopsis of the North American species, grouped in seven "phalanxes." Adequate descriptions are provided for all recognized taxa. There are some 17

pages of keys for identification, conveniently arranged by geographic regions, and a series of 163 maps to show geographic distribution.

There are no other illustrations except that each volume has a beautiful half tone frontispiece depicting one species of *Astragalus*, the drawings presumably the work of the author. One wishes that there could have been many more.

Barneby's treatment is primarily taxonomic but he summarizes the current, incomplete, morphological and cytological knowledge of *Astragalus*. His Atlas should form an excellent basis for future work on the cytogenetics and chemotaxonomy of the genus.

The general format of the two volumes is pleasing. A few inevitable typographical errors can be found. There are two somewhat minor inconveniences. One is that to find the author's name and the literature citation of a given taxon, one must search the fine print; I should prefer that the authority, at least, be shown in the heading. The other is that, in the index, page numbers are not given for synonyms; instead, one is referred to the so-called "correct" name, an unnecessary delay.

The fact that Barneby has made detailed field surveys as well as careful herbarium investigations, and, as he says, the genus *Astragalus* has been a favorite subject of study and contemplation for more than 20 years, has resulted in a monograph that could well set a standard to be emulated by his professional colleagues.—*Velva E. Rudd*

Mathematics: Its Content, Methods, and Meaning, edited by A. D. ALEKSANDROV, *et al.*, translated by S. GOULD, *et al.;* Vol. I, 358 pages; Vol. II, 376 pages; Vol. III, 356 pages; boxed $30; M.I.T. Press.

A three volume work, translated from the Russian under the auspices of the American Mathematical Society, is designed to give the serious reader a comprehensive view of mathematics, past and present, with some indication of future direction. The high level of readability is attested to by the large number of nonmathematicians who are enthusiastically working their way through the set.

The enormous scope is indicated by the twenty chapter headings:

A General View of Mathematics, Analysis, Analytic Geometry, Algebra: Theory of Algebraic Equations, Ordinary Differential Equations, Partial Differential Equations, Curves and Surfaces, Calculus of Variations, Functions of a Complex Variable, Prime Numbers, Theory of Probability, Approximations of Functions, Approximation Methods and Computing Techniques, Electronic Computing Machines, Theory of Functions of a Real Variable, Linear Algebra, Non-Euclidean Geometry, Topology, Functional Analysis, Groups and Other Algebraic Systems.

Of course there are important areas which have been omitted e.g., statistics, information theory, linear programming, and combinatorial analysis, but digestion of the material presented would make acquaintance with untouched areas much easier.

Some unevenness, perhaps the result of writing and rewriting, is occasionally exhibited. Thus, e.g., the section entitled "Representations and Characters of Groups" is five pages long and has only two sentences on group characters.

The high quality of exposition, the unparalleled breadth, and the emphasis on concept rather than proof for proof's sake should do much to raise the level of mathematical literacy everywhere.—*Roger S. Pinkham*

La Science Contemporaine, les sciences physiques et leurs applications, edited by L. LEPRINCE-RINGUET; Vol. 1, 360 pages, 1964; Vol. 2, 360 pages, 1965; no price; Librairie Larousse, Paris.

This quarto-size, two-volume set, in French, is intended for the intelligent layman interested in learning of modern physical science. There are about two dozen long articles on as many topics, written by French scientists and engineers. The topics range from such things as relativity and physics of waves, through satellites and radio astronomy,

to computers and electronics. Mathematics is employed where it is appropriate. There are many photographs and well-planned line drawings, with extensive use of color. This reviewer wishes that such handsome and interesting works had been available for reading practice at the time he was trying to learn technical French.—*W. J. Cunningham*

Foundations of Thermodynamics by P. Fong; 94 pages; $2.50; Oxford University Press, 1963.

The Second Law, An Introduction to Classical and Statistical Thermodynamics by H. A. Bent; 429 pages; $6; Oxford University Press, 1965.

These two books deal with somewhat the same subject matter and at somewhat the same introductory level, but do so from quite different viewpoints.

The little book by Fong is a monograph devoted largely to a development of the second law of thermodynamics. It does so, starting with known experimental facts relating to spontaneous changes of thermal, mechanical, and compositional types. A mathematical argument is used to define a potential function which increases during spontaneous changes, and which is additive for parts of a composite system. After this potential function is obtained, it is shown to be equivalent to the quantity entropy, historically found through a different line of reasoning. There is a final chapter on the microscopic interpretation of thermodynamics.

The longer book by Bent also develops the second law in a logical manner, but starts with different examples and follows a different and less mathematical line of reasoning. It covers a wide range of material, slanted toward applications in chemistry. Each short chapter appears to contain the lecture of one day in a college course. Many of the chapters are followed by selections from scientists of the past, or interesting comments about them, e.g., Joseph Black on melting ice, Planck on Clausius's hypothesis, Carnot on motive power of heat, an obituary for Maxwell's demon. A feature is the large number of

problems of widely diverse sorts, with solutions. Problems and solutions occupy almost as much space as the formal text itself.

Both these books should be of interest to anyone teaching or learning basic thermodynamics, and each is appealing for a different reason.—*W. J. Cunningham*

The Primates by S. EIMERL & I. DEVORE & THE EDITORS OF LIFE; 200 pages; about $5; Life Nature Library, Time Incorporated, 1965.

One of the latest additions to "Life Nature Library," written by a professional writer, an anthropologist, and the editors of *Life*, attempts the overwhelmingly difficult task of presenting an accurate, comprehensive, and also popularly written account of man's closest relatives. The attempt is not wholly successful, for it is difficult to summarize the vast literature on the Primates concisely, accurately, and clearly for any but fellow researchers.

There are several features of the book which make it a useful and reasonably good popularization of an important scientific subject. The book has some of the best photographs of primates this reviewer has ever seen. These photographs were collected not only from *Life's* own superb staff photographers, but also from a large number of investigators who are today studying primates in their natural habitats. Many of the excellent marginal illustrations serve to enhance the text and explain quickly some of the general and theoretical points. For example, the hands of various primates are imaginatively presented in the positions they assume when grasping objects.

The book begins with a definition of the Order Primates as a whole and describes briefly, with superb illustrations, each major group within the Order. The text and pictures are presented in approximately the sequence in which it is believed each of the major groups of living primates became a separate evolutionary lineage. Somewhat more than half of the book is devoted to an account of the behavior of the more social of the primates. There is considerable emphasis upon recent work on communication among the primates. There is also an extended discussion of the ways in which group integration is achieved in various species. Since one of the authors is noted for his work with baboons, it is not surprising that a large portion of this material on behavior is devoted to these charming and social, if not entirely sociable, primates. There are summary accounts of current research on the psychology and general biology of the Primates; there is a survey of the transition from ape to man; and there is a good introduction to the study of the expression of emotions among nonhuman primates, including some delightful marginal illustrations of expressions of emotions in chimpanzees. All too little is said about the desperate necessity of instituting conservation measures if our fellow primates are to survive for more than another generation or two. The book has an adequate bibliography and an index. The text is generally informative but the authors have not achieved the delightful balance between the conveying of complex information and the clarity of style that characterizes the best scientific popularizations.—*John Buettner-Janusch*

The Life of the Rainbow Lizard by V. A. HARRIS; 174 pages; 21s; Hutchinson & Co., Ltd., London, 1964.

Notice of this monograph seems desirable for it is a welcome addition to the several attempts in recent years by too few herpetologists to provide what is in each author's judgment a comprehensive account of the biology of a species of reptile. The present work is the result of six years of observations, most of which were made on *Agama agama* living on the grounds of the University of Ibadan in Nigeria. Also included are observations on *Agama* populations living under different ecological conditions in Lagos and Zaria.

Identification of individual lizards was based on gular pattern recorded by photographs and color coding with paint. According to Harris the gular pattern appears to be as specific to his lizards

as the fingerprint is to man. Most of this study is concerned with behavior, but there is information on thermal relations in a chapter entitled "A Place in the Sun" and structure of the integument is discussed under "A Coat of Many Colours." There is a short chapter on natural populations. Such chapter headings give some indication of the tone of this readable account. The author's style, which he says may appear anthropomorphic, is undoubtedly aimed at wider circulation. It is sure to be offensive to some.

It is not my purpose in this brief note to discuss the controversial subject of territoriality which occupies much of this work or to point out the areas where there is a lack of information on the life history of *Agama*. The latter will be obvious to those that search for them. However, it should be mentioned that the monograph is not without error. A paper by Charles M. Bogert presumably (1949, *Evolution 3* (3) 195–211) is cited erroneously on page 53 and is not to be found among the list of references. With regard to the review of the literature, the paper of A. J. Marshall and R. Hook (1960), *Proc. Zool. Soc. Lond.*, *134*, 197–205 on reproduction of *Agama* seems pertinent but is not among the list of references.

Agama agama is a lizard of more than casual interest to students of reptiles, and Harris presents much information of value. The attractive format, construction, and price of this monograph are a credit to the publishers.—*J. P. Kennedy*

Human Tumours Secreting Catecholamines by H. Hermann & R. Mornex, translated by R. Crawford; 207 pages; $8.50; Pergamon Press, The Macmillan Company, 1964.

In recent years, there have been many advances in our knowledge of catecholamine-secreting tumors. Thus, a modern perspective on the subject would be quite useful. Unfortunately, the present book falls short of this goal. The book is a review of 507 cases of pheochromocytoma (only 13 being the authors') and is mainly descriptive and documentative rather than synthetic

and interpretive. The title is misleading in that exclusive concern is with pheochromocytoma; the interesting secretory aspects of neuroblastomas are mentioned only in one sentence and ganglioneuroma not at all. The presentation *per se* is often curiously oblique even allowing for translation from the French (a favorite word is "semeiology").

The book is organized into four main sections entitled: Clinical Features, Diagnosis and Treatment, Anatomical and Biological Investigations, and Physiopathology. Errors of omission and misinterpretation are most obvious in the second section. The authors favor bioassay of urinary catecholamines (spinal dog) for routine diagnosis and offer an inadequate and downgrading summary of the status of chemical diagnosis. No chemical structures are shown and routes of catecholamine metabolism as indicated in their figure 23 (a "synoptic table") are confusing. Description of medical management in relation to surgery is not sufficiently detailed and the recent approaches of adrenolytic treatment preoperatively and volume replacement to obviate routine use of pressors post-operatively is not dealt with sufficiently. Medical management of malignant cases is not mentioned. In the third section attempts to relate catecholamine contents to size of tumors and type of hypertension (paroxysmal vs. sustained) is of interest. The discussion of physiopathology is superficial.

While the book is not a scholarly treatise it is an excellent reference source and deals with many of the unusual aspects of an interesting disease. It is of value to one with special interests in pheochromocytoma but is not recommended as an authoritative text for those who have only a casual knowledge of the subject.—*Albert Sjoerdsma*

Flame Spectroscopy, Parts I & III by R. Mavrodineanu & Part II by H. Boiteux; 721 pages; $42, boxed; John Wiley & Sons, 1965.

The title is somewhat misleading since this is a treatise on analytical spectroscopy with particular reference to the spectra emitted by various elements in acetylene—air and acetylene—oxygen flames. The book is divided into three parts. Part I (215 pages) consists of an historical introduction and 14 chapters devoted to the discussion of general flame properties and to the design and operation of flame burners for analytical spectroscopy. Part II (348 pages) consists of 8 chapters which describe the theory of the production of spectra and the spectra of the chemical species involved in acetylene flames. Part III consists of reproductions of the spectra excited in acetylene flames by various elements, a wavelength table, and a bibliography.

The standard of writing is variable; where the authors restrict themselves to analytical spectroscopy the book is excellent. Parts II and III are good. Part I suffers from superfluous sections and diagrams—for example, sectional diagrams of pressure regulators and rotameters in Chapter 4, and the diagrams of a liquid-liquid continuous extractor and a still for high purity water belong to a general laboratory manual and not to a book such as this one. The color photographs of flames are only of general interest and could have been omitted advantageously.

It is to be regretted that the manuscript appears to have been written in 1960–1962 and the references in some chapters are somewhat old. However, in the chapters on analytical spectroscopy additional references up to 1964 are added in blocks at the end of the relevant chapter but these are not referred to in the text. As might be expected, the English in this book is sometimes rather strange. In particular, the use of the terms "combustible" and "combustive" for the much clearer "fuel" and "oxidant" is to be regretted.

This book can be recommended as a handbook on analytical spectroscopy with many valuable tables of data. It would be very useful to anyone wishing to start from scratch in analytical spectroscopy without any previous knowledge. The more general sections could have been shortened and the colour photographs omitted, thus reducing the price.—*Graham S. Pearson*

Blood & Bone Marrow Cell Culture by H. J. Woodliff; 141 pages; $6; J. B. Lippincott Co., 1964.

The author has provided a rather accurate summary of his monograph at the beginning of the final chapter: "The review of the literature on blood and bone marrow cell cultures presented in the preceding chapters reveals that *in vitro* studies of these cells have provided useful information about some aspects of normal and pathological haemopoiesis. For the amount of work that has gone into such studies, however, the positive results appear rather meagre. This is due to the limitations of present technical methods. The potential value of cell culture methods is great, and recent advances along several lines suggest they will be used more frequently in the future."

The monograph appears to be intended for hematologists who may be interested in the techniques of cell culture and their applications to hematological problems. As the author points out, these applications have as yet been of limited value. Emphasis is placed primarily on the results of descriptive studies using the light microscope. The older literature is covered quite thoroughly, and the results are analyzed critically.

The author is mainly interested in such possible applications of cell cultures as their use in the diagnosis and classification of leukemia, assessment of drugs used in the therapy of leukemia, assay of hemopoietic stimulating hormones, and culture of various types of normal blood cells for use in replacement therapy. In the opinion of the reviewer, the greatest successes of the application of techniques of blood and marrow cell culture have not been in these fields, but in other areas, such as cytogenetics. The author mentions the cytogenetic work briefly in a section devoted to the action of phytohemagglutinin on blood cell cultures, but does not discuss the results obtained.

This book would be a useful addition to a general medical library, as a reference work for those interested in the history and present status of techniques

of blood cell culture. It is not of particular value to those interested in the applications of these techniques in fields other than hematology.—*J. E. Till*

Nuclear Interactions by S. DEBENEDETTI; 636 pages; $16; John Wiley & Sons, 1964.

• Present-day graduate students in physics are in an unenviable position compared to their predecessors. A generation ago, it was still possible for graduate students to understand the basic ideas discussed in most of the papers published in *The Physical Review*. Today, however, this is seldom possible. Even in one's own field, one often cannot make much sense of a paper without a considerable expenditure of time and effort. Because of this lamentable state of affairs, today no physicist can truthfully claim to be an expert in all phases of nuclear physics. Accordingly, it is an occasion of great rejoicing when a book appears which will help to bridge the ever-widening gap between graduate physics courses and research work. Professor DeBenedetti's exemplary textbook is such a work.

As this book takes its readers to the very forefront of our knowledge of nuclear physics, it will be necessary for them to have had courses in classical mechanics (Goldstein), electromagnetic theory (Stratton), mathematical physics (Morse and Feshbach), nuclear physics (Leighton), and quantum mechanics (Merzbacher). The outstanding virtue of this book is that it is a labor of love. The author actually explains the subject. Authors often expound—a profound and important difference. No idea is introduced without the accompanying physical motivation and its fruitfulness developed in considerable detail. All mathematical steps are given in most instances, and there are no "It can easily be shown that . . . " and "It is left as an exercise for the reader to show that . . . " to frustrate the reader. Numerous applications of the theory are given, and the bibliographies are excellent, detailed, and modern.

In Chapter 1, the general properties of nuclear forces are discussed. It begins with a discussion of the conservation laws and symmetries of space-time and ends with a discussion of the two-body problem. In Chapter 2, nuclei containing three or more nucleons and the many-body problem are discussed. The various models of the nucleus and the specific features and shortcomings of each model are developed in detail. As our knowledge of the nucleus comes primarily from scattering experiments, Chapter 3 is devoted to scattering phenomena of all kinds—neutron, electron and nucleon scattering and polarization effects. Chapter 4 is devoted to interactions between nucleons and radiation, a fairly well-understood process. Chapter 5 on nuclear reactions is a modern version of the treatment found in Blatt and Weisskopf. It must be admitted that our ignorance in this regime is still considerable. Chapter 6 treats the relativitistic interaction between fermions and radiation. A very clear and concise discussion of Feynman diagrams as propagators is given, which even experimentalists should have no trouble understanding. The treatment is rather incomplete, for there is no mention of Wick's theorem and the subtleties connected with the S-matrix approach. For such matters, the reader must consult Schweber or Bjorken and Drell. Pion physics is the subject of Chapter 7, and considerable experimental data are given along with a fine physically motivated discussion. The book ends with a readable treatment of weak interactions.

This is a remarkable book and deserves the serious attention of physicists. It will undoubtedly be widely adopted and used as a textbook and as a reference. The typography is good, and there are relatively few misprints. Physicists are indebted to Professor DeBenedetti for taking time out to write such an excellent book.—*Howard Chang*

Cosmic Rays by B. ROSSI; 268 pages; $5.95 cloth, $2.95 paper; McGraw-Hill Book Co., 1964.

Studies of the cosmic radiation have been immensely fruitful for twentieth century physics. Prior to development

of high-energy particle accelerators, cosmic ray phenomena constituted the sole clue to the realms of subnuclear processes lying beyond those hinted by radioactivity. The history of the use of cosmic rays as a tool for prying into those secrets has now been retold in fascinating form by an investigator who has himself been intimately involved in that history for several decades.

Rossi's book can be enthusiastically recommended to a broad range of readers. Specialists in the field of cosmic radiation should find his narrative exciting. At the other extreme, even undergraduate science students will find this an outstanding (the outstanding?) account of the intriguing area of cosmic radiation. Historians of science must not fail to take notice of Rossi's book, for it gives a deeply discerning treatment of a field rich in implications for modern science history and studded with surprising twists and turns of interplay between theory and observation.

From the heroic age of cosmic ray research, beginning with the 1912 balloon ascent of Victor Hess, and moving on after the war years to the energetic field work of Millikan (who coined the slightly unfortunate name "cosmic rays" because he felt sure for many disputatious years that these were extra-energetic gamma rays) and Compton (whose observations were instrumental in revealing that the "rays" were in fact charged particles), Rossi traces expertly and engagingly the steps by which two generations of diligent physicists sorted out the almost unfairly confusing array of events that comprise the atmospheric interactions of the cosmic radiation. Clarifications ran neck and neck with new confusions through the thirties; but after World War II attacks on many fronts nearly cleared the field within ten years. Today, the great remaining question is: where and how are the cosmic ray primaries formed?

Rossi, in a final epilogue, suggests that future historians of science may "close the chapter on cosmic rays with the fiftieth anniversary of Hess's discovery." That may or may not be what

the future holds; but Rossi's retelling of those fifty years of cosmic ray research will probably be the definitive historical treatment for years to come.—*James E. McDonald*

X-Ray Optics & X-Ray Microanalysis, edited by H. H. PATTEE, *et al.;* 622 pages; $22; Academic Press, 1964. Proceedings of the 3rd International Symposium, Stanford University, California, August 1962.

The publication of Symposium proceedings is now a widely accepted procedure in spite of serious objections that can be raised against this practice. Many communications offered to the public through this medium would not survive the reviewer's scrutiny if submitted to a reputable scientific journal. Usually, the tutorial papers contributed by authoritative invited speakers are repetitions of material already published elsewhere, with minor alterations, if any. If important original papers are published in Symposium proceedings, the information presented will probably appear later in a slightly different format in a scientific journal.

The proceedings of the Stanford Symposium on X-Ray Optics and X-Ray Microanalysis demonstrate that shortcomings observed in many publications of this kind can be avoided. Most of the papers in this volume are communications of significant original work. The presentations are clear and readable, and careful editing is apparent. A review of the literature published since the Stanford meeting indicates that most authors did not think it necessary to re-publish their work through other channels.

The Stanford symposium is the third of a series of international meetings organized by V. E. Cosslett, A. Engström, and H. H. Pattee. To the bewilderment of the librarian, these editors have chosen to change the titles of their successive symposia and of the respective proceedings. The first of the series (Cambridge, 1956) was called "X-Ray Microscopy and Microradiography," and the second (Stockholm, 1959) "X-Ray Microscopy and X-Ray Microanalysis." The fourth symposium, to be held in Paris, in September 1965, will be entitled "X-Ray Optics and Microanalysis." This change of emphasis from microradiography to microanalysis (mainly electron probe microanalysis) is reflected in the contents of the successive proceedings, as pointed out by V. E. Cosslett in the first paper of the current volume. Of 52 papers, 12 deal with x-ray physics and general aspects, 12 with diverse x-ray absorption techniques, mostly applied to biological specimens, 25 papers concern microprobe analysis, while 3 deal with microfluorescence techniques. Some areas of applied x-ray physics such as x-ray microradiography are by now well-established techniques, while, in reflection x-ray optics and microscopy, there is little hope for practical advance in the near future. The electron probe, however, has become a powerful tool of wide application; furthermore, the theory of quantitative electron probe microanalysis was, at the time of the Stanford meeting, in a state of evolution.

Under these circumstances, one would expect that obsolescence would quickly affect the significance of publications concerning quantitative probe analysis, particularly in view of the fact that the Proceedings appeared 15 months after the Symposium. Curiously, in spite of further developments in this field, most of the communications in this category, such as those of Duncumb and Shields, of Archard and Mulvey, of Poole and Thomas, of Green, and particularly that of Philibert, have, if anything, gained in perspective. The appearance of these papers has greatly stimulated the analysis of the electron impact and x-ray production in solid targets. It is probable that the procedures for quantitative analysis recommended by the above mentioned authors will be further implemented, modified, and adapted to digital computing routines. The reviewer expects that the papers contained in this book will be of permanent value to the scientist interested in quantitative x-ray spectrography.—*K. F. J. Heinrich*

Elementary Coordination Chemistry by M. M. JONES; 473 pages; $18.60; Prentice-Hall, Inc., 1965.

A good many chemists will appreciate the labors of Mark M. Jones in drawing together in this book so many loose ends of the vast and diffuse field of coordination chemistry. His scope is very broad, covering coordination compounds of the various elements, the nature of their bonding, determination of structures and of stability constants, thermochemistry, synthetic methods, polymerization, isomerism, electron exchange, hydrolysis, application to catalysis, and biological significance. To these topics he brings a sense of historical development, a wealth of references, and several valuable tabulations of experimental results. The result is a useful guide to those in the field who would broaden their perspective, as well as to those who want an introduction to any of its varied aspects.

The book's title suggests that it is also intended for use as a textbook. Indeed a number of exercises are included at the end of each chapter and methods and equations are generally developed in greater detail than would be necessary for a reference work. For teaching purposes, however, the book is rather amorphous, a quality inherent in its breadth of coverage. On the other hand, one is left with the feeling that the space devoted to procedural details might better have been given over to more critical evaluation of the approaches presented.

Inevitably, for such a wide range of material in a field developing so rapidly, some of the information is out-of-date. For example, mercury (II), discussed as one of the two ions whose hydrolysis products are *not* polynuclear (p. 210) has recently been removed from that category (Ahlberg, *Acta Chem. Scand.*, *16*, 887 (1962).

These are minor faults. As a most useful introduction and guide to the field, the book belongs on the shelves of all with an interest in coordination compounds and their properties.—*Thomas G. Spiro*

The Current Interpretation of Wave Mechanics: A Critical Study by L. de Broglie; 96 pages; $6; American Elsevier Publishing Co., 1964.

This slim volume summarizes in a semipopular fashion de Broglie's continuing criticism of the Copenhagen interpretation of quantum mechanics and outlines the alternative interpretation proposed by de Broglie, the theory of the double solution. Basic to de Broglie's view of science, though never developed in a critical way, is the idea that a truly adequate scientific explanation must enable the scientist to visualize or represent the physical processes that are actually occurring. The Copenhagen interpretation does not allow this in a critical way.

Observable particle localization is, as de Broglie sees it, the only window through which we can see the microphysical world. The double solution he developed in the twenties gave such localization a central conceptual role by picturing the electron as a little clock riding a guide wave in such a way as to be in synchrony with its frequency. The difficulties that led to the early demise of this view are now countered by introducing two adaptions derived from the work of Vigier and Bohm. The particle and guiding wave are now pictured as a singularity in a space-time tube in analogy with models used in general relativity, while sub-quantum perturbations are postulated which can knock the singularity from one wave to another in a random way. de Broglie is at his best in describing the behavior of individual particles, in some difficulty when attempting to elaborate the relationship between actual measurements and the mathematics that systematize them, and in deep difficulty when trying to explain the behavior of an assembly of particles in terms of real physical waves rather than configuration space wave functions.

In the past, some enthusiasts have projected a maximal interpretation of the Copenhagen position using it in a quasi-philosophical way as the ultimate criterion of knowledge and reality. The criticisms of de Broglie and others have certainly helped to deflate this. But, in my view, these criticisms do not seriously affect the minimal Copenhagen interpretation that guides the working physicist. He simply accepts the fact that the type of visualization of the microphysical "*Ding an sich*" de Broglie desires can not be developed in a critical self-consistent way and consoles himself with the fact that the presently accepted rules of interpretation yield a consistent, though perhaps incomplete, method of integrating the data known and knowable through an extension of present techniques.—*Edward MacKinnon*

Pharmacology of Conditioning, Learning, & Retention, edited by M. Ya. Mikel'son & V. G. Longo; 365 pages; $13; The Macmillan Co., Pergamon, 1965.

This volume, published jointly by Pergamon Press and the Czechoslovak Medical Press, is the first of a series of eleven, reporting the proceedings of the Second International Pharmacological Meeting, which was held in Prague in August 1963 under the auspices of the International Union of Physiological Sciences (SEPHAR). Since this is Volume One of the series, it includes the two brief opening addresses of the meeting by H. Rašková and C. F. Schmidt. The general editors of the series are H. Rašková and J. Vaněček. The book was printed in Czechoslovakia.

The papers presented here are from three half-day sessions, and the book is divided into two sections, entitled "General Problems and Electrophysiological Phenomena of Conditioning." The list of authors is truly international and is carefully balanced between Eastern European and Western sources. This means that the work of some Eastern workers is presented rather more comprehensively and perhaps more revealingly than is usually seen in the West. Of course, none of the reports is very new, since all are now no less than two years old, about par for a published symposium. However, the volume has merit in that it brings together under one cover a group of

summaries of a great diversity of work done in a single, but large and heterogeneous field. A drawback is that the members of the symposium were all pharmacologists and physiologists, so that some very interesting work being done by experimental psychologists is neither directly included nor well represented in references. There also are several informative approaches and techniques which are not represented in any of the papers in the volume, notably work with nucleic acids or inhibition of their synthesis, with spreading depression, and with sectioning of the corpus callosum and commissures.

The really outstanding opportunity in such meetings is for informal exchanges and discussions of a sort very difficult to capture in a formal publication. This book then is not unique in having rather desiccated transcripts of the discussions.

The principal criticism which might be leveled at much of the work reported here is the same as that which applies to the entire field of psychopharmacology, namely that it is too often phenomenological, lacking any unifying theories as background or even sometimes any clearly stated or properly formulated hypotheses. This might be defended on the grounds of caution, of the avoidance of drawing unfounded conclusions from insufficient evidence. The trouble is that even when theoretical discussions are offered, the congruence between them and the evidence is often highly doubtful. Indeed, the very objectives of some of the work are so obscure that one feels the need of an A. J. Carlson to stand up and ask, "Why did you do this?"—*Henry B. Murphree*

Fluvial Processes in Geomorphology by L. B. Leopold, *et al.*; 522 pages; $10; W. H. Freeman Co., 1964.

In conventional geomorphology textbooks, processes involved in the development of relief are discussed in a cursory and purely descriptive way, and most of the content is on the description of relief forms. In *Fluvial Processes*, the emphasis is on processes and how they

operate to produce relief forms. Quantitative data are used to draw empirical generalizations, which are analyzed, where possible, by theoretical approaches. The factors involved in the development of the earth's relief are so numerous, so complexly related, and so difficult to measure that no rigorous and systematic treatment of them is now possible; nevertheless, progress has been made in measuring and evaluating them, and a good account of the present state of this progress is given in the book. The approach used is decidedly more stimulating than the conventional descriptive treatment, and can be expected to continue substantially toward the improvement of teaching in geomorphology. On the important and controversial question as to whether or not the evolutionary history of relief can be inferred from its present properties, the authors have wisely taken the position that historical inferences must be made with great caution until more is known about the operation of present processes.

In the preface, the authors write that the book is primarily on landform development under processes associated with running water; and that subjects they have not themselves studied in the field or laboratory are omitted. Fortunately, their experience is broad, and some important processes not directly connected with running water (such as weathering and mass movement) are adequately treated. Instructors using the book as a text for the usual college course in geomorphology will probably find the two chapters on the river channel overly long and rather highly specialized. The use of mathematics and physical science in the analysis of processes is judicious and within the grasp of any well-trained student majoring in earth science. If the student has no background in hydrology, some of the graphs (flow duration curves, for example) will require additional explanation.

Many of the generalizations in the book are based on the relations between selected factors or properties, measured quantitatively and plotted graphically. In a textbook, this methodology should be accompanied by a section explaining the procedures and uncertainties. In particular, the student must be taught to give most careful attention to the classification of the data and the adequacy of the sample.

Geomorphologists owe the authors a great debt for synthesizing information from many papers and journals; and for virtually introducing, first in their professional work and now in this book, a new and fruitful approach to the subject.—*James C. Brice*

Nutrition—A Comprehensive Treatise, edited by G. H. BEATON & E. W. MCHENRY; Vol. II: *Vitamins, Nutrient Requirements & Food Selection;* 551 pages; $18.50; Academic Press, 1964.

This is the second volume of the three-volume series issued by the Academic Press on Nutrition. It has eight chapters dealing with the various aspects of nutrition mentioned above. It can be said at once that this is an excellent production, and the only criticism one can make applies to all treatises of this kind, that there seems to have been a lag period of at least two years between the time the articles were written and that of publication, since the latest references that could be found were to papers written in 1962. Drs. Dan and Søndergaard contribute the first chapter on fat-soluble vitamins and give a most interesting account of vitamins A, D, E and K. Dr. Grace Goldsmith gives a very full account of thiamine, riboflavin and niacin, historical, biochemical, and clinical. Dr. Chow deals with the other B vitamins, and Dr. Woodruff with ascorbic acid. The difficult question of dietary standards is discussed by Dr. Gordon Young. He compares the standards recommended by various authorities in different countries, about twelve in all, and discusses their philosophy and purpose. Dr. H. H. Mitchell contributes a chapter on a subject he has been interested in for years, nutritional adaptation. This is a timely topic, following on the section on nutritional requirements, and Dr. Mitchell points out at the end how difficult it is to arrive at dietary standards

in view of the body's well-known ability to adapt to different levels of intake. Drs. Scrimshaw and Béhar speak with long experience on the Causes and Prevention of Malnutrition, listing the causes under Agent Factors, Host Factors and Environmental Factors, and concluding with a discussion of General Preventive Measures. The final chapter, by Sir David Cuthbertson, is on Food Selection. He describes the, to us, extraordinary food habits of various peoples, and the food selection practised by western people under various conditions and in various environments. This chapter is a valuable summary of information not otherwise easily available.

Your reviewer can wholeheartedly recommend this book to those interested in nutrition, or whose work is in one of these fields.—*J. T. Irving*

Atlas

Photographic Lunar Atlas, edited by G. P. Kuiper; 230 unbound sheets of photographs plus 23 pages of description; $30 (out of print); University of Chicago Press, 1960.

First supplement to *Photographic Lunar Atlas: Orthographic Atlas of the Moon*, edited by G. P. Kuiper; 67 pages (plates); University of Arizona Press, 1960.

Second supplement: Rectified Lunar Atlas by E. A. Whitaker, *et al.;* 146 pages (30 plates); $35. University of Arizona Press, 1963.

Charts

Lunar Charts, Kuiper, *et al.*, advisors. Published by Aeronautical Chart and Information Center, United States Air Force, single charts published as completed. U. S. Government Printing Office, Washington 25, D.C., 50 cents each.

This review is an invitation to think about the features on the surface of the moon. The invitation is extended to scientists who may never have considered the moon before, for the subject of review is a body of readily usable, largely uninterpreted data.

Here is a consistent collection of raw materials from which thoughtful people

with varied backgrounds can build hypotheses. It is a rare opportunity to have access to a massive body of data which have been arranged into forms which are usable by nonspecialists.

These data start with some of the best lunar photographs taken with five major telescopes. Coordinates and nomenclature have been added, and foreshortening on the moon's spherical surface has been removed. This was done by projecting photographs onto a large sphere, and rephotographing at normal incidence to the sphere. At least three angles of illumination are presented in different photographs of each field.

Different illuminations often reveal different features in the same field, so relief charts have been drawn to compile much of what is on the photographs, as well as features which have been clarified by direct visual observation. Heights are shown, as derived from measurements of shadows. The amount of detail on these charts is a thing to behold.

These data appear in four different publications, with common editing or consulting by G. P. Kuiper and his associates. Funds and technical assistance were provided by agencies of the U.S. Air Force. The table attached to this review shows the form and quality of data in the different publications. A prospective interpreter of the moon needs all of these versions except the first supplement. If only one version can be obtained, it should be the second supplement.

A good way to start thinking is to compare one of the lunar relief charts with a terrestrial topographic chart on the same scale, such as a World Aeronautical Chart. This is particularly instructive if one chooses a WAC of a region of personal familiarity. Such comparison will quickly dampen the first flush of enthusiasm over using geological analogies, and will make clear how bold the lunar features are. Useful geological analogies must be accompanied with explanation of this difference in size of prominent features on the earth and moon. I have found this comparison of terrestrial and lunar charts to be highly exciting.

In making hypotheses on the relationships between lunar features, it is well to remember that a great deal of human energy has been expended on this activity. The theories which have not lasted have often been based on geological thinking. I expect that theories of shock hydrodynamics may give much insight into the large, photographable features on the moon, which are best described in morphological terms. Properties of clean solids in a vacuum are related to the very fine structure, which affect the physically measurable properties of heat conduction, radio emission and reflection, and light scattering.

At this time, the interpretations of physical measurements seem to contradict the interpretations of photographed morphology.—*David Cudaback*

	Atlas	1st Supplement	2nd Supplement	Charts
Scale	1:1,370,000	1:1,370,000	1:3,500,000	1:1,000,000
Nomenclature	Difficult to use	Fair	Good	Excellent
Nomenclature index	Difficult to use	None	None	None
Coordinates	None	Excellent	Awkward	Excellent
Contours and heights	None	None	None	Good
Visiblity				
Low ridges	Good	Poor	Good	Excellent
Craters	Good	Poor	Good	Excellent
Peaks	Excellent; if lucky, illum.	Poor	Good	Poor
Albedo variations	Excellent	Poor	Good	Very poor
Rectification	None	None	Good	Excellent
Whole moon views	Excellent	Good	Poor	None
Usability at visual telescopes	Excellent	Poor	Good	Poor

The Cellular Functions of Membrane Transport, edited by J. F. HOFFMAN; 291 pages; $6.95; Prentice-Hall, Inc., 1964.

This book contains papers presented in the 1963 Symposium of the Society of General Physiologists at the Marine Biological Laboratory, Woods Hole, Mass. The contributed papers are divided into four sections entitled, "General Aspects of Cellular Functions of Membrane Transport," "Role of the Membrane in the Regulation of Conduction and Contraction," "Role of the Membrane in the Regulation of Metabolic Processes," and "Role of the Membrane in Secretory Phenomena." The contributors to this volume have successfully presented a diversified approach to the study of biological transport by utilizing a wide range of biological material (microorganisms to mammals and higher plants). The authors have discussed their own research as it is related to work reported in the literature and numerous references are cited at the end of each paper. The editor should be congratulated for presenting a volume which emphasizes how membrane transport is related to and integrated with other cellular functions instead of simply defining and describing the occurrence of this phenomenon in various cells and tissues.

This book will be most helpful to the more advanced student and research investigator. The reader with little or no background to the subject matter may have some difficulty in following the interpretation of data and will undoubtedly benefit by reviewing some of the basic principles involved in biological transport before attempting to read this book. However, it is the opinion of this reviewer that the volume definitely contributes to a better understanding of the scope and importance of membrane transport.—*Robert G. Faust*

Wildlife Biology by R. F. DASMANN; 230 pages; $5.95; John Wiley & Sons, 1964.

This is a clear and well-written book in which some of the multiplicity

of factors affecting vertebrate wildlife populations are described. Basic ecological principles are briefly presented, and, in this framework, the aims, problems, and methods of the wildlife manager are discussed. A reader unaware of the complex interactions between wildlife populations and their environment will be enlightened and made to appreciate the difficult task of controlling these populations. Although the book is intended as a text for an undergraduate course in wildlife biology, the lay reader will find it interesting and completely comprehensible. It is short, profusely illustrated, and very readable. It is not sufficiently detailed or comprehensive to be considered as supplemental reading for a course in basic ecology, although an instructor in such a course may well find some of the examples and literature citations new and useful.

The book begins with some introductory remarks on the importance and history of wildlife management. The author proceeds to discuss ecosystems, biogeochemical cycles, energy flow, food chains and webs, ecological succession and its effect on wildlife, community stability and distribution, ecological niche, carrying capacity and some factors which affect it, basic characteristics of populations, migrations, home ranges, and territories. Some of the basic methods employed for determining the densities, movements, natality, mortality, and food habits of populations are then mentioned. The dynamics of wildlife populations are treated more extensively. A good, concise summary of present knowledge about population cycles is included.

The mutual benefits of careful management to both game populations and society are indicated. The author notes, for example, that there is an optimum density for populations, above which habitat destruction and low population productivity or health may result. Hunting, which can be effectively utilized as a mechanism to control population size may therefore satisfy the recreational needs of a large number of people and at the same time do a service to game populations. In the final chapter on land use, he warns that the human population in this country is in grave danger of soon surpassing its optimum density. "If we go on in our increase . . ., we will be faced with a wretched life in the future. If we can actually avoid shortages of food and other necessities, it will only be by submitting to an extreme regimentation of our lives and the sacrifice of the individual freedom which we now have."—*John J. Gilbert*

Mathematical Theory of Probability & Statistics by R. VON MISES. Edited and Complemented by H. GEIRINGER; 694 pages; $22; Academic Press, 1964.

This book is one which has been long awaited. The lecture notes upon which much of the material is based have been unavailable generally for some ten years. The editor has produced a scholarly work which reflects great care and patience. Editor and publisher have managed to leave very few typographical errors. The book, which contains a wealth of material, progresses leisurely, with illustrative examples being offered at each turn.

The recent re-emergence of interest in subjective versus objective probability and Bayesian versus non-Bayesian statistical philosophy makes timely the careful mathematical discussion of von Mises' formulation of probability theory and Bayesian limit theorems. There is continual contrasting with the more common Kolmogorov axiomatization.

It has been remarked that the von Mises theory starts at the most difficult point, and it is true that Chapter II requires careful thought; but it is also true that Cramér in his *Mathematical Methods of Statistics*, which is the most obvious book to compare with *Mathematical Theory of Probability and Statistics*, devotes some 75 pages to the Kolmogorov axiomatization and theory of Lebesgue integration.

By way of contrast, Cramér has a discussion of order statistics as well as a proof of Cochran's theorem on quadratic forms. Von Mises does not. On the other hand, von Mises has a careful discussion of the "moment problem" as well as general limit theorems on "statistical

functions" (functionals of the empiric cumulative distribution function). Cramér does not.

This book can be highly recommended to those desiring to learn the mathematics of analytic probability theory and statistics. It is unfortunate that the publisher felt it necessary to price it so high.—*Roger S. Pinkham*

The Origin & Evolution of Atmospheres & Oceans, edited by P. J. BRANCAZIO & A. G. W. CAMERON; 314 pages; $12.50; John Wiley & Sons, 1964.

Few books on planetary atmospheres have come out since the volume edited by Kuiper in 1952. The present collection of papers presented to a conference at the Goddard Institute for Space Studies therefore has particular interest.

The theme of the conference is faithfully adhered to, in that most of the sixteen papers elaborate on the arguments assembled by Rubey in 1951 supporting the motion that the earth's oceans and atmosphere originate in the effusions of volcanoes.

The papers on the terrestrial atmosphere tend to support Rubey's ideas. One of the most interesting is a study by Berkner and Marshall of the increase in oxygen content over geologic time. According to them, photosynthesis by primitive plants first raised the oxygen content to 1% of its present level, a critical value in that it would shield all but the top few centimeters of ocean from lethal ultraviolet, thereby permitting the large-scale growth of oceanic life. This epoch is associated with the Cambrian era, when life forms began to multiply rapidly. As marine photosynthesis raised the oxygen content to 10% of its present value, even the land was protected and land forms emerged. The present atmosphere is characterized, according to these authors, by an oscillation every 10^8 years between a surplus of CO_2 and of O_2.

Other papers are concerned with the Moon, Venus, Mars, Mercury, and the major planets. Gold suggests that the total water available at the surface of Venus is comparable to the amount in terrestrial oceans, but is largely in the

form of steam below the cloud layer. He further suggests that the Moon may have a deep layer of permafrost formed by its outgassing of water.

These suggestions, and the technical data needed to evaluate them, make this book a valuable one for the space scientist interested in planetary exploration.—*George B. Field*

Iconograph of the Characeae (A Revision of the Characeae, Vol. II) by R. D. Wood & K. Imahori; 812 pages; $35, Verlag von J. Cramer, 1964.

This book is the first published part of the most ambitious recent monograph on any group of algae. The iconograph would seem to satisfy the ambitions of its authors. It is essentially a set of drawings and descriptions of obtainable type specimens of all described taxa in the Characeae, with a few illustrations copied from other works where specimens are not extant. The drawings are at the same time a record of past work, a reference for future students, and a basis for the authors' revision of the group. With very few exceptions the illustrations are clear, uniform, well printed, and excellently done. They delineate the essential taxonomic features used in the separation of taxa in detail and at several magnifications. The habit drawings, which are shown natural size, "look alive" and illustrations of detail have depth and perspective. These illustrations of more than 300 described "species" of Characeae constitute a record of actual type specimens, in case some may later be lost. They are used effectively in conveying to the reader the authors' evaluation of the described taxa and the basis for their combination of the several types into one taxa. They also indicate differences between taxa recognized in the new arrangement. It seems that the new nomenclature is quite different, but an index leads one to the proper picture through synonyms.

The 812 pages include a table of contents, introduction, index to icones, and index to collectors' numbers. There are 395 icones with data for each and a description of each specimen seen by the authors.

While the actual price of $35 is high, it is in line with prices of recent scientific books of limited distribution.—*L. A. Whitford*

Vision & Value Series: Vol. I, *Education of Vision*, 233 pages; Vol. II, *Structure in Art & in Science*, 189 pages; Vol. III, *The Nature & Art of Motion*, 195 pages, edited by G. Kepes; $12.50 each; George Braziller, 1965.

Gyorgy Kepes' volumes present a discussion and statement, in terms both broad and specific, of this whole matter of seeing. Seeing certainly constitutes a large part of human experience, how large Kepes does not underestimate. How we see determines to a great degree the quality of that experience, and conditions us as individuals and as a society. What and why we do that which is to be seen lies at the roots of the contemplative and creative process. This interplay between seeing as experience, and the development of environment which becomes experience, runs the full gamut of man's faculties.

Kepes has written elsewhere: "Vision is a fundamental factor in human insight. It is our most important resource for shaping our physical, special environment and grasping the new aspect of nature revealed by modern science." In expanding that thesis, Kepes has done a superb job in the three volumes of this series that have been published. Three more are to follow. He has marshalled a well chosen group of psychologists, sculptors and painters, architects, mathematicians and physicists, engineers and inventors, and educators. In an introduction to each volume, Kepes summarizes and pulls together the varying attitudes of the contributors.

The first volume, on Education for Vision, might in a more ponderous sequence, have come last. But, by exploring education for vision, the writers perforce had to expound vision. The psychologists lead off with analyses of some fundamental characteristics of the faculties with which we see.

Arnheim, a psychologist, valiantly attempts to establish visual thinking as

an operation valid in its own right, and not a subsidiary or instrument for other means of knowing. Gerald Holton, a physicist, discusses vision as an implement for understanding the physical world. There is a counterplay between the more precious aspect of vision as an end in itself and vision as a means. An important quality of these volumes lies in the attack on the problem from so many directions. The result, amazingly enough, and Kepes is an amazing person, is not confusion but reinforcement.

Will Burtin's paper takes us into the thick of commercial propaganda where visual means are powerful toward understanding and persuasion. William J. J. Gordon's essay on The Metaphorical Way of Knowing leans strongly toward vision as means rather than end, which happens to be the prejudice of this reviewer which these volumes have reinforced.

The first volume ends with four papers on visual education, ranging from Julian Beinart's description of art education in Africa to Bartlett Hayes' discussion of the full circle of art and art education in our own country. He says that "whereas nineteenth-century art served to remind one of what he already knew, the art of today is a medium of discovery, the most obvious testimony to which is the frequent complaint of the public that he does not understand."

The volume on Structure in Art and Science gets down to cases, though in some instances not without considerable flitting about the core of the matter. Buckminster Fuller's essay, rather autobiographical in nature and properly so, is, as always, stimulating and, in this instance, beautifully clear. The great Pier Luigi Nervi is there, and right at the core. While others seem to vibrate and squiggle ecstatically, Nervi calmly reasons and builds. He does know a law when he finds it, gravity for example. His two essays titled "Is Architecture Moving Toward Unchangeable Forms?" and "On the Design Process" are finely reasoned and superbly illustrated with Nervi's own work. "The equilibrium and harmony that reign over technology, the objectivity that one is forced to assume in its presence, the modesty that its un-

plumbed mysteries demand of us, constitute such an elegant lesson that it cannot but have profound repercussions on all intellectual and moral manifestations and even on the life of a societ."

The volume on The Nature and Art of Motion is particularly apt in a world where, so much of the time, one finds himself in a state of rapid motion or observing rapid motion. Largely devoted to the wordsmanship of painters, and critics, it ends with two excellent contributions: Gordon Washburn's essay on museum design and Donald Appleyard's discussion of motion, sequence, and the city. The illustrations, throughout this volume particularly, are most revealing and helpful toward understanding.

The level of thought and expression in these first three of a six volume series is indeed high. The bookmaking is a delight, with a most satisfactory combination of text, illustrations, and captions. Gyorgy Kepes' introductions, one for each volume, are evidence of his own great understanding and of the wisdom with which he has put together such vital, varying, well-expressed contributions to a central problem of the arts and hence of our times.—*Robert W. McLaughlin*

Advances in Protein Chemistry, edited by C. B. ANFINSEN, JR., *et al.;* Vol. 19 (1964); 408 pages; Vol. 20 (1965); 369 pages; $14.50 each volume; Academic Press.

Although this series of "Advances" implies annual publication, these two volumes actually appeared only a few months apart. Such delays are unfortunate but is of small moment in this case. The fields covered are sufficiently divergent that practically simultaneous publication does not impair their value.

Volume 19 contains four articles: The Hemoglobins by Braunitzer, Hilse, Rudloff, and Hilschmann; Hemoglobin and Myoglobin by Fanelli, Antonini, and Caputo; Linked Functions and Reciprocal Effects in Hemoglobin, a Second Look by Wyman; and Thermodynamic Analysis of Multicomponent Solutions by Casassa and Eisenberg. The latter two have more physics than chemistry and are tough going for any one whose mathematics is rusty.

The four articles in Volume 20 are: Thrombosthenin, the Contractile Protein from Blood Platelets and its Relation to Other Contractile Proteins by Bettex-Galland and Lüscher; Hydrolysis of Proteins by Hill; the Unusual Links and Cross-Links of Collagen by Harding; and The Chemistry of Keratins by Crewther, Fraser, Lennox, and Lindley.

Volume 20 also contains a biographical sketch and a well-deserved tribute to the late Dr. Kenneth Bailey by Prof. S. V. Perry.

Every chapter in both books is followed by an ample bibliography covering the literature through 1963. Good indices, both author and subject, add to their usefulness. They are printed in clear type on a sturdy paper and the illustrations, line drawings, and tables are clear and well reproduced. Altogether, these two books present a detailed summary of the advances to date of protein chemistry in the fields covered and are worthy companions to their predecessors in the series.—*David B. Sabine*

Environment & Archeology: An Introduction to Pleistocene Geography by K. W. BUTZER; 524 pages; $12.50; Aldine Publishing Company, 1964.

Rightly pointing out that Pleistocene geology is primarily concerned with process, stratigraphy, and chronology, Butzer has attempted to provide a text more broadly introductory to Pleistocene geography. His attempt is highly successful. All students of the Pleistocene should read it once, and most of us will find sections to return to—including the 38-page bibliography. The latter supports and lends authority to the text, but so, too, do Butzer's own extensive training and field experience. The publishers have provided an attractive format relatively free from the distraction of minor errors, although the inversion of Figure 56 *is* disconcerting.

After a statement of objectives, and although consistent with them, the

thirty pages on stratigraphy and chronology are nevertheless too brief. They are designed only to set the stage, to provide a chronological backdrop. One may too easily miss, however, both the originality in part of the framework developed and the difficult ambiguities involved in building it. Problems of correlation continue to hamper regional synthesis. They need clear as well as sympathetic understanding by interdisciplinary co-workers.

Nearly half the book reviews types of environmental data, field methods of study, and inferences which may (or may not) be drawn. Vegetation, soils, and geomorphology (110 pp.) have organizational precedent in geographic texts and are well done. Interpretation of Pleistocene sediments (85 pp.) and paleontology (30 pp.) are newly organized and flow less smoothly. They are none the less informative and widely inclusive.

A third of the remainder (75 pp.) is devoted to reconstruction of late Pleistocene environments in the Old World (i.e., mid-latitude Europe to East Africa) and points the way to paleoclimatic interpretation. One could only wish that the data were as complete as Butzer's synthesis is skillful. The final section (Man-Land Relationships, 150 pp.) is certainly the most readable as well as original. Here his thesis, that the reconstruction of past environments must draw on many disciplines and points of view if we are to understand human development, is eloquently defended. He may finally say, in due modesty, "the relations of man to the land in prehistory are, as yet, poorly understood" (p. 471). By this time he has set guideposts which should both attract and provide direction to future students.—*Charles E. Stearns*

The Biosynthesis of Macromolecules by V. M. INGRAM; 223 pages; $8 cloth, $3.95 paper; W. A. Benjamin, Inc., 1965.

While there are several excellent textbooks available in general biochemistry, as well as in classical biology and genetics, the lack of introductory litera-

ture in the rapidly developing field of molecular biology has been noticeable. However, last year, a well written "Introduction to Molecular Biology" by G. H. Haggis, *et al.*, appeared, and now W. A. Benjamin, Inc., introduces a low-priced "Biology teaching monograph series," of which Dr. Ingram's book is the first published volume. This latter series will also include a book by Dr. James D. Watson on "The Molecular Biology of the Gene," and it now seems that the urgent need for good reviews on a somewhat more elementary level in this field will be at least partly satisfied.

Dr. Ingram's book is distinguished by clarity and an accumulation of a great deal of useful facts. While the text provides easy reading for a person somewhat acquainted with the subject, it will probably appear rather compact to a beginner. This is, however, unavoidable as the terminology in general use has to be followed. Difficulties are partly removed through the presence of a glossary at the end of the book with short explanations of many of the more specialized terms.

Of the seven chapters, six treat topics which are today generally regarded as in the area of molecular biology. The biosynthesis of nucleic acids and proteins, constituting the main part of the book, is thoroughly reviewed, and the methods for sequence analysis of these molecules are also discussed. While the structure and configurational properties of the nucleic acids as such are described, little information of a similar type is given about proteins. As this subject often is handled in a rather old fashioned way in conventional biochemistry textbooks, an up-to-date description here, complementary to the author's lucid treatment of the nucleic acids, would have been useful.

In one chapter the genetic control of protein structure is discussed, but the general viewpoint in this work is a biochemical rather than a biological or a genetical one. The book ends with a short chapter on polysaccharides and a recommended reading list. The essential aspects of the subject are covered in an authoritative and convincing manner and provide an excellent introduction to this exciting field for students and research workers in related areas.

Two confusing printing errors should be noted. In figure 3-4 the ionic strength for the right curve is written one order of magnitude too low, and in the legend to figures 7–5, the structure of cellulose is stated to have glucose in α-D-(1 $\rightarrow$ 4) linkages, while the figure is drawn in the right way with β-D-(1 $\rightarrow$ 4) linkages.—*Tomas Lindahl*

The Theory & Practice of Scintillation Counting by J. B. BIRKS; 662 pages; $17.50; The Macmillan Co., Pergamon, 1964. Vol. 27 of International Series of Monographs on Electronics & Instrumentation.

For approximately fifteen years, the technique of scintillation counting has been developing in importance as a means of detection and measurement of ionizing radiation. J. B. Birks has been associated with this development for much of this time and on several previous occasions has provided useful summaries for beginners in the field. This present book is by far the most ambitious undertaking to date in describing the contemporary state of knowledge relating to what is, by now, an exceedingly important technique in many fields, from biophysics to elementary particle physics. Unfortunately, any book treating such a fast moving subject becomes quickly outdated, but Birks has supplied a compendium that is impressively broad up to January 1963, and, in a final chapter added in proof, has managed to mention some developments up to October 1963. The publication date is November 1964.

While one may question the publisher's assertion that "anyone who uses the scintillation counting method will find this book indispensable," there can be no doubt of the claim that "the book will be of great value to physicists, chemists, nuclear technologists, biological and medical scientists and all other users of ionizing radiation." Unfortunately, the usefulness is impaired by the absence of a subject index. The author gives his reason for this omission, but the use of a detailed contents list and

author index still requires a considerable familiarity with the field, which the beginner can hardly be expected to bring to the subject. (For example, the question of linearity of energy response is discussed under a heading "Review of Experimental Data.")

The book discusses the subject by covering three major areas:

i. Studies of the scintillation process (in both organic and inorganic detectors).
ii. The properties of the scintillation counter as an instrument.
iii. Applications of scintillation counters.

In the process, some 1700 references are provided, many of which are useful in providing an historical description of the development of the technique. The discussions of the so-called fundamental processes make clear that these are still describable only in a semiempirical fashion and that much remains to be done before any fundamental understanding can be claimed. Fortunately, in a sense, effective use of this important technique does not require such understanding, and any laboratory utilizing the method would be well advised to provide itself with this product of Birk's careful and extensive labor.—*G. T. Reynolds*

Electromagnetics in Space, edited by K. R. Spangenberg; 277 pages; $15; McGraw-Hill Co., 1965.

A selection of topics from antenna theory and practice is presented in this volume for the use of engineers concerned with the transmission of information through space. The authors, who are members of the Lockheed Missiles and Space Company, are well known as experts in their field. Their work was organized by the late K. R. Spangenberg to whom the authors express appreciation for the role he played in guiding the book to completion.

Chapter 1, by A. S. Dunbar, reviews aspects of electromagnetic theory that continually arise in connection with

antennas. It contains compact information on reflection coefficients, propagation in plasma, polarization, dipole radiation and other items, presented in an elementary way.

Chapter 2, by T. H. Lee, establishes the equation relating received power to transmitted power and the following chapter by J. L. Bellamy gives information on the various sources of the noise against which the received power has to compete. Quantitative reference data on noise of extraterrestrial origin are given in easily usable form.

A chapter on spacecraft antennas by A. S. Dunbar then gives information about the types in use and the factors affecting their design. We learn that some materials, such as cadmium plate, tend to evaporate in space and that some dielectrics deteriorate seriously. Micrometeorite puncture and erosion have to be taken into account. Ways of unfurling antennas from a spacecraft are described together with other mechanical considerations. A good deal of knowhow has accumulated and one gains the impression that rather unlikely looking mechanisms work perfectly well in space and that designing the gadgetry could be a lot of fun.

The remaining half of the book deals with the detailed theory of certain antenna arrays composed of linear elements, with particular emphasis on interaction between the elements. This work, by E. A. Blasi, manager of information technology at Lockheed, leads to design procedures for large arrays which are explained in full detail. An extensive list of references is given, as with all the earlier chapters.—*R. N. Bracewell*

Radioisotopic Power Generators by W. R. CORLISS & D. G. HARVEY; 304 pages; $14.75; Prentice-Hall, 1964.

This volume is mostly concerned with the odd-numbered SNAP (Systems for Auxiliary Power) devices developed by the Martin-Marietta Corporation for the U.S. government. Whereas the even-numbered SNAP's are powered by neutron chain-reacting assemblies, the heat sources for the present machines derive from the thermalization of the emissions, usually alpha- or beta-rays, of unstable isotopes.

A practical radioisotopic generator typically has a power output less than 100 watts at a fuel cost of two cents (Sr^{90}) to $2 ($Po^{210}$)/watt-hr. Since the fuel cost from more conventional sources is a few tenths of a cent per *kilowatt*-hour, the application of the isotopic devices is limited to missions for which reliability is more important than investment. Such missions exist above the atmosphere, below the sea, and in certain relatively inaccessible locations on the earth's surface such as Antarctica.

After an introduction that describes the various types, performance characteristics, operational constraints and missions of the generators, the authors devote whole chapters to (1) fuels, (2) safety, (3) energy conversion, and (4) design principles. The second half of the volume gives detailed descriptions of most devices that have been built or brought to an advanced stage of development. Frequent historical digressions delineate contractual arrangements for the individual projects.

Although the authors' preface refers to the volume as a textbook, it is, rather, a technological treatise. It is also a mildly interesting case history of the application of the principles of systems engineering to a rather tiny corner of the technological landscape. It seems to the reviewer that by the time the radioisotopic generator field is large enough to attract a very wide readership, it is quite likely that this book will be quite dated. A company or an individual entering the field now, however, has at hand a well-written, attractively illustrated, and typographically pleasing introduction.—*R. C. Axtmann*

Chemical Engineering by P. DERIENZO; 243 pages; $7; The Macmillan Co., 1964.

According to the author, this book is intended to aid the experienced engineer in studying for the Professional Engineering Examination. The book concentrates on those unit operations and process applications which have re-

ceived the most attention on state examinations. This specialized treatment has one disadvantage.

If one is given a problem similar to the ones treated in the text—fine. It is more likely that one will need to apply the principles involved, rather than these specific problems, and for this reason, additional reading must be done.

The six chapters cover fluid flow, heat transfer, thermodynamics, drying, distillation, crystallization, extraction, chemical processes, equipment and control. This is an impressive list, but the treatment is, in most cases, a superficial one. Sample problems are presented to illustrate the various topics discussed.

Intelligent use of this book requires much supplementary reading. The book could be useful in suggesting topics for further study and in showing the type of problems usually asked on state examinations.—*Francis Eastburn*

Embryology of the Ovary & Testis Homo sapiens & Macaca mulatta by G. VAN WAGENEN & M. E. SIMPSON; 256 pages; $7.50; Yale University Press, 1965.

Development of human and macaque ovaries and macaque testis is traced from appearance of the genital ridge to the adult gonad. The testis series for the human terminates at 10 months postpartum. The ninety plates comprising the body of this atlas are of good quality for showing histological and embryological relationships. Cytological detail, however, often is inadequate for positive cell identification.

The expected close correlation in development and adult structure of *Homo sapiens* and *Macaca mulatta* gonads is observed.

Unfortunately, the text consists primarily of a static description of histology and embryology, without adequate integration of this information into a clear, comprehensive and dynamic account of the histogenesis of the adult gonad. This failure is most evident in reference to oocyte formation. The direct lineage between the primordial germ cells, which are differentiated early in development, and *all* definitive gametes now is proven beyond doubt. Yet, the authors of this atlas still cling to the idea that at least "some" oogonia are derived from the germinal epithelium. The critical papers on this point are not in the bibliography.

It will be a shock to many to see the *e* deleted in the spelling of meiosis. Such a change in spelling of a long-accepted term is most unfortunate, especially when "miosis," as used exclusively in this book, already has a precise scientific meaning, namely, excessive smallness or contraction of the pupil of the eye. Further errors in terminology include identification of germ cells in the embryonic testis as spermatogonia, whereas repeated observations have revealed that cells with the characteristic features of spermatogonia appear only after birth; and misclassification of spermatogenic cells in the adult macaque testis.

It is unfortunate that the concise, comprehensive presentation of modern concepts of histogenesis of the gonads, and especially of the germ cells, which could have come from this excellent collection of embryonic, neo-natal, and adult gonads has not been realized.—*E. F. Oakberg*

INDEX TO BOOKS REVIEWED. *This index is for quick reference to books reviewed in this issue. In the light of the pressure on space in THE SCIENTIST'S BOOKSHELF section, reviewers are urgently requested to bear in mind that, normally, the length of reviews should be about 300 words.*

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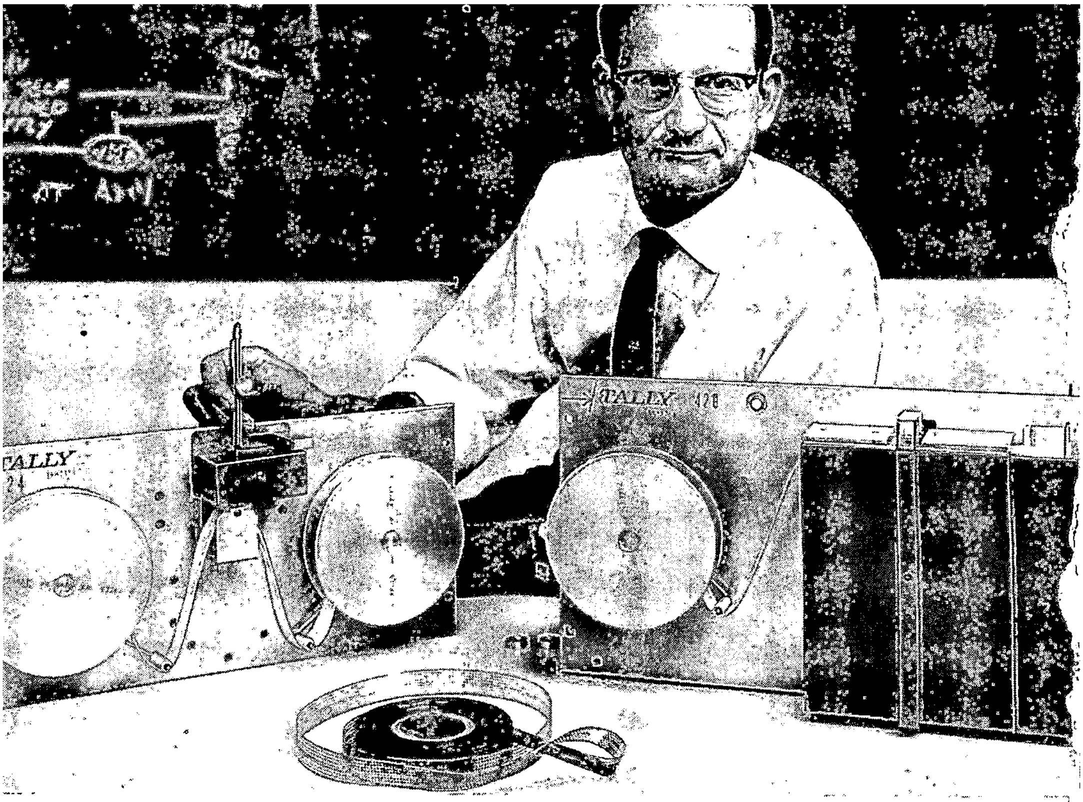

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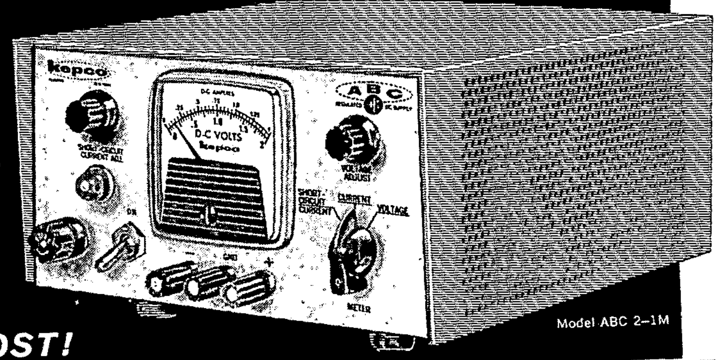

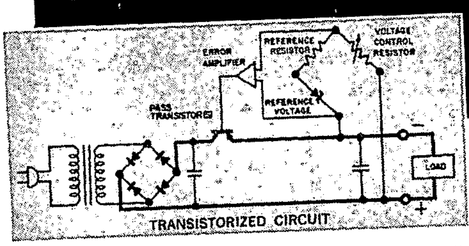

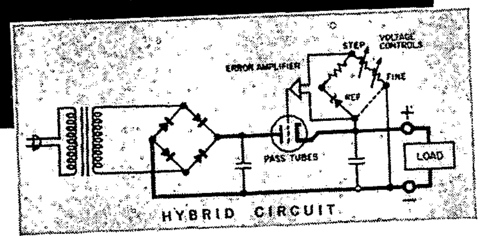

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VOLUME 53 DECEMBER 1965 NUMBER 4

PUBLISHED IN THE INTEREST OF SCIENTIFIC RESEARCH

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GERALD S. HAWKINS, *Sun, Moon, Men, and Stones* 391

A British-born American citizen, educated at Nottingham, London, and Manchester Universities, he studied meteors by radar under Sir Bernard Lovell at Jodrell Bank Radio Observatory. He is now Professor of Astronomy and Director of the Observatory at Boston University, an Astronomer at the Smithsonian Astrophysical Observatory, and Research Associate at Harvard College Observatory. He has conducted further researches on meteors with Super-Schmidt cameras and with a 10 megawatt radar at the Harvard-Smithsonian Observatories. The present article records his recent studies of prehistoric structures in Britain. The alignments found with celestial objects, typically at Stonehenge, have aroused great interest in what may be termed astro-archeology. Address: Department of Physics-Astronomy, Boston University, 700 Commonwealth Avenue, Boston, Mass. 02115.

WILLIAM N. DEMBER, *The New Look in Motivation* 409

In this article, which is based on a lecture given by the author on the occasion of his receipt of the fourth annual Distinguished Scientist Award, presented by the University of Cincinnati, Chapter of Sigma Xi, Dr. Dember describes one version of a newly emerging view of motivation. The "new look" illustrated here by reference to experiments with both rats and college students, stresses the role of information-processing in the arousal and direction of behavior, in contrast to the older view which ties motivation closely to the biological drives. The ideas presented are the product of collaboration between the author and Dr. Robert W. Earl, begun while both were graduate students at the University of Michigan. Dr. Dember has, since receiving his doctorate, taught at Michigan, Yale, Indiana, and at the University of Cincinnati, where he is Professor of Psychology and Assistant Dean of the Graduate School (Cincinnati, Ohio 45221). He is the author of two textbooks, *The Psychology of Perception* and *Visual Perception: The Nineteenth Century.*

JOHN R. DIXON and ALDEN H. EMERY, JR., *Semantics, Operationalism, and the Molecular-Statistical Model in Thermodynamics* 428

An Associate Professor of Engineering at Swarthmore College, Swarthmore, Pa. (J. R. D.) collaborates with a Professor of Chemical Engineering at Purdue University, Lafayette,

AMERICAN SCIENTIST is published quarterly (March, June, September, December) by the Society of the Sigma Xi. *Office of Publication:* Mack Printing Co., 20th and Northampton Sts. Easton, Pa.

Second-class mail privileges authorized at Easton, Pa. Additional entry at the New Haven, Conn., post office, December 9, 1943. This publication is authorized to be mailed at the special rates of postage prescribed by Section 132.122.

Ind. (A. H. E., Jr.) to record their experimentation in approaches to the teaching of thermodynamics in their respective classrooms. This collaboration has on many occasions been extended to the analysis of abstract subject matter and its relation to student knowledge, and to studies of university teaching in its influence on student initiative, creativity, and the forming of student opinion. Dr. Dixon received the Ph.D. degree in Mechanical Engineering at Carnegie Institute of Technology in 1960 and has taught at M.I.T., Carnegie Tech., Purdue, and Swarthmore. Dr. Emery graduated B.S. at Penn State, M.S. at M.I.T., and Ph.D. at the University of Illinois. After industrial experience he has taught chemical engineering for ten years at Purdue University.

ERNEST C. POLLARD, *The Fine Structure of the Bacterial Cell and the Possibility of Its Artificial Synthesis* 437

Born in Yunnan Province, China, 1906, educated at Cambridge University, taking a Ph.D. degree in Nuclear Physics, Dr. Pollard conducted research in nuclear physics at Leeds University and at Yale University until 1940. For five years he worked at M.I.T. Radiation Laboratory on Micro-Wave and Radar. Returning to Yale in 1945, he decided to move slowly into biophysics, initiating a research group there in 1949 and finally establishing a department of biophysics in 1955. The original research program dealt with the effect of radiation and heat on enzymes and viruses. By 1960 the emphasis had changed to the study of structure of bacterial cells. In 1961 Dr. Pollard transferred to The Pennsylvania State University where the Department of Biophysics was established two years later. This Sigma Xi-RESA National Lecture reflects his continuing interest in the presentation of science lectures to non-science students in Pennsylvania State University. The diagrams reproduced were prepared by Dr. Pollard as freehand drawings from which the lantern slides used in the lecture were made.

C. G. HIGGINS, *Causes of Relative Sea-Level Changes* 464

An Associate Professor of Geology in the Department of Geology in the University of California, Davis, took his B.S. and M.S. degrees in Chicago and his Ph.D. in 1950 at Berkeley. His earlier research concerned process geomorphology and historical geomorphology. In 1962–63, he spent thirteen months on an ONR research contract studying coastal geology and shore features and processes in Greece. On a sabbatical leave for 1965–66, he is returning to the eastern Mediterranean to continue his coastal studies. This review was originally prepared for presentation at the 2nd International Conference on Underwater Archaeology held in Toronto in the spring of 1965. The author's best-known early work, published in the Bulletin of the Geological Society of America, was concerned with the San Andreas fault north of San Francisco.

THOMAS H. JUKES, *The Genetic Code, II* 477

The Professor in Residence in the Division of Medical Physics and Research Biochemist in the Space Sciences Laboratory at the University of California, Berkeley, Dr. Jukes continues his story of "The Genetic Code" published by AMERICAN SCIENTIST in June 1963, p. 227. The new knowledge of the code has been made possible by a concerted biochemical attack in many laboratories on this central problem of the science of genetics.

JOHN TYLER BONNER, *Size and Cycle—An Essay on the Structure of Biology* 488

With the permission of the author and of Princeton University Press, we present in this article the introductory chapter from "Size and Cycle" published in September 1965. The author is the Chairman of the Department of Biology, Princeton University and an Associate Editor of AMERICAN SCIENTIST. Recent books by the same writer include *Ideas of Biology* (1962) and *Cellular Slime Molds* (1959).

C. R. WESTON, *A Strategy for Mars* 495

A graduate of the University of California, Santa Barbara, his Ph.D. thesis at Princeton University dealt with the topic of morphogenesis of microorganisms. At the University of Rochester since 1962, he is working on more general problems of microbiology related

to the task of designing life detection experiments for Mars—in particular the so-called "Wolf Trap" named for its poeponent, Professor Wolf Vishniac. His current activities still include some developmental biology, but are primarily related to his interest in the origin of life and the detection of Martian microorganisms. Address: Department of Biology, University of Rochester, River Campus Station, Rochester, N. Y. 14627.

IRVIN GLASSMAN, *The Chemistry of Propellants* 508

A B.S. in Chemical Engineering at Johns Hopkins University, Dr. Glassman carried out three years of war service as a civilian army assignee to the Manhattan Project at Columbia University. He returned to Johns Hopkins to complete his doctorate in 1950. Since then he has been associated with the School of Engineering in Princeton University where he is now Professor of Chemical Engineering. Originally an A.C.S. Lecture, it has been updated to review recent developments. Address: Department of Aerospace and Mechanical Sciences, Princeton University, Princeton, N. J. 08540.

M. H. GREENBLATT, *"The Legal" Value of π and Some Related Mathematical Anomalies* 427A

A graduate of the University of Pennsylvania and a Ph.D. in physics from the same institution, the author of this article has worked on the experimental aspects of secondary electron emission at the R.C.A. Laboratories in Princeton. The article is partly the result of research carried out in preparation of his recent book "Mathematical Entertainments." His present researches at R.C.A. in Princeton are primarily concerned with the guidance of space vehicles.

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Annual Subscription, $3.50. Single Copy, $1.00. Foreign Subscription, $4.00. Back issues of 1961 and eariler can be ordered from Walter J. Johnson, Inc., 111 Fifth Avenue, New, York N. Y., 10003.

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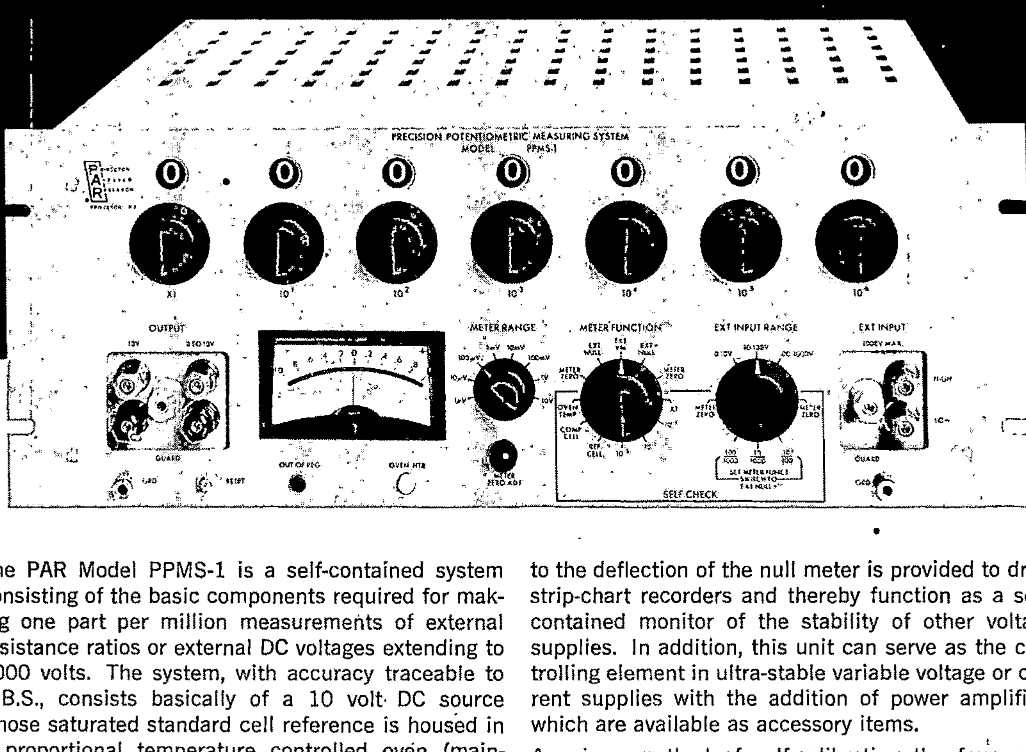

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409A

ARTHUR KEMBLE PARPART

1903–1965

The Board of Editors records with sorrow the death of Professor Arthur K. Parpart, on September 17, 1965. He had a heart attack at his summer home in Woods Hole, Massachusetts. A member of the Princeton University Faculty since 1931, he became Chairman of the Biology Department in 1948 and, in 1952, the first holder of the George M. Moffett Professorship in Biology. He was also President of the Marine Biological Laboratory, Woods Hole, where he and his family spent their vacations. In recent years, Professor Parpart has been responsible for securing major additional facilities for biological research in the two institutions. His own researches included pioneering studies of the role of surface membranes in the life cycle.

The Society of the Sigma Xi records with grateful thanks his services as Associate Editor of AMERICAN SCIENTIST over a ten-year period from the Spring issue of 1955. He brought to this task his rich background of knowledge in biology, editorial skills in specialized journals, and experience in the general field of scientific research and development in local and national areas. His family assisted him in the work of AMERICAN SCIENTIST in well-qualified secretarial and proofreading activities over a number of years.

By resolution of the Executive Committee of the Society of the Sigma Xi at their meeting on October 6, 1965, the Editor-in-Chief was instructed to prepare this memorial of appreciation and remembrance of Arthur K. Parpart, to provide copies for his widow and family, and to record the text in the December 1965 issue of AMERICAN SCIENTIST.

NEWS AND VIEWS

By the Board of Editors and the Membership of the Society of the Sigma Xi and the Scientific Research Society of America, RESA

ANNUAL CONVENTION OF THE SOCIETY OF THE SIGMA XI

In accordance with Article III, Section 1 of the Constitution notice is hereby given that the 66th Annual Convention of The Society of the Sigma Xi will be held on December 29, 1965, at 9:00 A.M., in association with the AAAS meetings at the Hotel Claremont, Berkeley, California.

Article III, Section 2(a) of the Constitution provides: Each Sigma Xi chapter is entitled to be represented at the Convention by not more than three delegates. Appointment of each chapter delegate shall be certified by the president or secretary of the chapter represented. Delegates representing a chapter shall be chosen from its Chapter Members, except that a chapter unable to appoint any or all of its delegates from its Chapter Members may appoint its Affiliate Members or from Chapter Members of other chapters. Each chapter represented at the Convention shall be entitled to one vote on all issues before the Convention.

Article III, Section 2(b) of the Constitution provides: Each Sigma Xi club is entitled to be represented at the Convention by not more than three delegates. Appointments of each club delegate shall be certified by the president or secretary of the club represented. Delegates representing a club shall be chosen from its Members except that a club unable to appoint any or all of its delegates from its Members may appoint as delegate any active Member of the Society. Each club represented at the Convention shall be entitled to one vote on issues directly affecting the clubs. All club delegates shall have the privilege of the floor to discuss any subject before the Convention.

It is expected that the chapters and clubs will make every effort to be represented at the Convention. The names of the delegates should be sent to the Office of the Executive Secretary at National Headquarters at once or not later than December 17, 1965.

For further information regarding procedure for the Convention, please see Section 3.131 of the Manual of Procedure.

Convention Program

December 29, 1965
66th Annual Convention

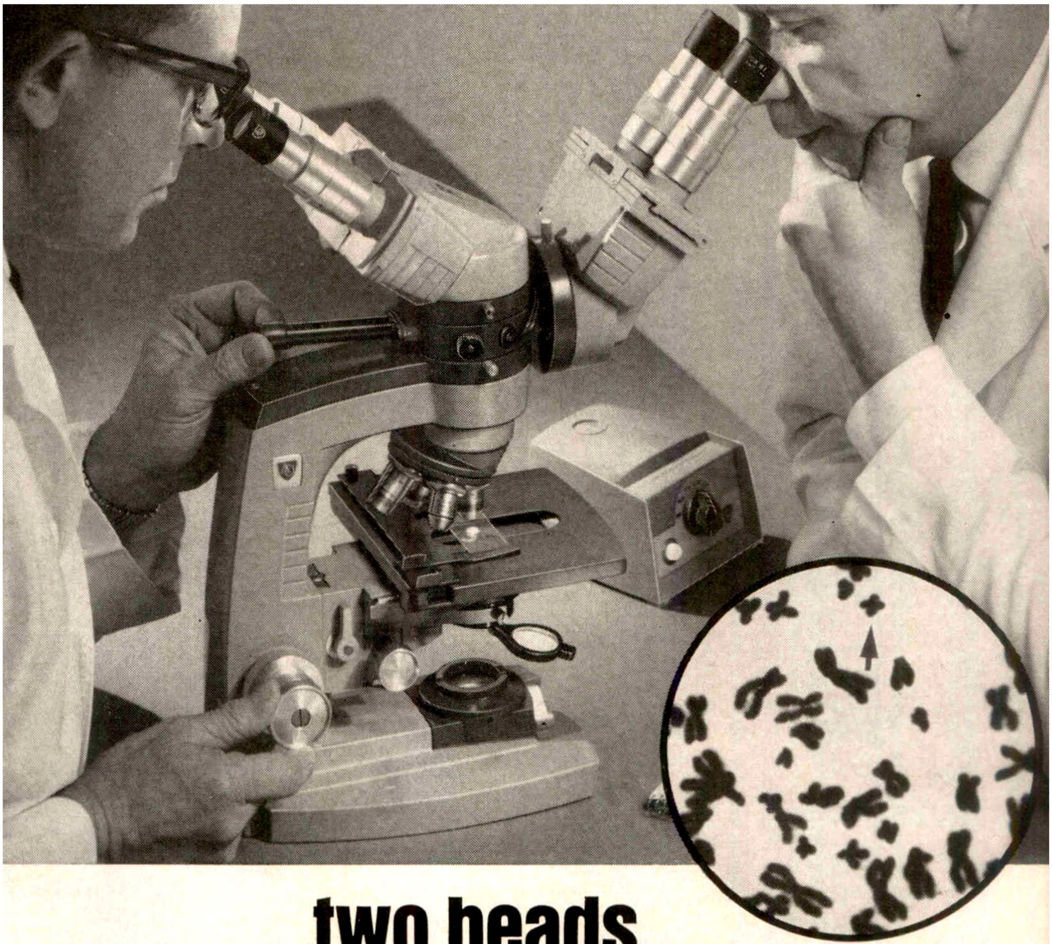
Hotel Claremont—Berkeley, California

8:00–9:00 A.M.—Registration — Horizon Room
9:00–Noon —First Session—Horizon Room
Noon–1:00 P.M.—Joint Sigma Xi-RESA Luncheon* — Lanais 2, 3
1:00–1:30 P.M.—Procter Prize Address —Lanais 2, 3
1:30–3:00 P.M.—Second Session—Horizon Room

* Luncheon tickets will be available in the Hotel Claremont, Horizon Room, from 8:00 A.M. until 9:00 A.M. on December 29 and delegates for whom reservations have been made may pay fee and pick up tickets at that time. Total capacity is limited and therefore it is advisable to secure a reservation.

* * *

8:30 P.M.
Joint Annual
Phi Beta Kappa-Sigma Xi
Lecture
Speaker: Dr. Jacob Bronowski
Address: "The Logic of the Mind"
at
Harmon Gymnasium
University of California, Berkeley



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AGENDA
66th ANNUAL CONVENTION

December 29, 1965

Hotel Claremont—Horizon Room
Berkeley, California

1. Appointment by the President of three members to serve as a Committee on Credentials
2. Report of the Committee on Credentials
3. Approval of Proceedings of the 65th Annual Convention
4. Report of the President
5. Report of the Executive Secretary
6. Report of the Treasurer
7. Report and Recommendations from the Executive Committee:
   A. National Assessments and Fees for 1966–67
   B. Petitions for Chapter Status from:
      1. Boston College
      2. Clemson University
      3. Houston University
      4. Idaho State University
      5. Mississippi State University
      6. Southern Illinois University
   C. Proposed Changes to Constitution and Bylaws:
      1. To amend Article VII, Section 3(c) of the Constitution to clarify requirements for Executive Committee approval of chapter nominations.
      2. To amend Article VI, Section 5(a) of the Bylaws in order to enlarge the Committee on National Lectureships.

      (Details of proposed revisions to Constitution and Bylaws given on page 416A.)
8. Report of Editor-in-Chief of AMERICAN SCIENTIST
9. Report of Editor of *Science in Progress*
10. Reports of other Committees:
    A. Membership-at-Large
    B. National Lectureships
    C. Grants-in-Aid of Research
11. Report of Nominating Committee and Election of Members of the Executive Committee:
    A. Member of the Executive Committee—term expiring 1967—to replace Dr. Herbert E. Longenecker, who was appointed to the Executive Committee in accordance with Article IV, Section 5(d) of the Constitution, upon the resignation of Dr. Eugene P. Odum.
    B. Member of the Executive Committee—term expiring 1969—to replace Dr. Frederick C. Lindvall whose term expires this year—1965.
    C. Member of the Executive Committee—term expiring 1969—to replace Dr. C. Guy Suits whose term expires this year—1965.
13. General Business
14. Adjournment

Suggested candidates for the election of Members of the Executive Committee may be sent to any member of the Nominating Committee:

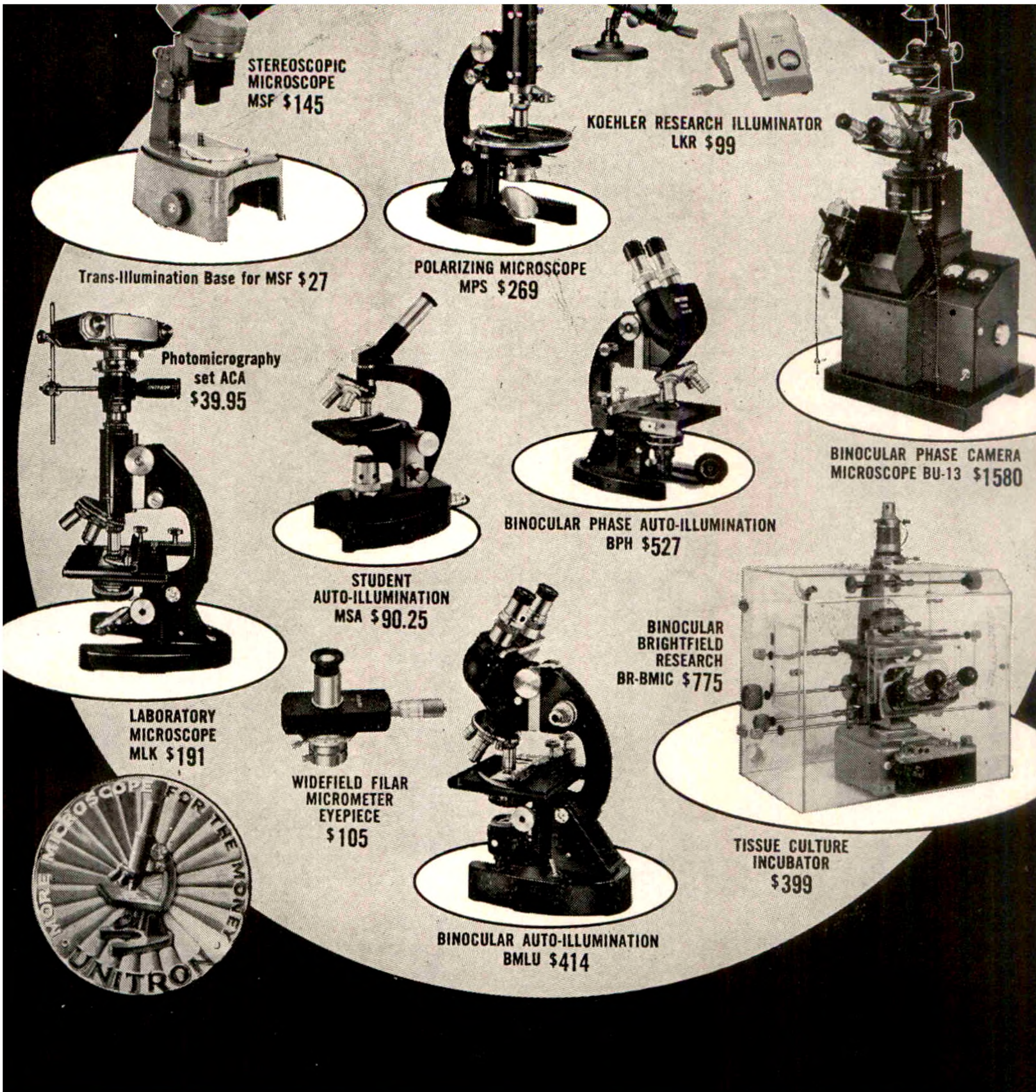
Dr. Waldemar T. Ziegler
School of Chemical Engineering
Georgia Institute of Technology
Atlanta, Georgia 30332

Dr. Dana Young
Southwest Research Institute
San Antonio, Texas 78206

Dr. Frank M. Carpenter
Biological Laboratories
Harvard University
Cambridge, Massachusetts 02138

Dr. H. Richard Crane
University of Michigan
Ann Arbor, Michigan 48104

Dr. Alexander Hollaender
Oak Ridge National Laboratory
Oak Ridge, Tennessee 37831, *Chairman*


STEREOSCOPIC MICROSCOPE MSF $145
KOEHLER RESEARCH ILLUMINATOR LKR $99
Trans-Illumination Base for MSF $27
POLARIZING MICROSCOPE MPS $269
Photomicrography set ACA $39.95
BINOCULAR PHASE CAMERA MICROSCOPE BU-13 $1580
BINOCULAR PHASE AUTO-ILLUMINATION BPH $527
STUDENT AUTO-ILLUMINATION MSA $90.25
BINOCULAR BRIGHTFIELD RESEARCH BR-BMIC $775
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PRESENT WORDING

CONSTITUTION

ARTICLE VII

Section 3.(c) In a non-degree-granting institution not having a chapter, any investigator who has shown noteworthy achievement as an original investigator in some field of pure or applied science, as described in Article I, Section 3. *Nominations* by a chapter's Committee on Admissions must be approved by a three-fourths vote of the (National) Executive Committee.

PROPOSED WORDING

CONSTITUTION

ARTICLE VII

Section 3.(c) In a non-degree-granting institution not having a chapter, any investigator who has shown noteworthy achievement as an original investigator in some field of pure or applied science, as described in Article I, Section 3. *These nominations for election (not promotion)* by a chapter's Committee on Admissions must be approved by a three-fourths vote of the (National) Executive Committee.

PRESENT WORDING

BYLAWS

ARTICLE VI

Section 5.(a) The Committee on National Lectureships shall consist of a chairman elected by the Executive Committee for a term of four years and *four* other members appointed annually by the President upon recommendation of the chairman.

PROPOSED WORDING

BYLAWS

ARTICLE VI

Section 5.(a) The Committee on National Lectureships shall consist of a chairman elected by the Executive Committee for a term of four years and *eleven* other members appointed annually by the President upon recommendation of the chairman.

THE EXECUTIVE SECRETARY'S PAGE

This page, in the first three issues of American Scientist this year, reported the progress of National Headquarters in converting its operations to a computerized system. Since the September issue, complete listings of membership have been prepared by the computer for each chapter and club. In addition, the membership of the Chapter-at-Large has been billed for their 1966–67 national dues under the new procedure. Now, with this issue, another of the important products and advantages of the revised operations has been realized—addressing American Scientist copies by the computer.

Previously, the addressing operation, using address stencils, took National Headquarters approximately four days, required two addressing machines and the time of four clerical assistants. This operation on the computer now requires but $3^1/_3$ hours and one operator.

As previously announced, the new system is based on an eight digit member number for each Member or Associate Member of the Society. The first two digits indicate the year in which the individual was first elected to membership, either as an Associate Member or as a Full Member. The second three digits indicate the electing chapter by the code which was included in the appendix of the September issue of American Scientist. The last three digits merely enumerate.

This member number is imprinted above the individual's name and address on the American Scientist mailing label. In addition, the code number of the chapter or club with which the Member or Associate Member is now active is also given. Starting at once, National Headquarters will, after making an address change in response to a notice from the Post Office or the individual concerned, forward that same notice to the secretary of the chapter or club with which the individual is active. Incidentally, address changes at National Headquarters have been averaging more than 20,000 per year.

On some addresses, the Zip Code is incomplete with only the first three numbers given. It would be of tremendous help if incomplete labels could be returned with the correct Zip Code numbers. These numbers are actually being used in preparing the mailing lists, i.e., the complete active membership tape is sorted by Zip Code number before the mailing labels are produced.

A number of by-products have developed during the conversion operation which are indicative of even greater potential benefits to the chapters and clubs from these new operations than perhaps had been envisioned. For instance, one chapter has sent a tape to National Headquarters with a request that it be returned with a duplicate of the National Headquarters records of its membership. This chapter will use this tape on its own computer equipment to address notices of meetings and other local communications. The chapter plans to maintain this tape once it has been received by using the same procedures as have been developed at National Headquarters.

T. T. H.

ANNOUNCEMENT

As Chairman of the Committee on Membership-at-Large, it is my pleasure to announce that, under the provisions of Article VII of the Constitution, the Chapter-at-Large has *Promoted* to full membership in the Society:

JOSEPH M. AEIN
Institute for Defense Analyses
Arlington, Virginia

CARL FULTON AUSTIN
U. S. Naval Ordnance Test Station
China Lake, California

GEORGE CHRISTOPHER BUELL
California State Department of Public Health
Berkeley, California

DAVID J. COTTER
Alabama College
Montevallo, Alabama

THOMAS S. EDRINGTON
Sandia Corporation
Albuquerque, New Mexico

J. C. MAURICE L'ARRIVEE
Canada Department of Agriculture
Brandon, Manitoba, Canada

WILLIAM CUMMING LEITH
H. G. Acres & Company
Niagara Falls, Canada

GORDON H. SILVER
G. H. Silver & Associates
Newtonville, Massachusetts

DONALD L. WOOD
Shell Chemical Company
Deer Park, Texas

Also, the Chapter-at-Large, with the approval of the National Executive Committee, has *Elected* to full membership in the Society:

VINCENT J. BAGDON
Materials Research Laboratory
Fort Belvoir, Virginia

WAYNE C. HANSON
Battelle-Northwest
Richland, Washington

LOWELL ALVIN KING
United States Air Force Academy
Colorado

JAMES H. MARKS, Chairman
Committee on Membership-at-Large

THE SCIENTIFIC RESEARCH SOCIETY OF AMERICA

The Youngstown Branch of RESA was installed on May 6, 1965, by Doctor Prentice with eighty-four charter members. The original movement to organize this branch was made at the Youngstown Sheet and Tube Co.; but, when it was known that a charter would be presented, many research scientists in and near Youngstown expressed a desire to join the group. The branch name, therefore, was changed to Youngstown, and it will welcome as members qualified scientists in the area.

A branch of RESA was installed at Panama City, Florida, by Doctor Prentice on August 7 with forty-three charter members. The research staff at the United States Navy Mine Defense Laboratory sponsored this branch, and scientists from several other important research laboratories in the neighborhood are members. Twelve members of Sigma Xi are included in the group. Officers of the branch are: George Austin, Jr., President; Charles Foster, Secretary; and Walter Burgmann, Treasurer.

The 1965 Convention of the Scientific Research Society will be held at the Claremont Hotel, Berkeley, California, on Wednesday, December 29 at 3:00 p.m. There will be a luncheon for Sigma Xi and RESA delegates at 12:00 noon on that day at the Claremont Hotel at which the 1965 Procter Award for Scientific Achievement will be presented and the annual Procter Award Address, open to all A.A.A.S. members, will be given—D.B.P.

The SIGMA XI-RESA National Lectureship Program

1965–66 Series Spring 1966

Lecture Schedule

CENTRAL TOUR—SPRING 1966

"The Origin of New World Civilization as Seen from Tebuacan"

DR. RICHARD MAC NEISH

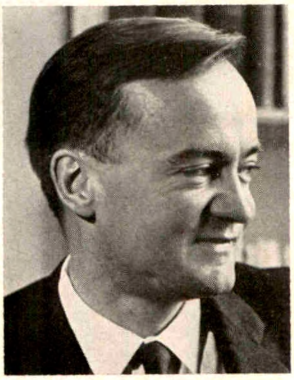
Head, Department of Archaeology

University of Alberta
Calgary, Canada
March 14–25, 1966

MAR. 14 Michigan Technological University ΣΞ Club, Houghton
15 Michigan State University ΣΞ Chapter, East Lansing
16 Dow Chemical Company RESA Branch, Midland, Michigan
17 Bowling Green State University ΣΞ Club, Bowling Green, Ohio
18 West Virginia University ΣΞ Chapter, Morgantown

* * *

21 Ohio University ΣΞ Chapter, Athens
22 Air Force Dayton Laboratories RESA Branch, Wright-Patterson Air Force Base, Ohio
23 University of Kentucky ΣΞ Chapter, Lexington
24 Indiana University ΣΞ Chapter, Bloomington
25 Ball State University ΣΞ Club, Muncie, Indiana



METROPOLITAN NEW JERSEY NEW YORK TOUR—SPRING 1966

"The Evolutionary Development of Natural Science"

DR. STEPHEN TOULMIN

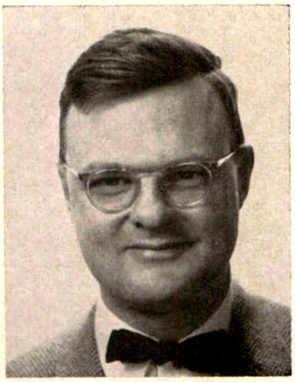
Professor of Physics

Harvard Project Physics
Harvard University, Cambridge, Massachusetts
March 14–25, 1966

MAR. 14 Rutgers—The State University ΣΞ Chapter, New Brunswick, New Jersey
15 City College ΣΞ Club, New York
16 Vassar College ΣΞ Club, Poughkeepsie, New York
17 Adelphi University ΣΞ Club, Garden City, New York
18 Brooklyn College ΣΞ Chapter, Brooklyn, New York

* * *

21 St. Bonaventure University ΣΞ Club, St. Bonaventure, New York
22 Manhattan College ΣΞ Club, Bronx, New York
23 Socony-Mobil ΣΞ Club, Socony-Mobil Laboratories, Paulsboro, New Jersey
24 Downstate Medical Center ΣΞ Club, State University of New York, Brooklyn
25 Polytechnic Institute of Brooklyn ΣΞ Chapter, Brooklyn, New York



MID-ATLANTIC TOUR—SPRING 1966

"Band and Bonds: An Appraisal of the Current State of the Theory of the Electronic Structure of Solids"

DR. HARVEY BROOKS

Dean, Division of Engineering and Applied Physics

Harvard University, Cambridge, Massachusetts
February 14–28, 1966

FEB.	14	*Frankford Arsenal RESA Branch, Philadelphia, Pennsylvania
		Franklin Institute RESA Branch, Philadelphia, Pennsylvania
	15	Lafayette College ΣΞ Club, Easton, Pennsylvania
	16	Drexel Institute of Technology ΣΞ Club, Philadelphia, Pennsylvania
	17	Swarthmore College ΣΞ Chapter, Swarthmore, Pennsylvania
	18	Army Chemical Center RESA Branch, Edgewood Arsenal, Maryland
		* * *
	21	U.S. Naval Research Laboratory RESA Branch, Washington, D.C.
	22	Medical College of Virginia ΣΞ Chapter, Richmond
	23	University of Virginia ΣΞ Chapter, Charlottesville
	24	Old Dominion College ΣΞ Club, Norfolk, Virginia
	25	University of Maryland ΣΞ Chapter, College Park

* *Host for joint meeting.*

MID-WEST TOUR—SPRING 1966

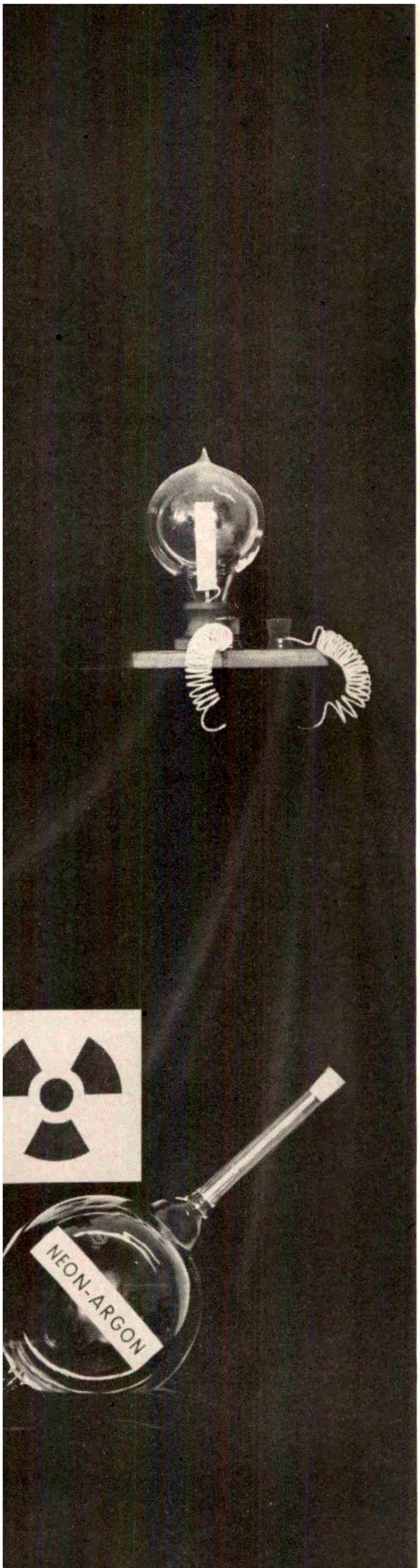
"Regulatory Mechanisms in Higher Plants"

DR. ARTHUR W. GALSTON

Professor of Plant Physiology

Yale University, New Haven, Connecticut
March 21–April 1, 1966

MAR.	21	Abbott Laboratories ΣΞ Club, Chicago, Illinois
	22	Beloit College ΣΞ Club, Beloit, Wisconsin
	23	University of Illinois-Medical Center ΣΞ Chapter, Chicago
	24	South Dakota School of Mines and Technology ΣΞ Chapter, Rapid City
	25	University of South Dakota ΣΞ Chapter, Vermillion
		* * *
	28	University of Minnesota ΣΞ Chapter, Minneapolis
	29	Iowa State University of Science & Technology ΣΞ Chapter, Ames
	30	University of Iowa ΣΞ Chapter, Iowa City
	31	Argonne National Laboratory RESA Branch, Argonne, Illinois
APR.	1	Loyola University ΣΞ Chapter, Chicago, Illinois



One of a series briefly describing GM's research in depth

A Noble Approach to an Old Problem

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He converted heat to electricity in a vacuum tube back in '83. But there was a barrier. We're knocking it down.

In a gas-filled thermionic tube, electrons can be boiled off of an emitter and directed to a collector, giving current flow. But only briefly. Then a cloud of electrons forms in the path . . . a space charge inhibiting further flow.

One way to get rid of this barrier is to neutralize it with positive ions, charged atoms of some gas. Many experimenters use vaporized cesium. But its atoms impede electron flow, requiring close interelectrode spacing. So GM Research physicists chose some of the noble gases—argon, neon, and xenon. They offer less impedance.

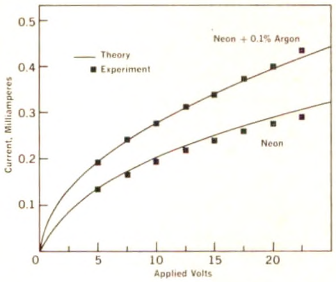
Our experimental emitter is a mixture of fissionable material and good electron-emitting material. Exposed to a neutron barrage in a reactor, the emitte gets hot from its own nuclear fission, sending electrons toward the collector. This same fission produces fragments that bombard the noble gas, generating ions to counteract the space charge.

We have developed a theory to predict the ion generation rate and have experimental data that backs it up. We think we understand why and how things happen.

General Motors is in the energy conversion business. The direct conversion of heat to electricity, with a device having no moving parts, interests us.

General Motors Research Laboratories

Warren, Michigan



Characteristics of tubes filled with gases ionized by fission fragments. Resulting current is a function of ion generation rate, which is increased greatly (from 1.8 to 2.6 x 10^{16} ions per cm^3 per sec) by small addition of argon.

NORTHEAST TOUR—SPRING 1966

"Evolution of an Enzyme"

DR. PHILIP HANDLER

James B. Duke Professor of Biochemistry and Chairman, Biochemistry Department

Duke University Medical Center
Durham, North Carolina
February 28–March 11, 1966

FEB. 28 *Wellesley College ΣΞ Chapter, Wellesley, Massachusetts
Boston College ΣΞ Club, Chestnut Hill, Massachusetts
U.S. Army Natick Laboratory RESA Branch, Natick, Massachusetts
MAR. 1 University of Vermont ΣΞ Chapter, Burlington
2 Brown University ΣΞ Chapter, Providence, Rhode Island
3 University of Rhode Island ΣΞ Chapter, Kingston
4 University of Connecticut ΣΞ Chapter, Storrs

* * *

7 Yale University ΣΞ Chapter, New Haven, Connecticut
8 Wesleyan University ΣΞ Chapter, Middletown, Connecticut
9 University of Massachusetts ΣΞ Chapter, Amherst
*Amherst College ΣΞ Chapter, Amherst, Massachusetts
Mount Holyoke College ΣΞ Club, South Hadley, Massachusetts
10 Albany ΣΞ Club, Albany Medical College, Albany, New York
11 Syracuse University ΣΞ Chapter, Syracuse, New York

* *Host for joint meeting.*

PACIFIC TOUR—SPRING 1966

"Portable Electrochemical Power"
"Fifty Years of Physical Chemistry"
"Direct Use of the Sun's Energy"

DR. FARRINGTON DANIELS

Emeritus Professor, Solar Energy Laboratory

University of Wisconsin, Madison, Wisconsin
April 4–15, 1966

APR. 4 Hughes Aircraft Company RESA Branch, Culver City, California
5 Ventura Branch of RESA, Port Hueneme, California
6 Humboldt State College ΣΞ Club, Arcata, California
7 University of Washington ΣΞ Chapter, Seattle
8 University of Oregon ΣΞ Chapter, Eugene

* * *

11 *University of California at Davis ΣΞ Chapter, Davis
Aerojet General Corp. RESA Branch, Sacramento, California
12 Atomics International RESA Branch, Canoga Park, California
13 University of Southern California ΣΞ Chapter, Los Angeles
14 University of California Berkeley ΣΞ Chapter, Berkeley
15 Chico State College ΣΞ Club, Chico, California

* *Host for joint meeting.*

PLAINS TOUR—SPRING 1966

"The Circulation of the Atmosphere"

DR. EDWARD N. LORENZ

Professor of Meteorology

Massachusetts Institute of Technology
Cambridge, Massachusetts
March 14–25, 1966

MAR. 14	University of Missouri at Rolla ΣΞ Chapter, Rolla
15	*University of Missouri at Kansas City ΣΞ Club, Kansas City Midwest Research Institute RESA Branch, Kansas City
16	Utah State University ΣΞ Chapter, Logan
17	Montana State University ΣΞ Chapter, Missoula
18	Washington State University ΣΞ Chapter, Pullman

* * *

21	University of Wyoming ΣΞ Chapter, Laramie
22	Marathon Oil Company RESA Branch, Littleton, Colorado
23	Wichita State University ΣΞ Club, Wichita, Kansas
24	Kansas State University ΣΞ Club, Manhattan
25	University of Kansas ΣΞ Chapter, Lawrence

* *Host for joint meeting.*

SOUTHEAST TOUR—SPRING 1966

"Molecular Mass Spectrometry"

DR. FRED W. MC LAFFERTY

Professor of Chemistry

Purdue University, Lafayette, Indiana
March 21–April 1, 1966

MAR. 21	University of North Carolina ΣΞ Chapter, Chapel Hill
22	Georgia Institute of Technology ΣΞ Chapter, Atlanta
23	Jacksonville University ΣΞ Club, Jacksonville, Florida
24	Oak Ridge RESA Branch, Oak Ridge, Tennessee
25	University of Tennessee ΣΞ Chapter, Knoxville

* * *

28	Auburn University ΣΞ Chapter, Auburn, Alabama
29	University of Mississippi Medical Center ΣΞ Club, Jackson
30	University of Tennessee—Medical Units ΣΞ Chapter, Memphis
31	Vanderbilt University ΣΞ Chapter, Nashville, Tennessee
APR. 1	University of the South ΣΞ Club, Sewanee, Tennessee

SOUTHWEST TOUR—SPRING 1966

"Instabilities and Phase Transitions Within the Metallic State"

DR. MORREL H. COHEN

Professor of Physics

University of Chicago, Chicago, Illinois

March 7–18, 1966

MAR. 7 *Texas Woman's University ΣΞ Club, Denton, Texas
North Texas State University ΣΞ Club, Denton, Texas
8 Northeast Louisiana State College ΣΞ Club, Monroe, Louisiana
9 Louisiana State University ΣΞ Chapter, Baton Rouge, Louisiana
10 University of Southwestern Louisiana ΣΞ Club, Lafayette, Louisiana
11 *William Marsh Rice University ΣΞ Chapter, Houston, Texas
University of Houston ΣΞ Club, Houston, Texas

* * *

14 The University of Texas ΣΞ Chapter, Austin, Texas
15 Southwest Research Institute RESA Branch, San Antonio, Texas
16 Texas Western College ΣΞ Club, El Paso, Texas
17 Arlington State College ΣΞ Club, Arlington, Texas
18 Bartlesville ΣΞ Club, Bartlesville, Oklahoma

* *Host for joint meeting.*

THE "LEGAL" VALUE OF π, AND SOME RELATED MATHEMATICAL ANOMALIES

By M. H. GREENBLATT

Many people have heard, at some time during their schooling, that "some legislature, somewhere, once tried to legislate the value of π, and set it equal to 3." The very idea of trying to legislate upon something as unlegislatable as the value of π is ludicrous. But such a bill was actually considered, and the details of this and the way it was handled, culminating in its rejection by the State Senate, can be somewhat amusing.

The bill in question is House Bill #246, which was introduced in 1897 into the Indiana State Legislature [1]. The bill was introduced by Representative T. I. Record, representative from Posey County. It does not suggest a single number for the value of π but rather suggests several different numbers. The bill was presumably offered as a contribution to education in the state of Indiana.

The first part of the bill states that the area of a circle is equal to the area of a square whose side is $\frac{1}{4}$ the circumference of the circle. If we represent the radius of the circle by r, the circumference would be $2\pi r$, and the bill would have us believe that the quantity $[2\pi r/4]^2$ is equal to the area of the circle. We have been taught that the area of the circle was simply, πr^2. The area suggested in the bill would be correct if we assumed that π was equal to 4. The bill then goes on to mention that the ratio of the chord to the arc of 90° is as 7 to 8. We would state the chord of 90° in a circle of radius r, was equal to $r\sqrt{2}$, and the arc of 90° is simply $(\pi/2)r$. This latter "truth" would lead to a value of $\pi = \sqrt{2} \times 16/7$. (This last statement is closer to the truth than was the first statement.) The same paragraph goes on to say that the ratio of the diagonal to one side of a square is as 10 is to 7. The assumption here is that the square root of 2 is exactly equal to 10/7. This approximation is good to 1%. The bill then says that the ratio of the diameter to the circumference of a circle is as 5/4 is to 4 (or, $\pi = 16/5 = 3.2$). The paragraph in question winds up by stating that, "since the rule in present use fails to work. . ., it should be discarded as wholly wanting and misleading in the practical applications." The bill ends with the triumphant statement that the author has "solutions of the trisection of the angle, duplication of the cube, and quadrature of the circle, which will be recognized as problems which have long since been given up by scientific bodies as unsolvable mysteries, and above man's ability to comprehend."

When the bill was first introduced into the House of Representatives, in Indiana, it was referred to the Committee on Swamp Lands. The person who referred the bill to this committee is not known, but if he were known today, he might be honored for having given such a diplomatic appraisal of the worth of the bill.

The Committee on Swamp Lands apparently recognized that the bill was not really in their province, and they recommended that the bill be considered by the Committee on Education. The Committee on Education considered the bill, reported it back to the House, and recommended that it should pass. And it did pass, unanimously, 67 to 0.

In the Senate, the bill fared a little bit worse. It was referred to the Committee on Temperance! (Perhaps the same shrewd chap who referred the bill to the House Committee on Swamp Lands had a part in referring it to the Committee on Temperance. A wonderful choice of committees!)

The bill passed the first reading in the Senate, but that is as far as it ever went. After that first passage, the senators were properly coached, and on the second reading, the Senate threw out this "epoch-making discovery" with much merriment.

This anomaly from the Indiana State Legislature may very well be mirrored in other state legislatures. The fact that

the author mentions in the bill that he also has solutions for the trisection of the angle, the duplication of the cube, and the quadrature of the circle, qualifies him for membership in that vast army of "impossible-problem solvers." They have been harshly described as many well-meaning, self-appointed, and self-anointed mathematicians, and a motley assortment of lunatics and cranks, knowing neither history nor mathematics, who supply an abundant crop of "solutions" of the insoluble problems each year. The problems of the trisection of the angle, the duplication of the cube, the quadrature of the circle, and the rectification of the circle, with ruler and compass alone, have been proved impossible. The proofs are accepted by the mathematical community, but the "cranks and lunatics" keep on proposing solutions and constructions.

A ruler and compass alone can be used to solve problems which involve the square root, but not problems which involve higher roots, such as the cube root. The trisection of a general angle involves the cube root, and, therefore, it cannot be solved by the use of a ruler and compass alone. Duplication of the cube involves the cube root of 2, and hence, it too, cannot be solved using only a ruler and a compass. The quadrature of the circle, which is equivalent to finding a square of area exactly equal to that of a given circle, is impossible with ruler and compass alone, because it involves a nonconstructible transcendental number, π. (A transcendental number is one which is not the root of a finite algebraic equation with integral coefficients.) Rectification of a circle, or the problem of constructing a straight line of length equal to the circumference of a given circle, is also impossible with ruler and compass alone, because the problem involves that nonconstructible transcendental number, π, again. All these statements can be proved beyond a shadow of a doubt. But the proofs keep coming.

Part of the reason that "solutions" keep appearing is that the rules of the game may not have been completely understood. Squaring the circle, duplication of the cube, and the trisection of the angle are to be done using the ruler and compass only. (The ruler, of course, is a straightedge, which can only be used for drawing straight lines; it cannot be used to measure distances.)

If we accept these restrictions for the two aforementioned drawing instruments, then it can be proved that these constructions are impossible. The proof of these statements is given in Courant and Robbins, "What is Mathematics?" [2], Chapter 3. As they point out, the sum, difference, product, and quotient of any two given lengths can be obtained by the use of ruler and compass alone. In addition, the ruler and compass can be used to obtain the square root of any length. Thus, only those numbers which are rational numbers or rational numbers increased by the square root of another rational number can be constructed. Put into different words, this means that the solutions of first and second degree equations can be constructed with the use of a ruler and compass alone. But the solutions of higher order equations cannot be so constructed. If we allow ourselves a (seemingly) trivial liberty, and allow the straightedge to be used for measuring lengths, then Figure 1 shows how an arbitrary angle can be trisected. In this figure, the angle AOB is to be trisected.

Around O, we draw a circle of arbitrary radius r. The straightedge is arranged so that it passes through point A, and two points on the straightedge, a distance r apart, are made to fall, one on the circumference of the circle at C, and the other at the extension of line BO, striking it at D. We call the angle AOB, angle x, and angle ODC is called angle y. Angle y is measured by $\frac{1}{2}$ the difference between the larger and smaller arcs of the circle that it intercepts.

Thus, angle y is measured by $\frac{1}{2}$ of the arc from A to B minus the arc from C to E. Since triangle ODC is isosceles, angle y must be measured by $\frac{1}{2}$ of angle x minus angle y. From this, we get the simple expression $y = x/3$. Thus, the trisection of the angle can be performed if we allow ourselves merely to measure one length. The cube root can be constructed by a special machine as is shown in Courant and Robbins on page 146.

All constructions which correspond to rational operations, and those which correspond to the extraction of a square root, can be done with a ruler and compass alone. If we are given a ruler and a compass, and two lengths, A and B, then, for example, we can lay the A and B next to each other, along a straight line, and thus produce the length A plus B. In a similar way, $A - B$ can be constructed. The product $A \cdot B$ can be constructed in the following manner:

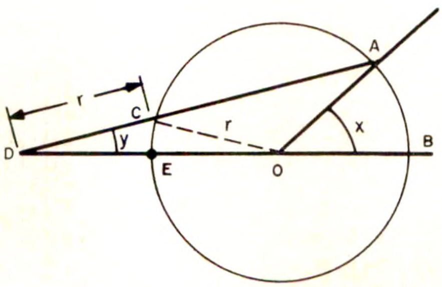


FIG. 1. The trisection of a general angle.

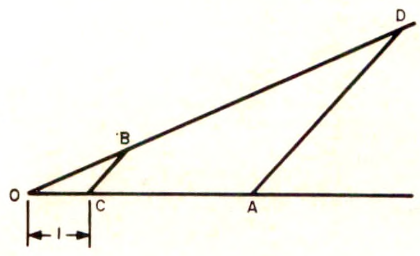


FIG. 2. Formation of the product A·B.

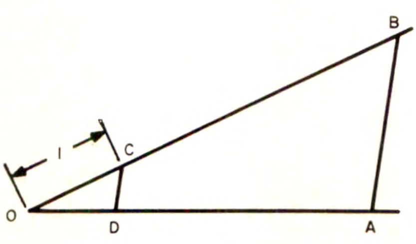


FIG. 3. Formation of the quotient A/B.

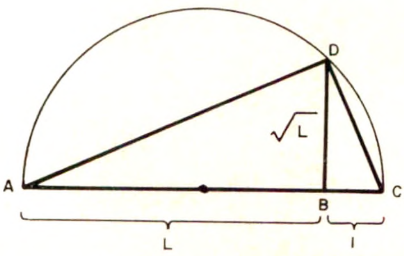


FIG. 4. Construction of the square root.

Two straight lines, which start at a common vertex O, are drawn. Along one of them, the length A is produced, and the unit length is also marked. On the other line, the length B is indicated, and a line from the end of the unit mark to B is drawn. Then, referring to Figure 2, a line through A and parallel to line CB intersects the extension of line OB at D, and OD is equal to the product $A \cdot B$. The division of A by B can be constructed in a similar manner. Referring to Figure 3, from point O two lines of length A and B are drawn. On line OB, the unit length is indicated, at point C. Then, line CD, which is parallel to AB is drawn. This line intersects OA at a distance from O equal to A divided by B.

In addition to these simple, rational operations, the square root of any length, can be constructed. To construct the square root of a dimension L, we have only to lay the dimension L on a straight line, and the unit length is placed right next to it. Then, labeling these points A, B, and C, as is shown in Figure 4, we bisect the length AC and draw a semicircle around it as is shown. Then, from point B, a perpendicular is erected. The length of this perpendicular from the base line up to the point of intersection, D, is equal to the square root of L. This is so because angle ABD and angle DBC are both right angles, and the two triangles are similar.

Having geometrical constructions which correspond to the sum, difference, product, and quotient of any two lengths, and having, further, a construction which can realize the square root of any length, we are in a position to construct the solution of any equation which is of order 2 or less. It is very often found, however, that the solution derived in this manner does not correspond to the most elegant solution of the problem.

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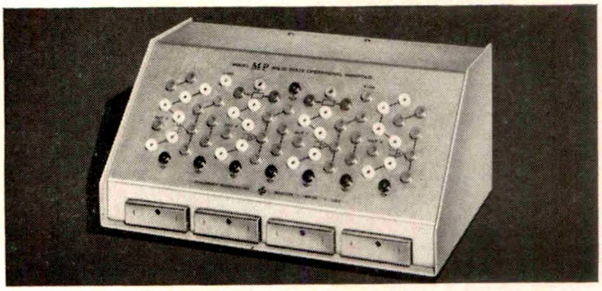

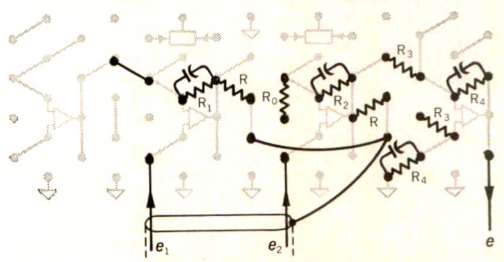

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APPLICATIONS

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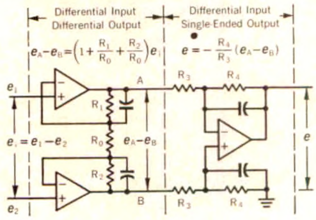

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But, the important thing is that all equations of the 2nd degree or less *can* be solved by this simple construction.

In 1797, the Italian mathematician Mascheroni proved that all constructions which were possible with a ruler and compass could also be done with the compass alone. (A Danish mathematician named Mohr had done approximately the same thing about 100 years earlier. These constructions are now known as the Mohr-Mascheroni Constructions.) [3] It is understood that the straight lines cannot be drawn, but the points defining these straight lines can be constructed.

The straightforward Mascheroni construction leading to the solution of some problems can look messy. This can best be brought out by considering a little problem. Given two points, A and B, construct the other two points which, with A and B, would be at the four corners of a square. This problem is ridiculously easy with ruler *and* compass. If one attacks it according to the general method by which Mohr-Mascheroni problems can be solved, the

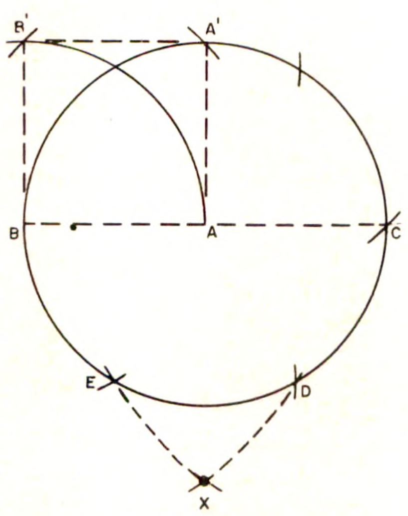

FIG. 5. Solution of the construction of a square.

problem turns out to be somewhat involved. On the other hand, we can consider a drawing, as is shown in Figure 5. The two original points are A and B, and a circle is drawn centered at A and of radius AB. This same radius is laid off 6 times around the circle, thus defining point C, which is along the straight line BAC. We can draw the arcs of radius BD and CE so that they intersect at the point marked X in the figure. The distance from A to X can be calculated, and it is equal to $\sqrt{2}AB$. Thus, the diagonal of the square has been constructed, and having this length, we can draw two more arcs, as shown in Figure 5, so that the two additional points A' and B', are defined. The strict Mohr-Mascheroni construction would have required us to construct two points which lie on a line perpendicular to the line from A to B, and which passed through A, and through B, individually. Then, the semicircle, centered at A and at B, would have had to be bisected. This can surely be done, but most people would probably prefer the first solution.

REFERENCES

1. Proceedings of the Indiana Academy of Science, Vol. 45, 1935, pp. 206–210.
2. COURANT, R., and ROBBINS, H. "What is Mathematics?" Oxford University Press. 1941.
3. GREENBLATT, M. H. "Mathematical Entertainments." T. Y. Crowell Co., New York, 1965, pp. 140ff.

The efforts in the Indiana Legislature recorded in the preceding article were forestalled by prehistoric Britons, according to the findings of Alexander Thom, Emeritus Professor of Engineering Science, at Oxford. On page 150 of *Stonehenge Decoded*© by G. S. Hawkins and J. B. White, published in September 1965, by Doubleday & Co., New York, are to be found the following paragraphs:

"Alexander Thom, Emeritus Professor of Engineering Science at Oxford, maintains that prehistoric Britons possessed hitherto unsuspected skills in geometry. He bases his conclusion on painstaking analysis of ancient stone circles.

"There are several hundred of these rings, varying in diameter from a few yards to 370 feet, scattered over England and Scotland. Called in Gaelic "tursa-

chan" or "mourners," and in Cornwall "merry maidens," they are about 4000 years old. Some 140 of them are still in good enough condition to be studied.

"Thom found that more than 100 of the "mourners" were circles, and thus uninteresting geometrically. But the rest of them were very curious. They were strange figures which at first glance looked like poorly constructed, sloppy circles, but which under close scrutiny were found to be of geometrically precise design. Most of them were composed of two disparate halves. One half was an accurate semicircle, the other was a flattened or bulging approximation of that semicircle. The flattened or bulging figures could be classified in six categories, and Thom found that he could reproduce them all, quite accurately, by simple geometric methods. All that was necessary was to lay out the 'good' semicircle with a stake and a rope and then use different centers, such as the points which trisect the diameter, and different radii, such as one third of the diameter, to lay out the second, 'bad' half of the figure in short arcs. One result of this asymmetric construction was that two of the six categories yielded almost circular figures whose circumferences, or peripheries, very nearly equaled exactly three times their diameters. For one group the ratio was 3.059. For the other, it was 2.957. For a true circle the circumference-diameter ratio, π, is 3.141596 . . . , a number that cannot be written exactly, which is one of the most annoying facts of mathematical life.

"Were those prehistoric Britons trying to make almost-circles whose 'π' equaled *exactly* 3?

"Thom, speaking as an engineer, says that the differences between 3.059 and 2.957 and 3.0 are so relatively small that a modern engineer could not easily measure them in the proportions of those stone "circles," and primitive men with primitive measuring devices very probably could not have detected them. If those ancient builders *were* trying to make $\pi = 3$ in their distorted circles they probably thought that they had succeeded."

Letters to Editors

DEAR SIRS:

I regret to have to call your attention to an alteration of the title of my paper "Ethical Problems of Scientists—A Summary," which appeared originally in *Physics Today*. As reprinted in the September 1965 number of AMERICAN SCIENTIST, the words "A Summary" are absent. One consequence of this absence is bibliographical confusion. The title used for the summary, as published in your journal, is now identical to the title of a full-length paper which appeared in the Summer number of the *Educational Record*, Volume 46, Number 3, pp. 282–296. The full paper will also appear under that title as a chapter in a book, "Lectures in Physics, History, and Society," edited by J. G. Dash, to be published by the University of Washington Press.

I am pleased that mention of the summary character of the article has been made in the section on "Contributors." But let me note that a writer on ethics faces an array of peculiar occupational hazards, including the suspicion of presumption and of a homiletic attitude and tone. These are hazards one can hope to minimize by carefully assembling and presenting evidence and discussion as a basis for what one hopes are reasonably well grounded conclusions. To expose a summary to view without identifying it very conspicuously as such is to expose oneself to more hazard than I would have risked on my own account.

Sincerely yours,
LAWRENCE CRANBERG
Professor of Physics
University of Virginia
Charlottesville, Va. 22903

DEAR SIRS:

I would like to add some comments to the claims made in the article "Liquefied Natural Gas—A New Source of Energy: Part II, Peak Load Shaving and Other Uses," as to the value of liquefied natural gas (LNG) as a rocket and jet fuel.

1. The quoted performance gains on going from LOX-RP-1 to FLOX-LNG are due largely to the inclusion of fluo-

rine in the oxidizer rather than to the change in hydrocarbon fuel.[1] Use of fluorine in large first stages has been considered for years but rejected to date for safety, testing, and cost reasons.

2. The quoted payloads for Saturn V to solar system escape may be relatively correct under whatever assumptions were made in their calculation, but the examples are specious: the three-stage Saturn V is not designed for solar system escape, and would, in fact, be very marginal for such a mission.[2] What is needed and would be used is not a new higher energy first stage nor a new nuclear third stage, development of either of which would be extremely expensive, but the addition of a high-energy fourth stage, such as Centaur, which is already available. With addition of Centaur to Saturn V, the solar system escape payload is 16,000 lb[3] compared to 10,300 lb quoted for the FLOXed three-stage vehicle.

3. The lower density of LNG relative to RP-1 (or a corresponding jet fuel) represents a significant disadvantage in both rocket booster and jet applications.

4. The desirability of a fuel as coolant depends on a variety of factors and on the application: where cooling is done with the coolant under supercritical pressure, as is possible in certain rockets, LNG could indeed absorb more enthalpy per pound than can RP-1; where the coolant is at low pressure, as would be the case in an SST, the greater liquid range of a fuel like RP-1 would make it the better coolant.[1]

5. Statements to the effect that H_2-O_2 is unsuited for a reusable vehicle, and that LNG is safer than RP-1 are, I believe, erroneous. Certainly most recoverable booster studies assume H_2-O_2 as the prime propellant candidate.

LNG is indeed an interesting fuel material, and may yet find application in rockets or air breathers. A more balanced, even if brief, treatment would be preferable for publication in AMERICAN SCIENTIST.

Very truly yours,

ROBERT C. OLIVER

8013 Glenmore Spring Road
Bethesda, Maryland 20034

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1. A. I. Masters, "Applicability of FLOX-Light Hydrocarbons as Liquid Rocket Propellants," AIAA Paper 65-581, Presented at AIAA Propulsion Joint Specialist Conference, Colorado Springs, Colorado, June 14–18, 1965.

2. P. Roman, "The Booster Crossroads," *Space Aeronautics, 43*, 58–61, March 1965.

3. B. Kovit, "The Coming Kick Stage," *Space Aeronautics, 43*, 55–61, August 1965.

DEAR MR. OLIVER:

Your letter of October 13 addressed to the editor of AMERICAN SCIENTIST regarding my paper on LNG (Part II) was forwarded to me for reply. Although I have worked intermittently as a consultant for the past 18-years on an assortment of problems related to rocket engines, I do not consider myself an expert in this field. However, I shall attempt to answer your comments based largely on my more recent experiences with recognized authorities from both NASA and the space industry.

1. I agree that fluorine poses new problems of safety to the space program, but not to the chemical industry, in addition to cost. Nevertheless its performance advantages must certainly justify the large sums being expended both by the government and private industry; I doubt seriously if it will be shelved. Your statement that the performance gains on going from LOX-RP-1 to FLOX-LNG are largely due to fluorine is specious. Tests have shown that LOX-LNG enjoys 9 to 15 sec improvement in specific impulse over LOX-RP-1, which in terms of payload capability is very significant. In addition, there is an important advantage in combustion stability with LNG which was not mentioned.

2. There was no intention to imply that Saturn V is designed for solar system escape. The use of solar system escape payloads as a basis for comparing performance of systems originates with NASA not me. It is not only a convenient basis, but it is also a more mean-

ingful criterion than specific impulse or density impulse. Further I don't believe it is justified to compare four-stage with three-stage vehicles. I have some thoughts about piling Centaur on Saturn V, but since our discussion revolves around a point which I purposely tried to play down by incorporating it only as a footnote, I don't think it deserves further comment.

3. The density of the LOX-LNG propellant is 80 per cent of LOX-RP-1. In the case of FLOX, the LNG system has a density of 88 per cent of RP-1. The corresponding figures for H_2 compared to RP-1 are 27 and 37 per cent. Nevertheless, both LNG and H_2 outperform RP-1 so that density is not so significant a criterion as is implied by your comment. In the case of supersonic aircraft, fuel density is not so critical as in subsonic aircraft because the former is not volume-limited. More important is the aircraft range attainable with the fuel. The combustion efficiency of LNG more than compensates for its low density, and for the applications with which I am familiar it enjoys a 10 to 15 per cent advantage in range over kerosene-type fuels.

4. I doubt if RP-1 could be modified economically to achieve the thermal stability needed to meet cooling requirements on supersonic aircraft, particularly above Mach 2.5. Furthermore, I do not understand your implication that vaporization of the coolant is prohibited. To my knowledge even the thermally stabilized kerosene fuels do not have sufficient heat-sink capacity to handle turbine cooling alone, which for some supersonic aircraft may be slightly over one-half of the total cooling requirements. What the ultimate fuel for supersonic aircraft will be, I'm not prepared to speculate. I, however, do know that LNG is very much in the picture, judging from the continuing inquiries I receive from those responsible for development of the SST. We have studied the nucleate and film-boiling characteristics of LNG at temperature differences up to 1000°F. and pressures up to the critical and they seem promising.

5. There are two schools of thought on reusable engines, those that believe they are sheer nonsense and those who swear by them. In the latter category I have seen some very convincing information from engine manufacturers (of all people) that prompted my statement regarding the relative merits of LNG and H_2 as fuels for recoverable vehicles.

Now on your last point regarding the relative safety of LNG over RP-1, I feel that I can speak as an authority. In fact I don't know of anyone who understands the properties of these two substances that would disagree with me, except you. If you have some information in this respect, I would greatly appreciate receiving it since the question of safety in handling potentially hazardous materials is one of my primary areas of concern. You might be interested to know that we have experimented with the monopropellant system, LNG-LOX, and although it is shock-sensitive, it is not so sensitive as some chemicals that have been handled commercially for many years. Its performance potential is so attractive, it merits consideration. If it could be developed, it could probably outperform any chemical system we have considered to date.

I have not had the opportunity to see the three references you cited; none of them were available at the time I wrote this paper. Thank you for your comments.

Very truly yours,

C. M. SLIEPCEVICH
Research Professor-College of Engg.
The University of Oklahoma
Norman, Oklahoma

DEAR SIRS:

Questions connected with the evolution of human intelligence have been of great interest to me for a long time. Professor Reed's article in the September issue of AMERICAN SCIENTIST provides data of considerable interest, but the conclusion may be questioned if another path of inductive reasoning is followed.

The evolutionary process is most properly viewed as a process of "natural selection" which has unfortunately also been described as a "survival of the fittest." This is unfortunate because it is so difficult to consider a specimen "fit"

except by human standards. An understanding of the evolutionary process is improved by adding some corollary "laws" to the concept of natural selection. These corollaries are:

1. Each specie tends to live in an equilibrium with its environment and the single most important factor is the food supply. Other factors such as nesting places or hibernation caves may be limiting factors for some species but dominant species are always up against a food limitation.

2. Any particular organ or part of an animal is only as good as it has to be, on the average, in all the members in a gene pool. This must be so because nature requires only that the individual survive and reproduce. There is no bonus for excellence. The hawk has marvelous telescopic vision because he must have. The elephant can see only well enough to graze on the branches within reach. The fish in unlighted caves have degenerate eyes and no skin pigment. Humans have vestigial body hair because the use of warm homes and clothing has made a coat of fur unnecessary. A heavy fur coat may be a liability in a house that can harbor lice. The additional nutritional requirement can be a factor. Races of men who had to have heavily pigmented skin have it. Those northern people who had to live indoors and wear clothing much of the year had the skin degenerate to a thin, unpigmented and almost hairless type. "Degenerate" is probably a poor word to use, as the skin is adapted to do its work of elimination, cooling, protecting and sensing without being "over-engineered" according to an average requirement. Sunburn and chafing are minor problems in Northern Europe. In a primitive tribe in Africa where tropical sun, fungus and heat exist, a very blond child would probably not live to the age of one year. The skin is pigmented according to the needs in each gene pool.

Science has not shown that humanity has a special place in nature's scheme such that the ordinary laws can be ignored. Most of humanity lives almost in an equilibrium with the food supply even today. Various changes in technology have expanded the earth's carrying capacity such that famine conditions have not been common for a few generations in Europe and America.

Now if humanity follows nature's laws and if it is not necessary for any organ to have a capability on the average in excess of the minimum required for survival, then it is reasonable to expect that the average human intellectual genetic endowment is what was required for survival in the primitive societies that existed several thousand years ago. It was probably surprisingly high by today's standards because life existed then with poor nutrition, no formal education and a short life span. A mentally retarded child or a psychopathic personality would not be expected to live long.

Another intuitive probe into the evolution of human intelligence makes me feel that the average intellectual endowment (not attainment) is today about as it was 1000 years ago. However, the development of highly organized societies with a division of occupations and a resulting stratification of society has very likely caused a spreading of the curve of distribution. This means that there are more brilliant individuals and more incompetents in a complete sample of the population today. A democratic society will not reduce this effect but intensify it. The gulf between the janitor and the teacher, the working man and the business owner, the welfare recipient and the college faculty, etc. is almost as great in the United States as anywhere. But by permitting the most capable offspring of poorly educated parents into the higher levels of society, the distribution curve will be flattened more and more. By the same token, the less competent offspring of high level parents will have to accept a lower status in their life work.

Is the intellectual genetic endowment in humans changing from generation to generation? My guess is that its average value has not changed very much. The average intellectual attainment, which is something else again, is improving in the western nations and perhaps others. The range of intellectual abilities is prob-

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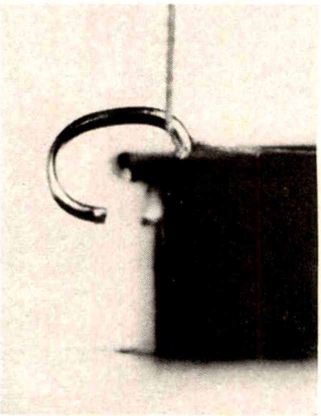
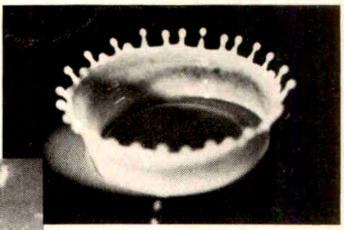
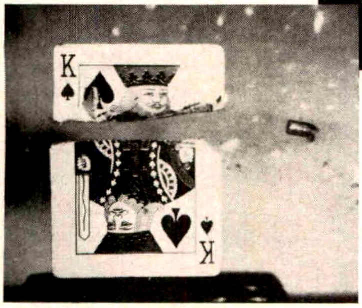


Close-up single-flash photograph of a textile ring traveler. Rotational speed of spindle was 10,000 rpm and linear speed of traveler was 120 feet per second. Actual movement of the $\frac{3}{16}$" x $\frac{1}{16}$" traveler during the 3-microsecond flash from the Strobotac was only five ten-thousandths of an inch. Photo data: camera, 35mm with 140mm lens and extension bellows; film, Plus-X; exposure, f/5.6. Photo courtesy Whitin Machine Works.



The jeweled crown of a splattering milk drop photographed in 1-millionth of a second with a conventional camera and a Strobotac.



Single-flash photograph of a playing card being split by a .22-calibre bullet traveling at about 1150 feet per second. Photo data: 135mm lens with +3 close-up attachment located about 10 inches from card; film, Polaroid Type 47; exposure f/8 for 3 microseconds.

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ably wider now than heretofor. The range is enough to provide leadership and scientific and technical skills—more could be used. The average I.Q. which is needed to permit survival and reproduction today in our well-fed European and American nations must be lower than what was required 10,000 years ago. Consequently, we must assume that there is danger of a degeneration, not an improvement in the intellectual genetic endowment in humans.

Let us start thinking about some eugenic controls before nature decides the way with famine and catastrophe.

Very truly yours,
ROBERT SELFRIDGE
7620 Capricorn Drive
Citrus Heights, California

I am pleased that my short paper stimulated Mr. Selfridge to write the short, short paper above. It restates the traditional eugenic position that man's genotype is still in need of considerable improvement. The basic concept is acceptable to me. My paper showed that the *genetic* component of intelligence cannot change rapidly in the world population, and that if it is changing at all at present it could be improving.—SHELDON REED

MISCONDUCTORS

Through the courtesy of the Editor of the *Journal of the Royal Society of Arts*, and with the permission of Sir Charles Goodeve, we reprint a portion of the discussion succeeding a lecture by Professor Sir Willis Jackson on "New Materials in Engineering" to the Royal Society of Arts printed in the September 1965 issue of its journal.

SIR CHARLES GOODEVE, F.R.S. (British Iron & Steel Research Association): One serious question, and if the Chairman will permit, one frivolous. The serious one is that rumors keep occurring that super-conductivity at room temperature is likely to be discovered. Is there any evidence of that? The second is, why are semi-conductors known as feminine?

THE LECTURER: I know of no evidence to support the rumor you mention. On the other hand, I have been told that the Americans have produced a semi-conductor laser capable of operating at room temperature. As to the sex of semi-conductors, I do not know.

SIR CHARLES GOODEVE: I would have thought that your students would have told you that it is because their resistance decreases as they warm up! At the Bell Telephone Laboratories semi-conductors are known as "misconductors."

From the Department of Mechanical Engineering of the University of Texas comes the following commentary on recent issues of AMERICAN SCIENTIST.

DEAR SIRS:

You are guilty of spreading an insidious disease. My current symptoms:

p. 178A, Chorus Girls
p. 157, The Frug
p. 159, Suburbia

The reading of such articles will never be quite the same again.

(Signed) J. A. Scanlan

Members of Sigma Xi and RESA who keep back issues of AMERICAN SCIENTIST can trace the symptoms of the disease at the places cited in the above letter.—

GENTLEMEN:

My compliments to Scanlan for submitting, and to you for publishing, the report on those symptoms of an insidious disease, involving Chorus Girls (mea culpa), The Frug, and Suburbia. I appreciate your sending me the advance warning . . .

The fact that my effort (which really *did* resemble two Musketeers) was published upside-down, whereby it even better resembled Chorus Girls, highlights the generality and power of the imaginative approach to chemical formulae. This I applaud.

Would that all diseases were as beneficient—to mens and corpus both—as this one. I welcome anything that limbers up the imagination. The gentleman in Canada really started something, and I am most grateful to him for it. It may, indeed, dispel some of the image of "grim young scientists" that I have heard about.—Charles H. Chandler

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BOOKS RECEIVED FOR REVIEW

TO THE MEMBERSHIP: *Our readers are reminded that this section of their journal is intended to be an information service listing first editions of the most important new scientific hardbound and paperbound books received for review in our Princeton Editorial Offices. Titles are for 1965 publication unless otherwise noted.*

From **Academic Press**:

Advances in Lipid Research, Vol. 3, edited by R. PAOLETTI & D. KRITCHEVSKY; 263 pages; $13.

Advances in Astronomy & Astrophysics, Vol. 3, edited by Z. KOPAL; 391 pages; $14

Handbook of Microwave Ferrite Materials, edited by W. H. VON AULOCK; 518 pages; $12.

Methods & Goals in Human Behavior Genetics, edited by S. G. VANDENBERG; 351 pages; $8.95.

A Theory of Sets by A. P. MORSE; 130 pages; $7.95. Vol. 18 of Pure & Applied Mathematics Monographs & Texts, edited by P. A. SMITH & S. EILENBERG.

Energy Metabolism, edited by K. L. BLAXTER; 450 pages; $15. Proceedings of Troon, Scotland Symposium, May 1964. European Assoc. for Animal Production Publication No. 11.

International Review of Connective Tissue Research, Vol. 3, edited by D. A. HALL; 281 pages; $12.

Geometric Invariant Theory by D. MUMFORD; 146 pages; $5.50.

Plasma Turbulence by B. B. KODOMTSEV; 149 pages; $5.60.

Mammalian Radiation Lethality, A Disturbance in Cellular Kinetics by V. P. BOND, *et al.;* 340 pages; $9.50.

The Kjeldahl Method for Organic Nitrogen by R. B. BRADSTREET; 239 pages; $9.50.

Theory of Jets in Ideal Fluids by M. I. GUREVICH; 585 pages; $15. Translated from the Russian.

Scintillation Counters in High Energy Physics by YU. K. AKIMOV; 198 pages; $9.50.

Evolving Genes & Proteins, edited by V. BRYSON & H. J. VOGEL; 629 pages; $19.50. Symposium of the Rutgers State University Institute of Microbiology.

The Control of Fertility by G. PINCUS; 360 pages; $9.

Lattice Defects in Quenched Metals, edited by R. M. J. COTTERILL, *et al.;* 807 pages; $22. International Conference, Argonne National Laboratory, June 1964.

Optical Physics by M. GARBUNY; 466 pages; $14.50.

Fundamentals of Carbanion Chemistry by D. J. CRAM (Vol. 4 of *Organic Chemistry, A Series of Monographs* edited by A. T. BLOMQUIST); 289 pages; $9.50.

Boron-Nitrogen Compounds by K. NIEDENZU & J. W. DAWSON; 176 pages; $6.75. In English.

Focus on Bacteria by E. KLIENEBERGER-NOBEL; 145 pages; $5.50.

Solid State Physics, Advances in Research & Applications, Vol. 17, edited by F. SEITZ & D. TURNBULL; 379 pages; $14.

Deranged Memory (A Psychonomic Study of the Amnesic Syndrome) by G. A. TALLAND; 356 pages; $9.50.

Selected Papers on Molecular Genetics (A Collection of Reprints), introduced by J. H. TAYLOR; 649 pages; $9, cloth; $5.95 paper. Vol. I of *Perspectives in Modern Biology.*

Computers in Biomedical Research, Vol. I, edited by R. W. STACY & B. WAXMAN; 562 pages; $20.

Radiation Dosimetry, edited by F. W. SPIERS & G. W. REED; 308 pages; $14; *Quantum Electronics & Coherent Light,* edited by C. H. TOWNES & P. A. MILES; 371 pages; $16. Courses 30 & 31 Proceedings of "Enrico Fermi" International School of Physics, Varenna, Italy.

Methods of Animal Experimentation, Vol. I, edited by W. I. GAY; 382 pages; $13.50.

Advances in Heterocyclic Chemistry, Vol. IV, edited by A. R. KATRITZKY; 462 pages; $15.

Advances in Child Development & Behavior, Vol. II, edited by L. P. LIPSITT & C. C. SPIKER; 269 pages; $4.

Analgetics (Chemistry & Pharmacology), Vol. V of Medicinal Chemistry Monograph Series, edited by G. DESTEVENS; 475 pages; $17.

Mechanisms of Hormone Action, edited by P. KARLSON; 275 pages; no price given. A NATO Advanced Study Institute.

Methods of Matrix Algebra by M. C. PEASE, III, Vol. 16 of *Mathematics in Science & Engineering,* edited by R. BELLMAN; 406 pages; $13.75.

Light: Physical and Biological Action by H. H. SELIGER & W. D. MCELROY; 417 pages; $12.

The Amino Sugars, the Chemistry & Biology of Compounds Containing Amino Sugars, Vol. II-A, edited by E. A. BALAZS & R. W. JEANLOZ; 591 pages; $22 (subscription price $19.50).

Advances in Theoretical Physics, edited by K. A. BRUECKNER, Vol. 1; 323 pages; $12.

Advances in Atomic & Molecular Physics, edited by D. R. BATES & I. ESTERMANN; Vol. 1; 408 pages; $13.50.

The Theory of Inbreeding by SIR RONALD A. FISHER, 150 pages; $6.

Cells and Tissues in Culture, Methods Biology & Physiology, edited by E. N. WILLMER; Vol. 1, 788 pages, $25; Vol. 2, 809 pages, $25.

Metabolism of Steroid Hormones by R. I. DORFMAN & F. UNGAR; 716 pages; $32.

The Theory of Electron-Atom Collisions by G. F. DRUKAREV; 161 pages; $6.50.

Encyclopedia of Chemical Technology, Vol. 6, 2nd revised edition, KIRK-OTHMER; (Complexing Agents to Dextrose & Starch Syrups), 932 pages; $45 single copy.

Symmetries in Elementary Particle Physics, edited by A. ZICHICHI; 1964 International School of Physics "Ettore Majorana"; 429 pages; $12, cloth; $7.95, paper.

Applied Optics & Optical Engineering, A Comprehensive Treatise, edited by R. KINGSLAKE, Vol. II; 390 pages; $15.

Recent Progress in Photobiology, edited by E. J. BOWEN; 400 pages; $12; Proceedings of an International Congress held at Oxford; 1964.

Lignin Biochemistry by W. J. SCHUBERT; 131 pages; $8.

Behavior of Nonhuman Primates by A. M. SCHRIER, *et al.;* Vol. 1, 318 pages, $9; Vol. 2, 342 pages, $9.50.

The Physiology of the Insect Central Nervous System, edited by J. E. TREHERNE & J. W. L. BEAMENT; 277 pages; $10.

Applied Geochronology by E. I. HAMILTON; 267 pages; $10.

Biochemistry of Quinones, edited by R. A. MORTON; 585 pages; $18.

Theory of Optimum Aerodynamic Shapes: Extremal Problems in the Aerodynamics of Supersonic, Hypersonic & Free-Molecular Flows, edited by A. NIELE; Vol. 9, Applied Mathematics & Mechanics; 455 pages; $16.50.

Chemical Oceanography, edited by J. P. RILEY & G. SKIRROW; Vol. 1; 712 pages; $25.

Nonlinear Partial Differential Equations in Engineering by W. F. AMES; Vol. 18, Mathematics in Science & Engineering; 511 pages; $16.

Paired-Associates Learning: The Role of Meaningfulness, Similarity, and Familiarization by A. E. GOSS & C. F. NODINE; 358 pages; $13.50.

The Peptides Vol. I: Methods of Peptide Synthesis by E. SCHRODER & K. LUBKE; 481 pages; $20.

Advances in Geophysics, Vol. XI, edited by H. E. LANDSBERG & J. VAN MIEGHEM; 349 pages; $14.

From **Addison-Wesley Publishing Co., Inc.:**

Development of Concepts of Physics by A. B. ARONS; 972 pages; $14.75.

Atomic & Space Physics by A. E. S. GREEN & P. J. WYATT; 619 pages; $18.75.

From **Aldine Publishing Co.:**

Women's Prison: Sex & Social Structure by D. A. WARD & G. G. KASSEBAUM; 269 pages; $7.50.

Negroes in Cities; Residential Segregation & Neighborhood Change by K. E. & A. F. TAEUBER; 284 pages; $9.75.

Problems of Youth: Transition to Adulthood in a Changing World, edited by M. SHERIF & C. W. SHERIF; 336 pages; $8.95.

Social Change in Israel by J. MATRAS; 211 pages; $6.95.

A Dictionary of Geography by F. J. MONKHOUSE; 344 pages; $7.95.

Population in History (Essays in Historical Demography), edited by D. V. GLASS & D. E. C. EVERSLEY; 692 pages; $17.50.

From **American Elsevier Publishing Co. and Elsevier Publishing Co.:**

The Planet Uranus (A History of Observation, Theory & Discovery) by A. F. O'D. ALEXANDER; 316 pages; $12.75.

A Concise Encyclopedia of Metallurgy by A. D. MERRIMAN; 1178 pages; $7.50.

Satellites of the Solar System by W. SANDNER; 151 pages; $6.50.

Ultrastructural Plant Cytology by A. FREY-WYSSLING & K. MUHLETHALER; 377 pages; $24.

Stationary Phase in Paper & Thin-Layer Chromatography, edited by K. MACEK & I. M. HAIS; 358 pages; $16.

Toxicity & Metabolism of Industrial Solvents by E. BROWNING; 739 pages; $32.50.

Neutron Radiography (Methods, Capabilities & Applications) by H. BERGER; 146 pages; $9.

The Art of Research (A Guide for the Graduate) by B. E. NOLTINGK; 142 pages; $5.75.

Radiochemical Survey of the Elements by M. HAISSINSKY & J.-P. ADLOFF; 177 pages; $12.

Comprehensive Biochemistry, edited by M. FLORKIN & E. H. STOTZ; 323 pages; $17. Vol. 6: Lipids & Amino Acids & Related Compounds.

Exotic Diseases in Practice by B. MAEGRAITH; 361 pages; $9.50. *Organization for Protein Biosynthesis* by A. WISEMAN; 217 pages; $8.

The Penicillin Group of Drugs by G. T. STEWART; 212 pages; $10.

Nobel Lectures in Physiology or Medicine, Vol. II; 1922–41, by the Nobel Foundation; 3 vol. set; $85.

Cheese by J. G. DAVIS; Vol. 1, *Basic Technology,* 463 pages, $15; Vol. 2, *Annotated Bibliography with Subject Index,* 275 pages; $13.

Serotonin by S. GARATTINI & L. VALZELLI; 392 pages; $25.

Identification of Mineral Grains by M. P. JONES & M. G. FLEMING; 102 pages; no price given.

From **Basic Books, Inc.:**

Science in the Nineteenth Century, Vol. III of A History of Science, edited by RENÉ TATON, transl. by A. J. POMERANS; 623 pages; $17.50.

The New Intelligent Man's Guide to Science by I. ASIMOV; 864 pages; $12.50.

From **W. A. Benjamin, Inc.:**

Bioenergetics by A. L. LEHNINGER; 258 pages; cloth $6; paper $2.95.

Periodic Correlations by R. RICH; 159 pages; cloth $8; paper $3.95.

Solid-State Chemistry of Binary Metal Hydrides by G. G. LIBOWITZ; 139 pages; $7.50.

Mathematics for Introductory Science Courses: Calculus & Vectors by D. A. GREENBERG; 213 pages; cloth $5; paper $2.45.

Oxygen by M. ARDON; 106 pages; $6.75.

Molecular Biology of the Gene by J. D. WATSON; 494 pages; cloth $10; paper $5.95.

Quantum Mechanics in Chemistry by M. W. HANNA; 253 pages; $7 cloth; $3.95 paper.

Organic Reaction Mechanisms by R. BRESLOW; 232 pages; $7 cloth; $3.95 paper.

From My Life (The Memoirs of RICHARD WILLSTATTER), translated from the German by L. S. HORNIG; 461 pages; no price given.

From **Consultants Bureau Enterprises,** including **Plenum Press:**

Review of Plasma Physics, Vol. I, edited by M. A. LEONTOVICH; 326 pages; $12.50.

The Pathology of Thinking by B. V. ZEIGARNIK; 211 pages; $12.50. International Behavioral Sciences Series.

The Use of Surfactants in the Petroleum Industry, edited by P. A. REBINDER; 346 pages; $45 paper.

Advances in Cryogenic Engineering, Vol. 10, Part I—Sections A–L; Part II—Sections M–U, edited by K. D. TIMMERHAUS; 940 pages total; $17.50 per vol. Proceedings of 1964 Conference, August, Philadelphia, Pa.

Acoustic Coagulation & Precipitation of Aerosols by E. P. MEDNIKOV; 180 pages; $25 paper.

Optical Methods of Investigating Solid Bodies, edited by D. V. SKOBEL'TSYN; 188 pages; $22.50 paper.

Cosmic Rays, Vol. 26, edited by D. V. SKOBEL'TSYN; 254 pages; $27.50. Proceedings (Trudy) of the P. N. Lebedev Physics Institute.

Developments in Applied Spectroscopy, Vol. 4, Proceedings of the 15th Annual Mid-America Spectroscopy Symposium, edited by E. N. DAVIS; 546 pages; $18.50.

Advances in Electronic Circuit Packaging, Vol. 5, edited by L. ROSINE; 297 pages; $15. Proceedings of the 1964 Electronic Circuit Packaging Symposium.

Advances in X-Ray Analysis, Vol. 8, edited by W. M. MEULLER, *et al.;* 472 pages; $20. Proceedings of the 13th Annual Conference on Applications of X-Ray Analysis.

Nuclear Magnetic Resonance in a Flowing Liquid by A. I. ZHERNOVOI & G. D. LATYSHEV; 166 pages; $22.50 paper.

Probability Tables for Locating Elliptical Underground Masses with a Rectangular Grid, by I. D. SAVINSKII; 110 pages; $15.

Digital Computer Applications to Process Control, edited by W. E. MILLER; 593 pages; $17.50. Proceedings of IFAC/IFIP 1st International Conference held in Stockholm.

Space Physics with Artificial Satellites by YA. L. AL'PERT, *et al.,* translated from the Russian by H. H. NICKLE; 240 pages; $25.

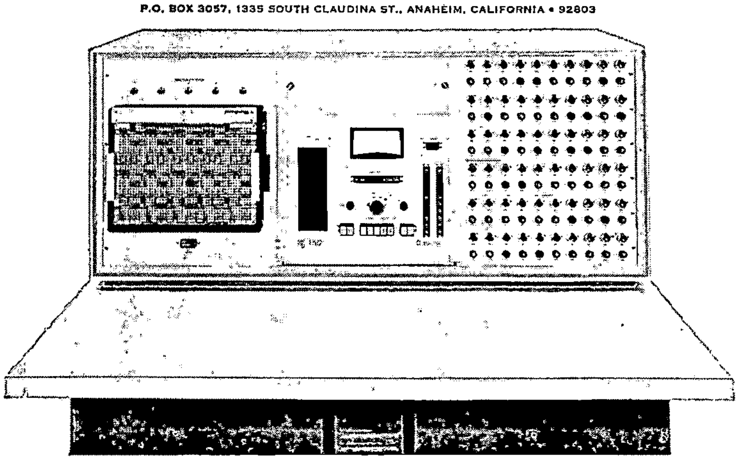
Lectures on Gas Chromatography—1964: Agricultural & Biological Applications, edited by L. R. MATTICK & H. A. SZYMANSKI; 256 pages; $12.50.

Gases in Cast Metals, edited by B. B. GULYAEV; 257 pages; paper, $27.50.

Structure of Aqueous Electrolyte Solutions and the Hydration of Ions, by O. Y. SAMOILOV; trans. by D. J. G. IVES; 185 pages; paper, $15.

Distribution Functions of the Element & Mineral Contents of Igneous Rocks, D. A. RODIONOV; 80 pages; paper, $17.50.

Applied Capillary Microscopy: The Role of Microorganism in the Formation of Iron-Manganese Deposits by B. V. PERFIL'EV, *et al.;* 122 pages; paper, $22.50.



Ultrasonics in the Chemcial Industry by V. A. Nosov, Soviet Progress in Applied Ultrasonics, Vol. 2; 164 pages; paper, $25.

Stable Radicals by A. L. Buchachenko; 180 pages; $15.

Plant Indicators of Soils, Rocks, and Subsurface Waters, edited by A. G. Chikishev; 209 pages; $27.50.

Handbk. of Ultraviolet Methods by R. G. White; 365 pages; $17.50.

Polypropylene Fibers & Films by A. V. Galanti & C. L. Mantell; 179 pages; $12.50.

Nuclear Structure & Electromagnetic Interactions, edited by N. MacDonald; 510 pages; $22.50.

Biological Aspects of Social Problems, edited by J. E. Meade & A. S. Parkes; 226 pages; $9.75.

Gas Chromatography of Steroids in Biological Fluids, edited by M. B. Lipsett; 315 pages; $12.50. Proceedings of the Warrenton, Va., Workshop, February 1965.

From **Doubleday & Company, Inc.**:

Give and Take: The Biology of Tissue and Transplantation by F. D. Moore; 216 pages; $1.25, paper.

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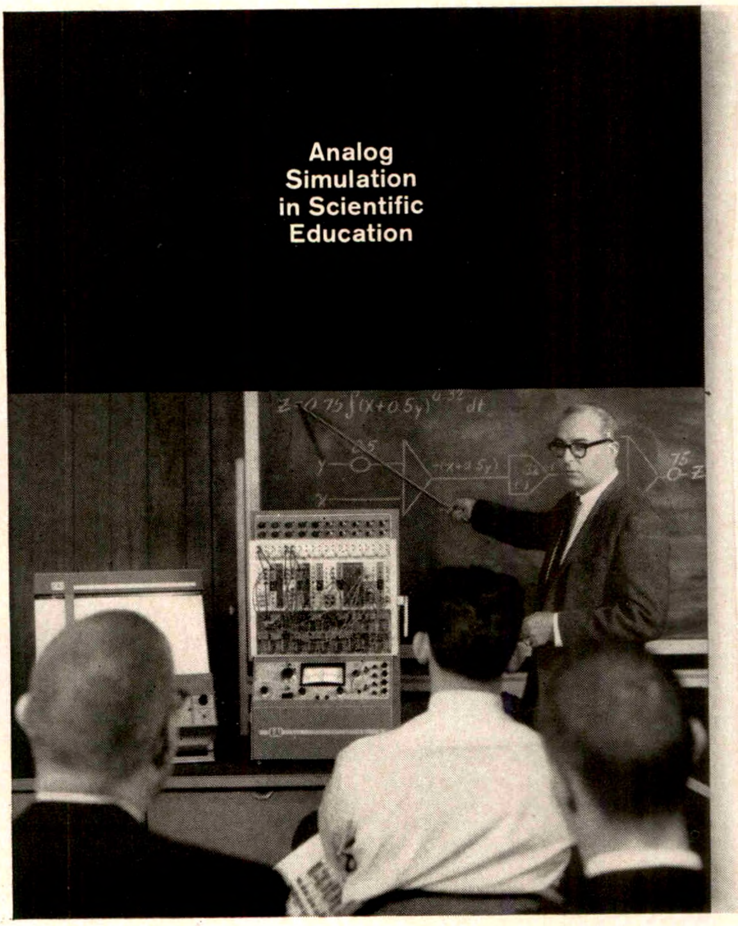
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Illustrations of the Huttonian Theory of the Earth by JOHN PLAYFAIR; facsimile reprint; 528 pages; $2.75 paper.

(*To be continued in March 1966 issue*)

Report from

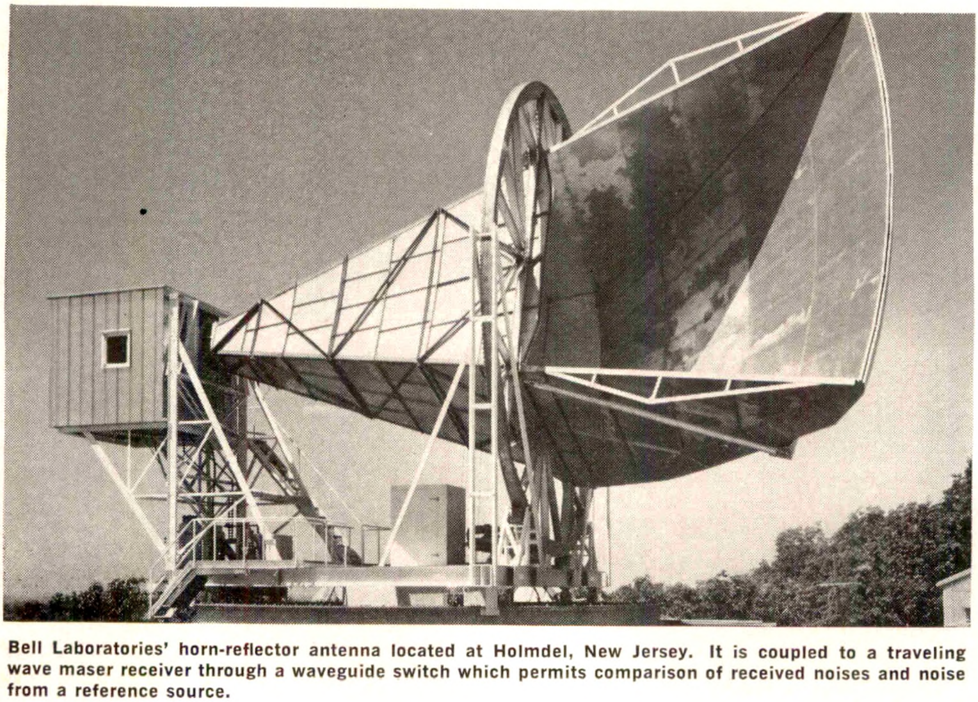
BELL LABORATORIES

A radio problem that may

Activities in technology sometimes have surprising implications. For example, recent antenna tests conducted by Bell Telephone Laboratories at Holmdel, New Jersey, have apparently produced evidence about the early history of the universe.

In their radio communications studies, Bell Laboratories scientists had been using a horn-reflector antenna (employed on Project Echo and Telstar® experiments) to measure the radio noise emitted by Cassiopeia A, an exploded star now surrounded by fiery gas. This and other similar measurements require accurate knowledge about or elimination of noise produced by the atmosphere, the ground, and the components of the antenna system itself. Now, noise from the Earth's atmosphere can be accurately measured and the antenna is so directional that ground noise is negligible (verified through a series of tests with a mobile transmitter). The electrical joints in the antenna system and waveguide were reworked and sealed to eliminate any possible noise due to leakage. And, an extremely accurate noise-level reference source—the best produced so far—was designed and built especially for this project.

But there was some noise which could not be explained. It was stronger than that radiated by the distant fixed stars.



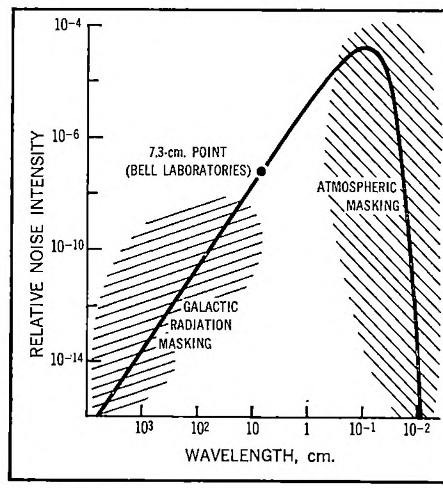
Bell Laboratories' horn-reflector antenna located at Holmdel, New Jersey. It is coupled to a traveling wave maser receiver through a waveguide switch which permits comparison of received noises and noise from a reference source.

have a ten-billion-year-old solution

It showed none of the patterns typical of man-made interference. Drs. A. A. Penzias and R. W. Wilson were frankly puzzled. Strangely enough, similar unexplained noise, of the same order of magnitude, had been suspected by Bell scientists during the Project Echo and Telstar experiments. At that time, though, measurement techniques were not sufficiently perfected to allow them to be certain of their suspicions.

Not far away, however, at Princeton University, an explanation was being devised without knowledge of the Bell experiments. A group under Prof. R. H. Dicke was seeking information about the relationship between gravity and the recession of distant galaxies from us and from each other. The original composition of our galaxy (inferred from spectral lines of "old" stars) and the belief—held by many astronomers—that all matter was once compressed into a vastly smaller volume than at present, suggested to the group that the universe was at that time much hotter—a veritable fireball. Such a fireball would emit a characteristic "black-body" radiation which—after cooling through billions of years of expansion—would have fallen in frequency from about 10^{20} cps. to about 10^{10} cps. It would thus lie in the radio spectrum, at wavelengths of a few centimeters. This was very much like the noise which was puzzling the men at Bell Laboratories.

A mutual acquaintance saw a possible connection and put Bell in touch with Princeton. Result: the signal received at Bell Laboratories has enabled Prof. P. J. Peebles of Princeton to draw the hypothetical radiation spectrum shown in the figure. Future measurements at other



Virtually all of the "black-body" radiation which might have come from the supposed primordial fireball is concentrated between wavelengths of 7500 cm. and 0.01 cm. However, the long-wave end of the spectrum is masked by the galactic radiation to which radio astronomers listen and the short-wave end is masked by the Earth's warm-air atmosphere. Therefore, only the portion of the curve between about 20 cm. and 1 cm. can be studied. Bell Laboratories has supplied a point at the Telstar wavelength (7.3 cm.). Bell and Princeton scientists will next look for other points along the same curve. If these points are found, they will be powerful evidence of such radiation and, in turn, of the former existence of the fireball itself.

wavelengths within this spectrum are planned at both Bell and Princeton to determine whether there was a primordial fireball. If so, it will be the first reliable view man has had of events 10 billion years ago.

Bell Telephone Laboratories

Research and Development Unit of the Bell System

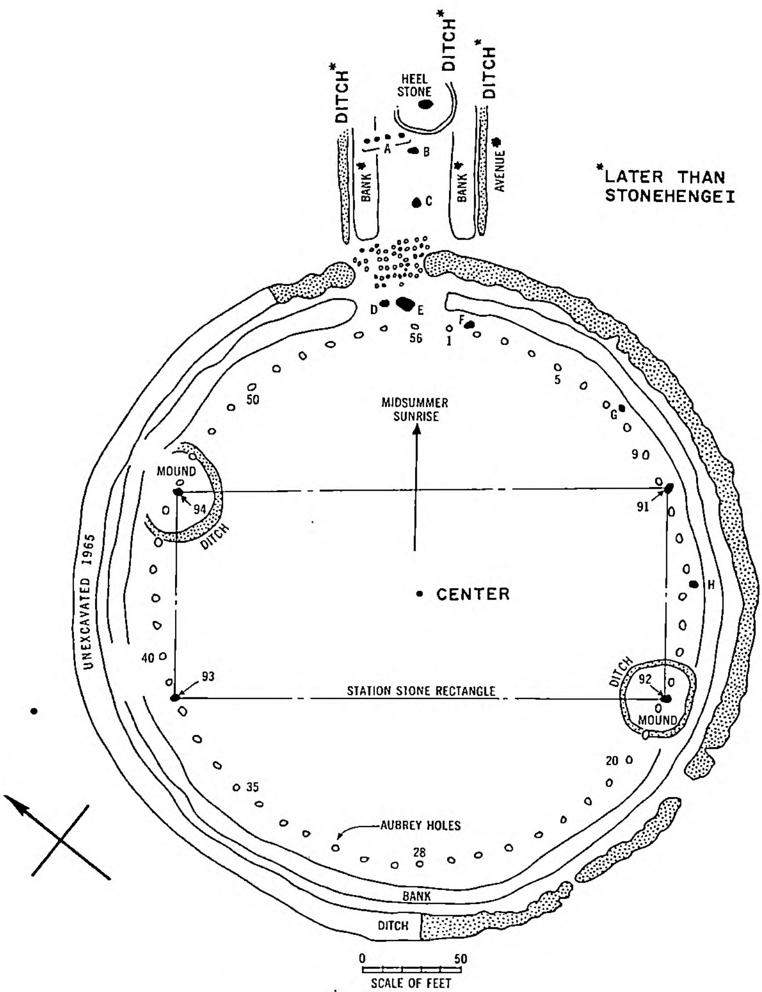


FIG. 1. Stonehenge I c. 1900 B.C.

AMERICAN SCIENTIST, *53*, 1965

AMERICAN SCIENTIST

WINTER · DECEMBER 1965

SUN, MOON, MEN, AND STONES

By GERALD S. HAWKINS

THE SCHOLAR rarely works in the boundary zone between two disciplines by choice, and if necessity compels him to do so he feels strangely uncomfortable when outside his original field. As an astronomer, I recently examined the question of where certain archways and stone alignments in southern England pointed. The calculations were straightforward, though tedious, and the method was well understood in astronomy. The results of the calculation were clear and unambiguous. The only trouble was that, at first sight, the findings seemed unbelievable. In following the clues, I was led into archaeology—or rather I had to study the archaeological details so carefully determined in a one-acre plot of the chalkdowns over a time span in prehistory of approximately 300 years. The findings then seemed to throw some light on the thought processes of prehistoric people which could best be understood by anthropologists. Prehistorians became interested in the fact that a set of stones could convey and preserve information without the need for a written word, and classical scholars began to find confirmation for part of the findings in the myths and legends. Stonehenge at once presented problems with which a scholar of any one discipline would be uncomfortable.

The first construction work at Stonehenge began around 2000 B.C. At that time, Britain had for several thousand years been well separated from continental Europe by the widening of the North Sea and English Channel as the glacial ice retreated and melted. Hunters and other migratory people had crossed to England and southern Ireland before the land bridge disappeared, and there was still a great deal of travel between Europe and Britain over the widening seas. Along the coasts bordering the Irish Sea, the passage graves give evidence of settlers arriving by boat from the Mediterranean area. The voyage became

increasingly difficult and, by 2000 B.C., the inhabitants in Britain had developed a characteristic culture with traceable associations with their continental counterparts.

The actual dates of Stonehenge are uncertain to within a century or so, but the chronology may be based roughly on the facts that the entire

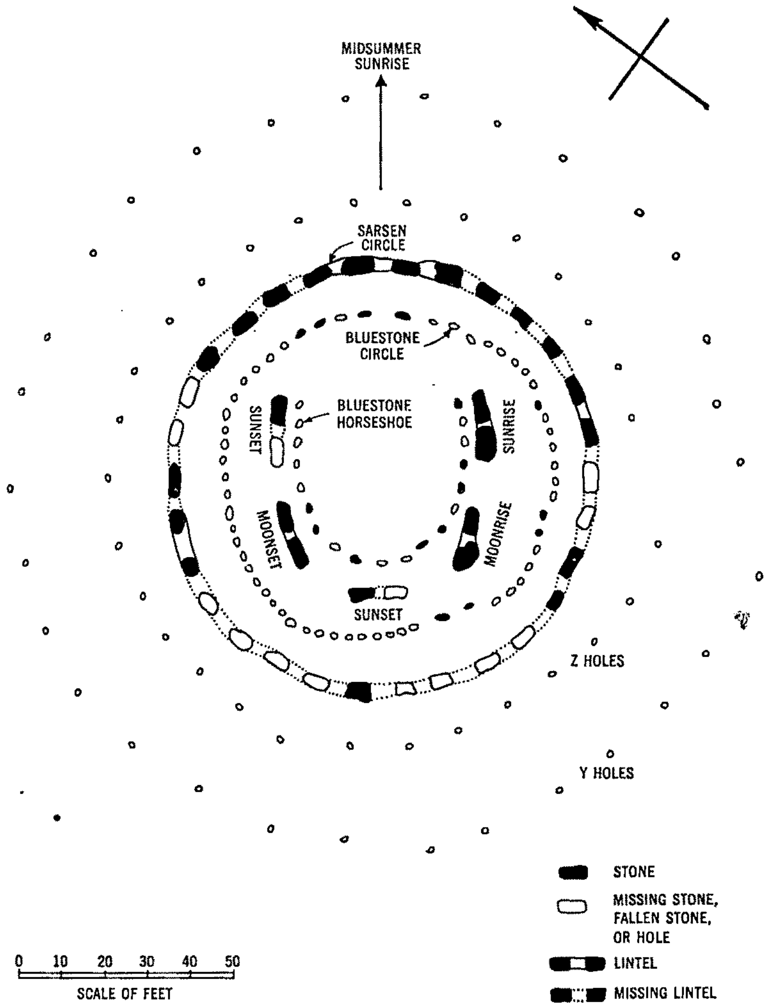


FIG. 2. Stonehenge III c. 1600 B.C.

building operation seems to have stretched over approximately three centuries, and that the trilithons—the archways of dressed sandstone—were put in place towards the end of the building operation in 1700 B.C. This date is fixed to within ± 150 years by the radiocarbon dating of a deer antler found buried at the base of one of the stones.

In phase I, the Stonehengers dug a circular ditch into the chalk some

350 feet in diameter, 15 feet wide, and 6 feet deep (Fig. 1). Beneath the thin layer of turf, the downs are composed of chalk which forms the white cliffs of England when exposed on the southern coast. The excavation from the ditch was piled in a wall or bank some 320 feet in diameter. The white bank encircled Stonehenge except for a gap towards the northeast. Subsequently, many holes were dug and stones were set, but

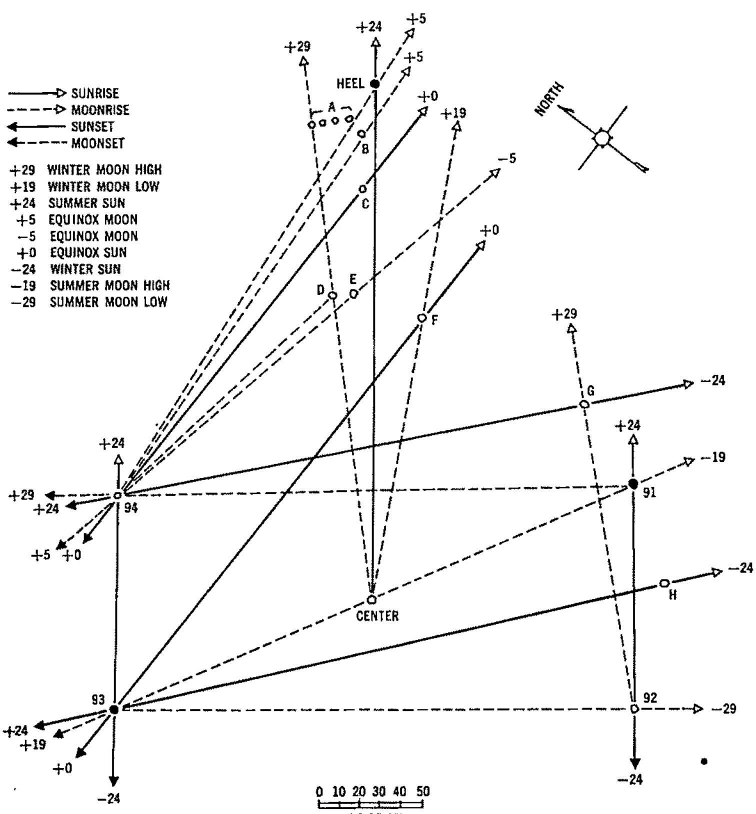


FIG. 3. Sun and Moon alignments of Stonehenge I.

the exact chronological order of the operations is not certain. The work probably extended for two or three centuries up until, but not including, the time of Stonehenge III, Figure 2. It is convenient to treat all of these stones and holes as one unit under the title "Stonehenge I."

The 35-ton heelstone was erected in the position where it stands today. Holes A, B, C, D, and E were dug near the heelstone and probably at one time held stones. Stone holes F, G, and H were dug around the perimeter, though some archaeologists feel that G and H are so shallow that they may not have been dug for the express purpose of holding a

stone. The 56 evenly spaced Aubrey holes were dug around the perimeter and then almost immediately refilled with white chalk rubble. Certainly, these are not stone holes and they are too large for wooden posts. At later times, these Aubrey holes were disturbed for the insertion of cremated burial. After the Aubrey holes were dug, stones were set at the corners of an approximate rectangle, designated in the plan as 91, 92, 93, and 94. Stones 92 and 94 were differentiated from the rest by digging a ditch around them, as also was the heelstone.

The sarsen boulders of this group that remain today are rough, with only slight traces of tooled dressing. In all probability, they were found as a natural outcrop in the vicinity of Stonehenge and selected for the appropriateness of their natural shape. The heelstone, for example, rises to a rounded point and its silhouette is symmetrical as seen from the center of the circle. The heelstone now tilts at an angle of approximately 30°, but presumably, in its original condition, was upright and then the tip of the stone would project approximately 0°5 above the level of the skyline. The other stones were probably set to be level with the skyline when viewed from across the circle.

What were the Stonehengers doing? An analysis of all possible lines between the stones and stone holes shows that certain of these lines point to critical positions of the rising and setting of the Sun and Moon. The alignments that I recognized with the help of a machine computing program are shown in Figure 3. Approximately half of these alignments have been published in *Nature* (Hawkins, 1963, 1964). The remainder, particularly the equinox alignments, have been published in *Stonehenge Decoded* (Hawkins and White, 1965). To understand these alignments it is necessary to go over some astronomical facts concerning the Sun, Moon, and ecliptic. Meanwhile, in passing, it is interesting to note that most of the stones mark two or more different alignments, which virtually rules out the possibility of a chance placement, and which could indicate a deliberate attempt to make each position mark both the Sun and the Moon.

A civilized person hardly notices that the Sun does not always rise in the east. From March 21 to midsummer's day it rises each morning further to the north, reaching an extreme on either June 21 or 22 depending on the leap year calendar. From June to December the point of sunrise swings to the south and, for temperate latitudes, the six-month angle of swing is almost 90°. Classical antiquity said that chariots were pulling the Sun on its yearly course around the tilted ecliptic, and the apparent movement was well recognized as the cause of the seasons.

Because the Earth's orbit is almost a circle, and because the tilt of its axis of spin does not change appreciably during the year, there are obvious symmetries. The midwinter Sun sets opposite to the position where the midsummer Sun rises. Midsummer sunset is opposite mid-

winter sunrise. Also, sunrise is opposite to sunset on the day of the equinox.

The winter-set, summer-rise line is marked by the short sides of the rectangle and by the line to the heelstone. The winter-rise, summer-set line is marked by 94-G and, less accurately, by 93-H. The equinox line is marked by 94-C and 93-F.

Thus, the Stonehengers had lines that marked the seasons of the year, irrespective of whatever arbitrary calendar system they had chosen to adopt. They had the means to confirm that the Sun was on course. They certainly had reasons to be vitally concerned with the observations. If the Sun ever failed to turn at the heelstone at midsummer and day after day rose further to the left, then intense heat and drought would surely follow. Today we have absolute confidence in the regular move-

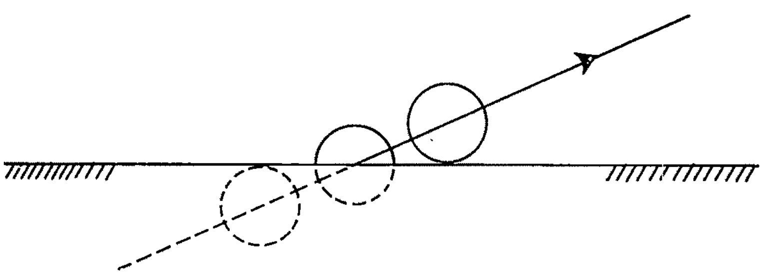


FIG. 4. The apparent path of the Sun or Moon when rising in north temperate latitudes.

ment of the Earth around the Sun, and in the precise equations that govern the sunrise and the seasons. Yet astronomers are always prudently checking the positions in the sky just to be sure.

At this point let us digress for a moment to consider what is the actual instant of sunrise or sunset. The astronomer's answer is definite and unambiguous. The instant of astronomical sunrise is when the upper rim of the Sun is level with the astronomical horizon—a flat horizontal plane at eye level. Of course, no one ever sees *this* sunrise. The closest one could come would be to float, almost submerged, in a calm ocean, but even then the Sun would be totally invisible. A few seconds must elapse before the rim of the Sun projects sufficiently to be seen. Generally the skyline, the observer's horizon, is at a higher altitude than the astronomical horizon, and the visible sunrise is delayed further.

Meteorological sunrise on the other hand is when the disc of the Sun is bisected by the astronomical horizon—a more practical definition but still unrelated to the actual skyline. A third possibility is the instant when the lower rim of the Sun touches the horizon, the instant when the glowing disc seems to separate from the Earth. Similar definitions may be used for sunset.

For an observer on the equator, the Sun rises vertically and the direction of sunrise is the same whatever definition is used. At temperate latitudes, the apparent path of the Sun is inclined as shown in Figure 4, and, not only is there a time difference of about four minutes between the first gleam and the leaving of the horizon, but there is a difference in the directions of the various sunrises of about 1°.

The first alignments that I calculated, particularly the heelstone sunrise, tended to match the full disc leaving the skyline. Later, when the total number of alignments found for the Sun and Moon had reached 35, the average was more in correspondence with the disc bisected, similar to the meteorological definition. Because of the slight deviations

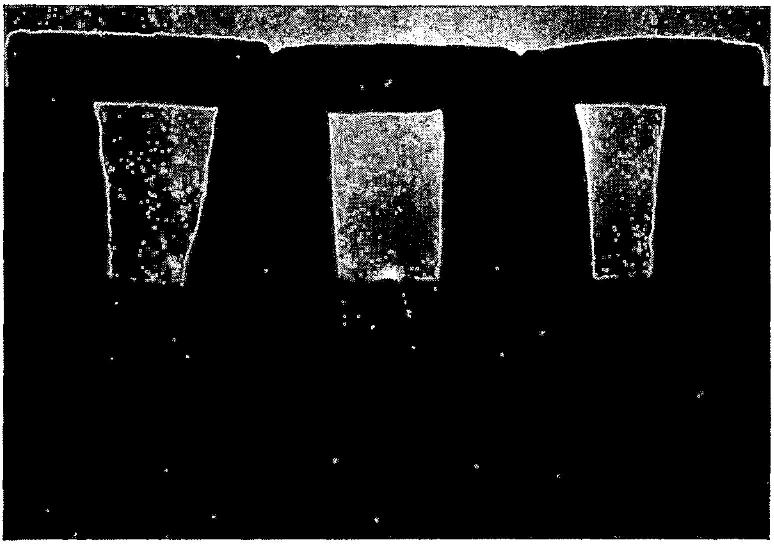


FIG. 5. Sunrise, June 20, 1964.

found among the various alignments, it is not possible to be sure of the exact definition used by the Stonehengers. Indeed, for the placing of 30-ton boulders and naked-eye observations, it is an admirable achievement to point anywhere within the narrow 1° sector between first gleam and last contact. Ironically enough, there was more uncertainty in the modern survey than in the actual setting of the stones. Due north had been determined by Sir Norman Lockyer but there were some ambiguities in his work which were finally resolved with the help of the photograph shown in Figure 5. Even today the skyline altitude is not known precisely for a full 360° at Stonehenge.

We are fortunate to be able to use Stonehenge as an observatory today, 4000 years after its construction. If the Stonehengers had chosen stars, the stones would have become inoperative within a period of a few

centuries. This is due to precession, the conical motion of the Earth's axis which carries the north celestial pole around a circle in the sky once every 26,000 years. The coordinates of a star change by almost 1° per century, and there is a corresponding change in the direction of star rise. But the turning point of the Sun at the solstice is fixed only by the obliquity, the half angle of the apex of the cone swept out by the Earth's axis.

Obliquity does change, but at a slower rate than precession. The tilt angle oscillates between the narrow limits of 23° and 24° with a period of about 40,000 years. In 2000 B.C. the angle was 24°.0; in 1964 A.D. 23°.4. This would make the Sun approximately one diameter higher in prehistory than it is in the picture of Figure 5. It is a relatively simple matter to allow for obliquity, atmospheric refraction, the parallax angle caused by the finite distance of the sun and moon, and other details in an astronomical calculation for an epoch several thousand years ago. It is much more difficult to decide whether trees were growing on the skyline and to what height.

More remarkable than the Sun alignments are the alignments with the position of moonrise and moonset. The Moon travels in its orbit around the Earth once every 29.53 days as measured with the Sun as a reference point. In this period, it appears to follow a path close to the ecliptic and reaches the full phase when opposite to the Sun. Thus, in one month the position of the Moon on the horizon shifts as much as does the Sun during a year. The position changes by about 90° in a span of two weeks and the apparent shape of the Moon changes from day to day. Casual observers would conclude that the behavioral pattern was chaotic.

If we fix upon a particular phase, then the pattern is more orderly. The full Moon, for example, rises as the Sun is setting and is on the opposite horizon. Thus, at midwinter when the Sun is setting along the short sides of the rectangle (91–92, 93–94) the full Moon rises in the general direction of the heelstone as viewed from the center. To carry the antithesis further, the Moon is also opposite in season—in winter it occupies the summer position of the Sun. There are five more space-time antitheses and these are also marked at Stonehenge by the summer, winter, and equinox lines.

Can we be sure that the Stonehengers noticed these simplifications in the motions of the Sun and Moon? A strong affirmative answer can be given. At any particular season the full Moon is exactly opposite the Sun once every nine years or so. In the intervening years the winter full Moon at Stonehenge rises for nine years to the left of the heelstone and for nine years to the right as seen from the center. The end points of this long swing are marked by holes D and F. Similarly the end points of the swing of the rising full Moon in summer are marked by the diagonal and long side of the rectangle 91–93 and 92–93. The 19-year swing of the equi-

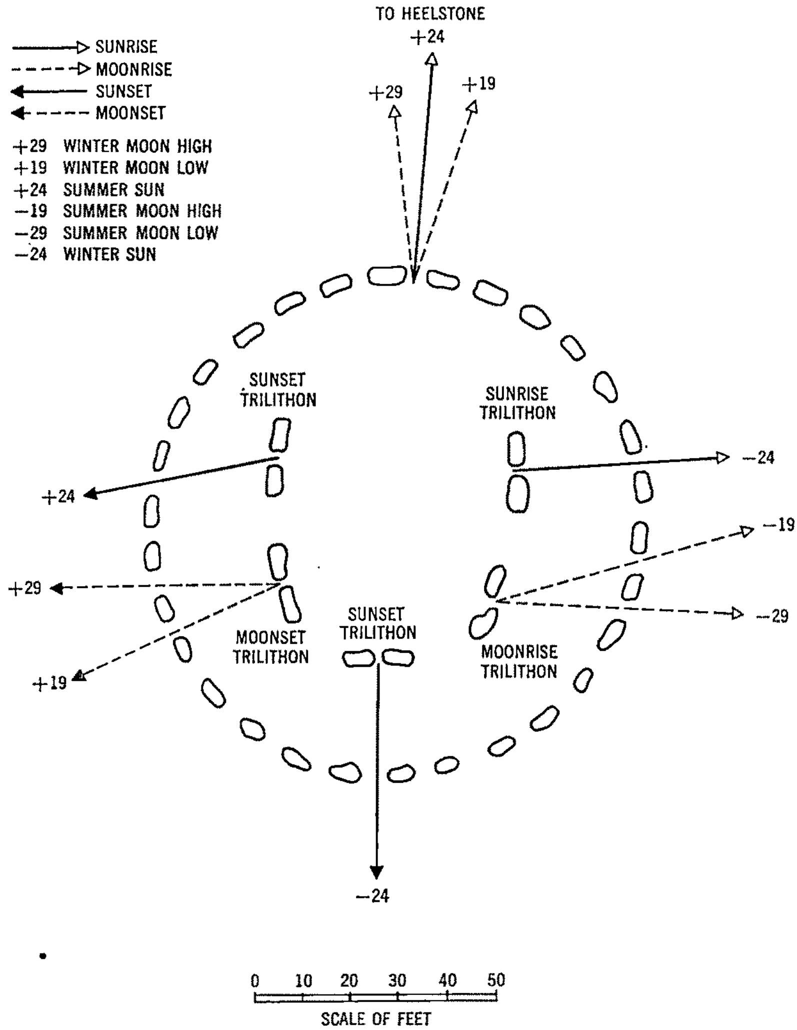


Fig. 6. The Sun and Moon alignments of Stonehenge III.

nox full Moon is marked by the angle B, 94, E, and the swing of the summer moonset by the angle 93, 91, 94.

The Stonehengers have picked out the critical extremes and midway positions of the Sun and Moon. In astronomical terms they marked the declinations given by $\delta = (24n + 5m)$ where n and m are 0, ± 1. The odds that the Stonehengers marked this unique set deliberately are millions to one in favor.

The archways of Stonehenge III follow the same pattern. Looking through the sunrise trilithon, Figures 2 and 6, only one view is possible

and the direction is that of midwinter sunrise. On the other side of the horseshoe, the view is sunset at midsummer and the great trilithon marks sunset at midwinter. There was no need to build a trilithon for midsummer sunrise; it is marked by the heelstone as viewed through the entrance gap in the sarsen circle and this is the view shown in Figure 5. The other two trilithons mark the swing of the full Moon as it rises at midsummer and as it sets at midwinter. The midpoint of the swing of the Moon is marked by the sunrise and sunset trilithons where it is to be seen at intervals of 9 or 10 years. There was no need to mark the winter moonrise swing in Stonghenge III because the original lines of Stonehenge I were still available. Similarly, the equinox sightings from 94 were still open.

This is a remarkable summary of a complex pattern of natural phenomena. We begin to read in the placing of the stones the thought process of the Stonehengers. The human motives for the gigantic undertaking emerge—a calendar to mark the seasons, pointers for following the solar and lunar gods, satisfaction in an instrument ingeniously contrived, artistically designed, and skillfully built. But in these conjectures the anthropologist must take over—what were the intellectual capabilities of *Homo sapiens* (*britannicus*) 4000 years ago?

The British archaeologist, R. S. Newall, reminded me of the allusions to astronomical temples by Diodorus of Sicily, *ca.* 50 B.C. Diodorus refers to an ancient temple in the north dedicated to the sun god. The Moon was observed there and the stars returned to their original positions after 19 years. The priests were called Boreadae and kept their knowledge within the family.

This remarkable legend could so well apply to Stonehenge. The swing of the Moon follows a period of 18.61 years, almost the "19" of Diodorus. More exactly from the practical viewpoint, counting in whole years, the swing averages two intervals of 19 followed by one of 18. Certainly, the observers' positions were strictly limited. Only one person could conveniently look through the archways and over the stones. Who knew what the high priest saw as he worshipped the sun or moon god?—perhaps only those who were in the family. Classicists and prehistorians will probably be able to explore more fully the thread of this legend.

The question of secrecy, occultness, or subtlety is an intriguing one. The obvious axis of symmetry runs north-south. If the stones had been set around one central observation point, the astronomical significance of Stonehenge would have been discovered several hundred years earlier than it was. Sir Edmund Halley, for example, spent much time at the site in the 1720's, and as an astronomer could not have failed to recognize the pattern. But the Stonehengers designed first the rectangular setting and the later circles using the midsummer line as the axis. The basic Sun-Moon pattern is not symmetrical about this axis as

may be seen by folding Figure 7. Yet, by their particular choice of phenomena, they made a symmetrical monument which has been recognized primarily heretofore as an architectural marvel. It has been a mystery in modern times, perhaps it was also mysterious to those who were not within the family.

Newall also argued that Stonehenge might be representative of a culture because one rarely finds isolated achievements in antiquity. The only other megalithic site for which a plan was available to me was Callanish, on Lewis, the northernmost island of the Outer Hebrides.

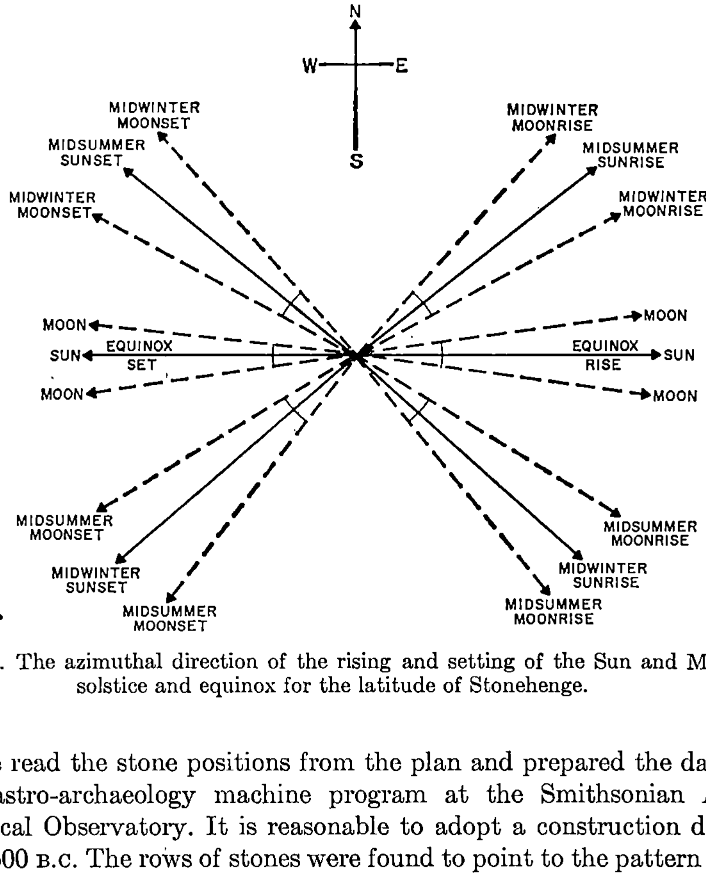


FIG. 7. The azimuthal direction of the rising and setting of the Sun and Moon at solstice and equinox for the latitude of Stonehenge.

We read the stone positions from the plan and prepared the data for the astro-archaeology machine program at the Smithsonian Astrophysical Observatory. It is reasonable to adopt a construction date of *ca.* 1500 B.C. The rows of stones were found to point to the pattern of the Sun and Moon at this period as at Stonehenge. The extreme path of the full Moon at midsummer was the main axis of the monument, and the site seemed to have been chosen so that the midsummer Moon appeared to set into distant Mt. Clisham, the highest peak on the Outer Hebrides. Of course, in this northern latitude the direction of the rising and setting of the Sun and Moon was different from the directions at Stonehenge.

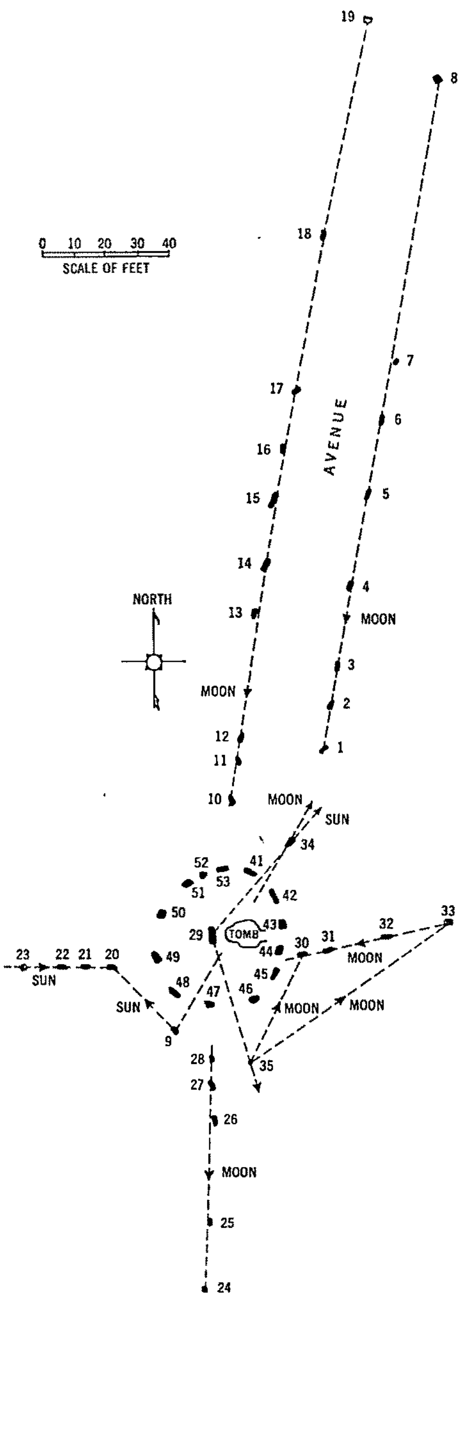


FIG. 8. The Sun and Moon alignments of Callanish, Scotland.

An exact replica of Stonehenge would not have worked at Callanish. The design adopted by the ancient Scots was a distorted cross with triangular wings as shown in Figure 8.

It now seems probable that we have indeed found readable information from the closed book of prehistory. Perhaps other sites will yield fresh information on what seems to be a rudimentary beginning of the scientific method of observation, deduction, abstraction, and prediction. To be sure, these ancient peoples may have regarded the Sun and Moon as mystical gods, but this does not detract from the intellectual and practical achievement. Today, our own generation is reaching for the Moon with more fervor and excitement than a dry, airless 2000-mile ball of rock would seem reasonably to deserve.

Professor Thom has found remarkable geometric and mathematical properties in the megalithic circles in Britain (*New Scientist,* 1964). Many circles are distorted in a manner that makes the circumference exactly three times the diameter. These circles seem to be an exercise to rationalize π. Other stones are set in right angle triangles where the ratio of the sides and hypotenuse are 3, 4, and 5 and other Pythagorean number sets. Many stone settings required the bisecting of an angle. All this Stone Age work was done long before Pythagorus or Euclid.

As an astronomer, I did not know how far it was prudent to carry this investigation. It is a great sweeping generalization to categorize disciplines, but in one extreme corner of archaeology it is believed that when we deal with people who could not write and who had not discovered metals we are dealing with an inferior *Homo sapiens.* Within anthropology, we find the other extreme position that regards *Homo sapiens,* whatever the date in the past may be, to be our equal. I adopted as a working hypothesis the view that anything that I could discover at Stonehenge was probably known also to the Stonehengers. This hypothesis automatically rules out the third possibility, that in some respects they might have been superior to us. This hypothesis led to the discovery of the possible use of Stonehenge as a calculating device for predicting eclipses. It also led to the discovery of the eclipse cycles based on the tropical year which have hitherto been overlooked.

The swing of the Moon marked at Stonehenge is controlled by the regression of the nodes of the Moon's orbit. When the full Moon or new Moon is at one of the nodes, then the Sun, Earth, and Moon are exactly in line. This is the well-known prerequisite for an eclipse. When the full Moon is at a node it passes within the dark shadow of the Earth and, apart from deep red light filtered through the Earth's atmosphere, the full Moon is extinguished. When the Moon is at the other node, then it passes in front of the disc of the Sun. If the alignment is precise, the photosphere is obscured and a total eclipse of the Sun takes place. If the alignment is not exact, then only a portion of the Sun is obscured.

If we take as an example the full Moon at winter, at the center of its swing it rises over the heelstone. It is then exactly opposite the setting Sun and at the node of its orbit. A total eclipse of the Moon will take place. Calculations show that when the winter Moon rises over the heelstone, it is an infallible rule that some type of eclipse will take place. Either the full Moon is eclipsed or, at the time of the new Moon, an eclipse of the Sun occurs. On the average, only half of these eclipses are visible at a fixed spot on the Earth's surface such as Stonehenge because of the rotation of the Earth. But, nevertheless, the full Moon rising over the heelstone in winter would indicate that there was the imminent danger of an eclipse taking place that month. Similarly, the danger of an eclipse during the month of the spring Moon, fall Moon, or the summer Moon was indicated by the other alignments.

Thus I could visualize Stonehenge being an instrument which was useful for giving some warning of the danger of an eclipse and, following my working hypothesis, I presumed that the Stonehengers had noticed this property of the monument. Could I use Stonehenge to predict a particular eclipse with certainty? Could I tell by observation whether on the night of the eclipse it would be visible at Stonehenge or whether it was visible only on the other side of the Earth? Standing at Stonehenge when an eclipse was about to take place I realized that the answer was "yes," and therefore presumed that the Stonehengers were also aware of this possibility. The high priests could, in fact, have predicted the time of an impending lunar eclipse with an accuracy of about one hour.

Critical observations would have to be made on the evening of the suspected eclipse. If the Moon rose less than thirty minutes before the Sun set, then the eclipse would be visible from Stonehenge. The Moon moves eastward towards the Earth's shadow at a rate of $0^\circ.5$ per hour. The Moon rises (and the Sun sets) at a rate of 15° per hour owing to the spin of the Earth. Thus, thirty minutes of time shows that the Moon must travel 6° to be exactly opposite to the Sun, which requires twelve hours for the Moon to cover. In discovering this, of course, I was using our own concept of time, but the Stonehengers could easily have figured in terms of lunar diameters. If the Moon rose after the Sun had set on the night of the full Moon, then the Stonehengers would be able to tell with surety that the danger of the eclipse was past and the phenomenon would not occur.

To investigate this possibility further I computed the position of moonrise for the period 2000–1000 B.C. In a way, I was observing with the Stonehengers and could see with them any patterns that existed in the behavior of the Moon and the Sun. It became quite clear that there was a pattern based upon the tropical year, the year of the seasons. For example, the full Moon nearest to the time of the winter solstice is eclipsed in a repetitive fashion based upon a 56-year cycle. The full

Moon of any other season, such as the spring Moon, the harvest Moon, or the midsummer Moon similarly follow the pattern based upon a 56-year cycle. Once discovered, the fact is obvious. It is the result of the commensurability of the eclipse year with the tropical year. One simply has to find integers, a and b, which come close to satisfying the equation $346.620\ a = 365.2422\ b$. When b is 56 and a is 59, the two periods agree to within four days. Thus, the winter Moon is in danger of eclipse every 56 years and this cycle will be sustained accurately for a period of about 300 years, as shown in Figures 9 and 10. There are other cycles that exist

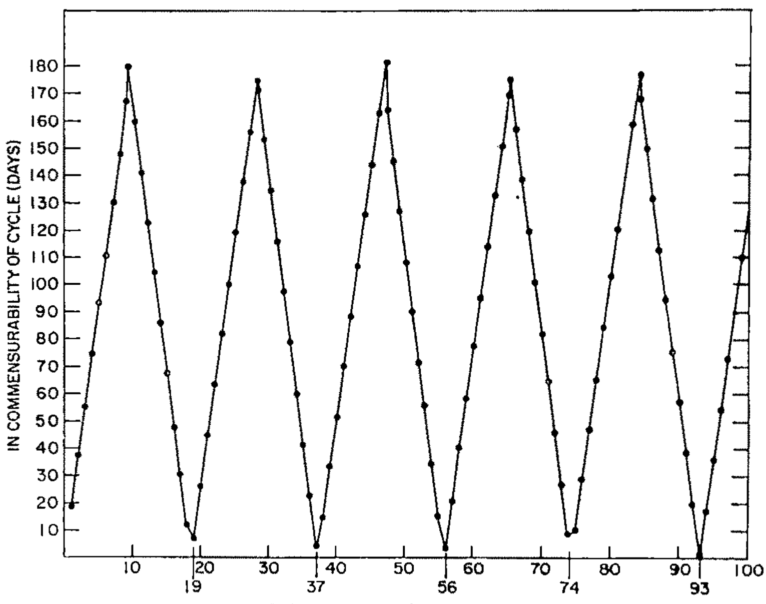


FIG. 9. The commensurability of the eclipse year and tropical year.

at 19 and 37 years but these are not as accurate as the 56. A cycle of 93 years is more accurate; in fact, it would persist for more than 1000 years.

Now archaeologists have never been able to suggest a reason why the circle of Aubrey holes was made to contain 56. In fact, without corroborating clues it would be meaningless to speculate on the number. However, the strong evidence for lunar observations sustained over many years makes it reasonable to suppose that the Aubrey circle contains the information of the nodal regression of the Moon and the seasonal eclipse cycle. Writing 1500 years after Stonehenge, Diodorus seems to convey the information of the cycle but his information is degraded. Three periods of 19 years make 57, and the Diodorus cycle would be one year in error when compared to the more accurate 56.

The Aubrey holes could have served as a crude counting device for predicting the swing of the Moon and the particular year when an eclipse should be expected at a certain season such as winter or spring. There are many ways of using the circle as a computer. One way is to set six stones or other markers at the holes as shown in Figure 11. Every year at a particular date, say midsummer's day, the stones are each moved counterclockwise to the next hole. When a white stone is at Aubrey 51 or 5, then eclipses are to be expected in the spring or fall. When a black or a white stone is at Aubrey 56 then eclipses will occur in the month of midwinter or midsummer. The circular computer would follow the moon cycle accurately year after year for three centuries.

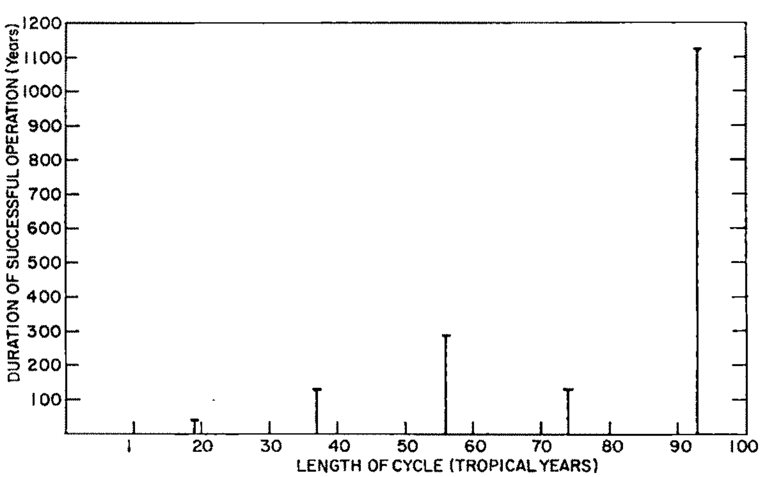


FIG. 10. Success rating of allowable seasonal eclipse cycles.

Then eclipses would begin to occur one year ahead of the computer, and matters could be put right by advancing all stones by one hole.

If Diodorus was referring to Stonehenge as it existed 1500 years before his time then his information was incomplete. The accepted translation of "stars" makes no sense astronomically because the constellations return to their seasonal positions in one year as was well known to the Greeks. Certainly, he makes specific reference to the Moon in the same context and the 56-year cycle is composed of two periods of 19 and one of 18. Perhaps Diodorus or his source of information had lost the Boraedae knowledge of the short cycle at the 56th year.

Astronomically, all the circles and the horseshoe at Stonehenge make sense. The 30 sarsen archways would serve as a counting device for predicting the phase of the Moon. One has simply to shift a marker from one arch to another each day. The Aubrey computer tells the year of a possible eclipse, the sarsen circle tells the day.

The number 30 is the nearest integer to the length of the month which is 29.53 days on the average. It was well known in classical times that a better approximation was to alternate the day count, 30, 29, 30, 29, etc. There are 30 *Y* holes and 29 *Z* holes which are late additions to Stonehenge. They could have been used for a better approximation to the prediction of the phase of the Moon. The bluestone circle contained originally between 59 and 61 stones, the exact number is uncertain. I sug-

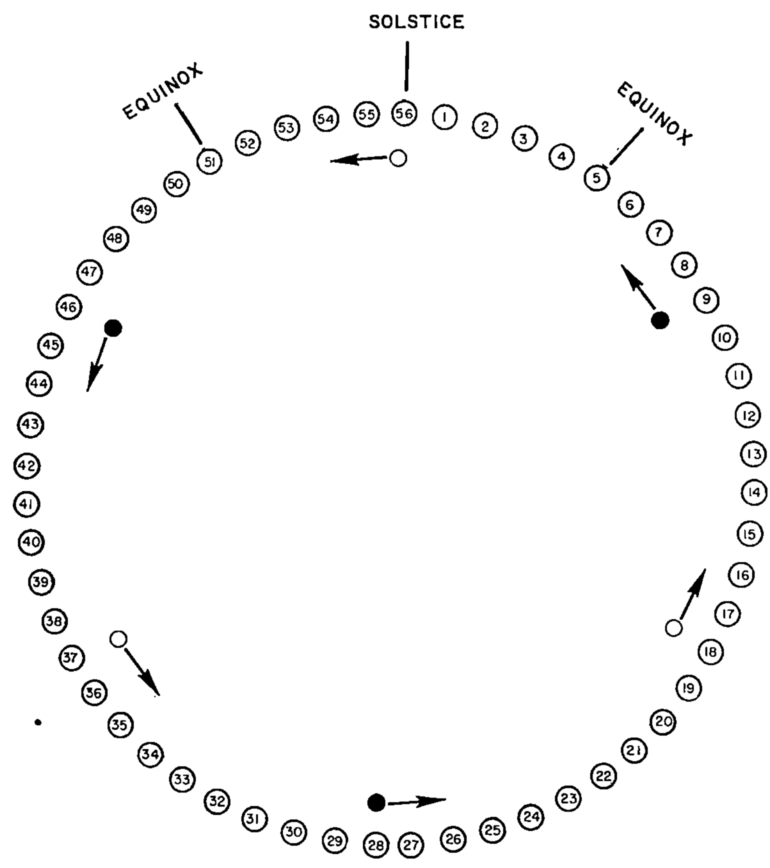


FIG. 11. The Aubrey circle as an eclipse predictor.

gest the number was 59 so that by moving a marker each morning and each evening the computer would allow 29.5 days to the month and keep in step with the Moon with remarkable accuracy.

The bluestone horseshoe contained 19 stones, one of which was the unusual micaceous sandstone block now called the altar stone. Thus, the horseshoe provided another counting device for marking the passage of a 19-year cycle. By omitting the altar stone every third cycle, the necessary 18-year cycle could also be marked.

At this point the working hypothesis seems to make extreme demands. There is certainly a great deal that is possible at Stonehenge, but how much of it really was known to the Stonehengers? It would require at least one 56-year cycle of observation to find the pattern, and several more cycles to confirm the result. For a few centuries the rising and setting of the Sun and Moon must have been noted day by day and the information memorized and transmitted verbally from one generation to the next. This information is equivalent to a comprehensive astronomical ephemeris and would represent a considerable portion of the total knowledge of the neolithic culture.

Archaeologically, the Aubrey holes are thought to be an early feature of Stonehenge I, but astronomically the holes would be easier to understand if they were dug much later. Archaeologically we must conclude that the designers and builders started at Stonehenge with the problem virtually solved and the facts established. Astronomically, we would prefer to see the problem solved over the years based on data obtained during the construction of Stonehenge I. The chronological evidence from archaeology is that the ditch of 92 cuts through Aubrey 19, but the evidence is not fully convincing.

If this amount of knowledge existed so early in Britain, then it is to be expected that similar achievements await discovery in other areas, particularly the Mediterranean. The Callanish site is on an island and the type of tomb found there is similar to others found on the coasts of the Irish Sea. It is presumed that ocean passages were possible *ca.* 2000 B.C. and the voyagers would naturally be expected to carry information from one point to another. At the moment, however, the astro-archaeology of Callanish and Stonehenge, particularly the latter, stands alone.

The solar-lunar knowledge contained in the writings of the classical Greek scholars is much less than seems to be contained in the placing of the stones at Stonehenge. Even the Babylonian tablets do not treat the problems of the movement of the Moon until *ca.* 700 B.C. Stonehenge is therefore isolated in time as well as isolated geographically. On the basis of the present evidence one might conclude that the intellectual achievement at Stonehenge was lost when the structure fell into disuse *ca.* 1400 B.C.

As I write this account, Professor G. de Santillana brings some words of Plutarch to my attention that may have some bearing on an attempt to retrieve prehistoric knowledge.

Plutarch· De Iside et Osiride

ca. 44: There are also some that will have the shadow of the earth, into which they believe the moon to fall when eclipsed, to be called Typhon.

ca. 30: It is also most apparent that the Pythagoreans look upon

Typhon as a daemoniac power; for they say he was produced in an even proportion of numbers, to wit, in that of 56. And again, they say, that the property of the triangle appertains to Pluto, Bacchus, and Mars; of the quadrangle to Rhea, Venus, Ceres, Vesta, and Juno; of the figure of 12 angles to Jupiter; and of the figure of 56 angles to Typhon; as Eudoxos relates.

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ILLUSTRATION CREDITS

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Fig. 5. From CBS-TV News, "Mystery of Stonehenge," courtesy of CBS.

AMERICAN SCIENTIST, *53*, 1965

THE NEW LOOK IN MOTIVATION

By WILLIAM N. DEMBER

ABOUT a decade and a half ago, when I began graduate training in psychology, one of the most exciting research topics concerned the effects on perception of motivational and cognitive variables. For example, evidence was presented that showed recognition thresholds for words to be affected by the motivational significance of the words; thus, words with positive or pleasant connotations might have lower thresholds —that is, be correctly recognized at a shorter presentation duration— than neutral words, whereas the recognition of negatively toned or taboo words required longer durations of presentation than those needed for the recognition of neutral words. Other research showed the perceived size of an object to be related to its value. Thus, children tended to overestimate the size of coins relative to discs of neutral value, and poor children were more subject to this type of error than were rich children. By now, a host of such studies has been conducted; with each passing year the methodology employed gets a little more sophisticated and the results a little more equivocal. But that is a different story.[1] The point for the present purpose is to note that this approach to the study of perception—which became known as the New Look in Perception—emphasized the interaction between processes which psychologists had in general hitherto been careful to keep in separate conceptual categories, and, in the textbooks, in separate chapters.

To speak of a New Look implies an "old look." In the case of perception the old look was one in which the variables influencing perception were thought to reside in two sources: (1) the physical stimulus and (2) the sensory system, consisting of receptor organ, afferent nerve and subcortical and cortical sensory projection areas. The New Look in perception was not antineurophysiology; rather, it postulated kinds of interactions within the nervous system that were more elaborate and diffuse than those allowed for in the classical neurophysiology and neuroanatomy of the nineteenth century. Interestingly enough, direct evidence for some of the neural mechanisms consistent with the New Look has been reported, as, for example, in the discovery of the cortical priming function of the reticular arousal system.[2]

Let me make one last remark about the New Look in Perception before I turn to its analogue in motivation. And that is to note that the New Look was not entirely new. Similar ideas had been proposed earlier

[1] For a review of this literature, see Dember (1960).
[2] See, for example, Samuels (1959).

by such diverse theorists as Freud and John Dewey. What made the New Look novel was its attempt at empirical verification of its postulates.

The New Look in Motivation

What about the New Look in Motivation? From the sense of my previous remarks you might anticipate, first of all, that the New Look in Motivation will be characterized by an emphasis on *interaction* between motivational and perceptual and cognitive processes. Secondly, you might expect to hear something about the "old look" in motivation. Finally, you should not be surprised to learn that the New Look in Motivation has a long history of its own, and that what might better be

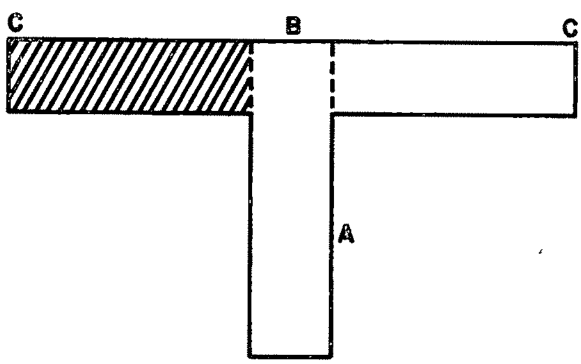

Fig. 1. The T-maze, consisting of (A) starting alley, (B) choice-point, and (C) goal arms. The dashed lines indicate guillotine doors, which can be lowered to prevent retracing.

called the "latest new look" is characterized not so much by its theoretical postulates as by its attempts at empirical verification.

Now, rather than elaborate on these ideas at this point, I would prefer to defer their discussion until later, and launch immediately into a description of some of my own research experience in this area.

Alternation Behavior

Let me begin where I began—with a phenomenon called "alternation behavior." This behavior has been studied almost exclusively in rats, but has also been observed in other species, including man. It can be described very simply with the aid of the first figure.

Figure 1 depicts in plan view a piece of apparatus that is popular among those who investigate the behavior of rats. It is called a T-maze, and consists of: a starting alley; a choice-point region; and two goal arms. Frequently, guillotine doors are located at the juncture of the goal arms and the choice-point so that once the rat has entered an arm it can be prevented from leaving that arm until the experimenter is ready to

remove it from the maze. Depending on the purpose of a particular experiment, the goal arms may be kept identical in stimulus properties, or specific differences between them may be introduced, as, for example, by painting one arm black and the other white. For the experiments which I will first describe, let us assume that the left arm is black and the right arm is white.

The T-maze is typically used in studying *learning* in the rat, and usually in the following manner. The rat is first deprived of some commodity to establish a state of physiological need and a concomitant state of "psychological drive"; for example, a schedule of food deprivation might be instituted in order to make the rat hungry. Then the rat is introduced into the T-maze; at the end of one of the goal arms a bit of food is located. If the rat enters that arm, it is allowed to eat the food and is then removed from the maze. Such "trials" are repeated until the rat has reached a criterion performance level—perhaps entering the proper arm on 10 successive trials.

Such a problem, though it may seem simple to us, will usually not be solved immediately by the rat. Indeed, it may take 20 or 30 trials before criterion is reached. The gradual improvement in performance that occurs over trials, as evidenced by the behavior of a large group of rats, is considered to reveal the gradual establishment of a habit. One can then study the variables that influence habit acquisition; for example, strength of hunger drive during training, amount and quality of the drive-reducing commodity (i.e., the reward or reinforcement), the distribution of training trials, and a host of other variables, limited only by the imagination and ingenuity of the experimenter.

Now, in pursuing such an experiment about 30 years ago, Wayne Dennis (1935) noticed something unexpected. He had assumed, along with other researchers, that the acquisition of the habit—say, of always turning right in the maze—was superimposed on an initially random process. That is, the rat's behavior on the first few trials at least was expected to be unsystematic. But Dennis found it to be quite the contrary. In particular, on examining the records of the early trials, he discovered the phenomenon of *alternation:* that is, if a rat made a right turn, for example, on the first trial, the probability was high that it would turn left on the second trial. Incidentally, this will happen, on the early trials, even if the rat finds food on the first trial. Thus, the behavioral stereotypy which emerges during the course of learning is superimposed on an already existing and strong tendency toward behavioral variability, and variability that is *systematic,* not random.

What Dennis did next, as any good scientist would, was to put aside his investigation of learning and to explore the alternation phenomenon. I have reviewed his and other work on alternation elsewhere (Dember and Fowler, 1958; Dember, 1961) and will not burden you with the details of

all those investigations. What is significant for this presentation is the assumption that Dennis made about *what* the rat was alternating with respect to. According to Dennis, the rat was alternating with respect to the *maze arms*.

What other possibilities might there be? When an animal is observed entering the black goal arm, one could describe that bit of behavior by reference to the arm, as Dennis did, by saying that the rat entered the *black* arm, or the arm on the left, or perhaps the arm oriented toward the west. All such statements focus on the part of his environment with which the rat's behavior brings him into contact. A different kind of assertion would focus on the behavioral act itself, and say that the animal made a left-turning *response*.

If, on the next trial, the rat alternates, one could say, again with Dennis, that the rat was alternating with respect to maze arms (he went into the black arm on the first trial, and into the white arm on the second). Or, from the other point of view, one could say that having made a left-turning response on the first trial (which happened to bring the rat into the black arm), the rat alternated by way of a right-turning response on the second trial (which happened to bring him into the white arm).

Dennis's view, which sees alternation as stimulus-alternation, not only has historical priority; it also would seem the more natural of the two accounts. Strangely enough, however, the response-alternation point of view was the one that prevailed.

This happened, and quite obviously to one who is familiar with American psychology, because there existed a dominant behavior theory which had the concepts available to allow the deduction of response-alternation, i.e., Hull's (1943) learning theory. Within Hull's theory is contained the concept of "reactive inhibition"; it is the function of the concept of reactive inhibition[3] to account for the process of experimental extinction and related phenomena—that is, the process whereby the animal stops making a learned response when that response is no longer followed by reward. In essence, reactive inhibition is a hypothetical quantity which grows each time a particular response is made. The size of the increment to reactive inhibition is a direct function of the amount of effort required to make the response and also a function of how many times the response has been made. An additional property of reactive inhibition is that it spontaneously dissipates over time. The behavioral effect of reactive inhibition is to decrease the tendency to make the response that gave rise to it. If you think of reactive inhibition as a fatigue-like state, its properties and effects may become more readily apparent.

The application of Hull's reactive inhibition concept to the case of alternation behavior is straightforward. Whenever a response is made—

[3] An additional concept, conditioned inhibition, is necessary for a complete account of the extinction process.

say, a left turn in a T-maze—a certain quantity of left-turning, reactive inhibition is built up. Given a second trial, the animal will be somewhat less inclined than previously to make that response, and given no other compelling tendency to turn left, it will turn right by default, and thus will alternate. In these terms, alternation reflects the animal's attempt to minimize the aversive consequences of its own responding. At this point, one might wonder why an animal behaves at all—why make that first left turn or the second right turn? Why not stay put in the starting alley? The answer, I believe, from the Hullian point of view is that the animal *would* be entirely quiescent were it not for the behavior-arousing physiological drives. That answer, by the way, expresses the essence of the "old look" in motivation, whether it be the version of the animal psychologists, such as Hull, or of the classical Freudian psychoanalysts.[4]

To continue with the alternation story, recall that two accounts of alternation have been proposed, which we will refer to as the *stimulus-alternation* hypothesis and the *response-alternation* hypothesis. The response-alternation hypothesis was for several decades the generally accepted account, largely because of its nice fit within Hullian theory, but also because it allowed for some testable derivations that were empirically confirmed. For example, the response-alternation hypothesis predicts decreasing probability of alternation with increasing time between the two trials; it predicts increasing probability of alternation with increases in the number of forced turns to one side of the maze prior to a free-choice trial. These and other predictions were confirmed, lending additional credence to the response-alternation hypothesis, but also additional impact to the experiments next to be described.

Since the conflicting hypotheses were designed to account for the same phenomenon—i.e., alternation in the T-maze—any attempt to choose between them must necessarily be based on some variation from the standard procedure. A beautiful set of experiments was finally conducted by Murray Glanzer (1953*a*; 1953*b*) that permitted a clear choice between the opposing hypotheses.

Glanzer's theoretical position was derived from Dennis's. It asserted that alternation occurred with respect to environmental stimuli, not to the rat's own prior responses. According to the theory, any time an organism is exposed to a stimulus, a quantity, called "stimulus satiation," is built up which has the effect of decreasing the probability that the organism will respond to that stimulus on future occasions. Note that this postulate is *formally* just like the reactive inhibition postulate in Hullian theory. Indeed, Glanzer has endowed stimulus satiation with all the properties of reactive inhibition; for example, it accumulates with increasing exposure to a stimulus; it spontaneously dissipates in time, and

[4] Robert White (1959) has most clearly drawn the parallel between motivational concepts, old and new, in academic psychology and psychoanalytic theory.

so forth. As a result, Glanzer's theory can predict equally as well as Hull's the outcome of the experiments mentioned earlier relating to the interval between trials, number of forced turns in the goal arm, and so on.

Beyond this, Glanzer devised some situations for which the two theories make opposing predictions. One of these situations made use of a cross-shaped maze, as depicted in Figure 2. On a given trial, the

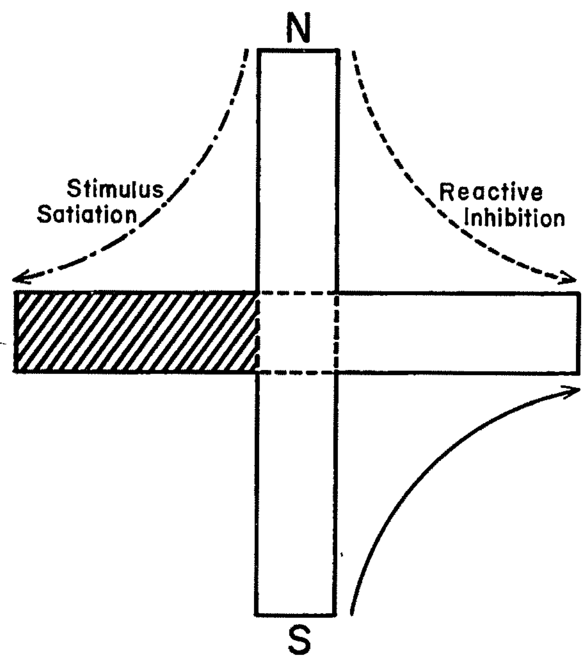

FIG. 2. The cross-maze. The solid arrow represents the behavior of an animal on trial-one. Behavior predicted on trial-two is represented by broken arrows, one for the reactive-inhibition prediction, the other for the satiation prediction.

maze is used as a T, with one of the two possible starting alleys blocked off at the choice-point. The two trials which any animal runs, however, are made from opposite starting alleys. For example, if on trial-1 a rat is started from the south starting alley, it will start its second trial from the north.

Now, consider the rat, who, starting from the south, turns right on the first trial. According to reactive inhibition theory it should make a left-turning response on the second trial, regardless of the alley into which that response takes it. But, to alternate responses in the cross-maze, the animal must repeat maze arms.

According to satiation theory, that same animal, having been exposed to the white arm on the first trial, should avoid that arm on the second trial, and therefore should enter the black arm, even though that means repeating its previous right-turning response. The experiment suggested by these arguments was done and clearly confirmed Glanzer's prediction. The rats were stimulus alternaters, not response alternaters.

A second, and equally elegant experiment, was conducted, based on the following argument. According to the Hullian theory, reactive inhibition following a response dissipates over time. Thus, if a rat makes a right turn and is then confined in the maze arm instead of being immediately removed, the reactive inhibition generated on the first trial should decrease in amount. A second trial, following a long delay in the maze arm, should be less likely to exhibit the alternation tendency than an immediate second trial. In addition, it should not matter where the rat spends its time during the delay. A long wait between trials outside the maze should have the same effect on probability of alternation as a long delay within the maze arm.

From the point of view of satiation theory, it matters considerably where the rat spends its time, but matters little how it got there. Indeed, a long delay in a maze arm is really a longer than usual exposure to the to-be-satiated stimulus; the longer the exposure, the greater the satiation; the greater the satiation, the greater the probability of alternation. Thus, rats delayed in the maze arm should alternate more than rats given an immediate second trial, and rats delayed in the maze arm should alternate more than rats given an equally long delay outside the maze. Again, the data clearly confirmed Glanzer's predictions. Rats are stimulus alternaters, not response alternaters.

The dramatic success of Glanzer's satiation theory made it worthy of further inspection and test. One such test, the first of the studies in which I was directly involved, examined the validity of the assumption that exposure to a stimulus *per se* is a sufficient condition for inducing alternation behavior (Walker, *et al.*, 1955). For these experiments a simple modification was made in the standard procedure: instead of walking into one of the goal arms on the "exposure" trial, rats were *placed* in the arm by the experimenter. After an appropriate length of exposure, the animals were removed from the arm and given an immediate free-choice trial. Several variations of this procedure were run, but in no case was there any evidence that this "passive exposure" to the goal arm stimuli had an effect on the rats' subsequent behavior. At that point it looked as though alternation depended on the rats' making an active choice on the first trial.

Several years after those initial failures to find alternation following passive exposure, the effect was obtained in experiments by Glanzer, by myself, and by others. Why those earlier studies did not reveal the

effect remains a mystery.[5] In a way, however, that failure was fortunate, for it led to a pair of new experiments which are of great significance, at least for purposes of this presentation.

Response to Change

The first of the pair was based on the following argument. In the previous passive exposure experiments the animals were not in direct contact with the stimuli at the juncture of the choice-point and the goal arm. But that is the place at which the choice was to be made on the second trial. Thus, it would seem a fairer test of satiation theory if the first trial were so designed that the rat's exposure to the goal arm stimuli occurred within a context that included the choice-point region.

That argument was translated into an experiment (Kivy, Earl, and Walker, 1956) for which the standard T-maze was modified slightly by employing transparent glass doors instead of the usual opaque doors to block off the goal arms from the choice-point. These glass doors were already in place when the exposure trial began. When the rats were introduced into the starting alley, they could wander up to the choice-point and peer through the glass doors into the goal arms, but could not enter either arm on that trial. What the rat saw on the exposure trial was two goal arms of the same brightness, either both black or both white. The animals were given lengthy exposure trials to assure adequate satiation; they were then taken out of the maze, the glass doors were removed, and one of the goal arms was changed in brightness.

The design of the experiment is given in Figure 3. It was expected that if Glanzer's theory were correct, and if this version of the passive exposure procedure were the appropriate one, then those rats exposed to black goal arms on the first trial would enter the white arm on the second trial, and *vice versa.* And that is exactly what happened.

Before discussing the second experiment of this pair, let me recall a comment I made earlier about Glanzer's theory—that it was formally identical with the reactive inhibition theory, with the exception of the assumed source of the alternation tendency. The formal identity that I referred to was related to the postulates about the growth and decay of the inhibiting quantity. But there is another way in which the two theories are identical: for both, alternation behavior represents an *avoidance* response. For both, the animal does what it does on trial-two because it cannot do what it did on trial-one. Hull's rat turns left because, in some sense, it is "tired of making right turns"; Glanzer's rat enters the black arm because it "can't stand seeing whiteness," not because it finds the black arm attractive.[6]

[5] The mystery may have been cleared up, and indeed the whole problem of alternation behavior reopened, by an exciting group of experiments recently reported as a doctoral dissertation by Robert Douglas (1964).

[6] For a newer and more sophisticated analysis of alternation theories than the one offered here, see O'Connell (1965).

Now, during the time when we were working on these studies with Professor Walker, Robert Earl and I were beginning to develop some theoretical ideas about motivation which I intend to discuss more fully a little later. These ideas were stimulated in large part by the work I have been describing, but they also derived from a variety of other sources, including our own introspections. One notion we had was that it was inappropriate to consider the choices an animal or a person makes to be determined exclusively by avoidance tendencies. This might prevail in some instances, but there must also be cases in which an alternative is chosen because it has positive valence, and positive in an absolute, not just a relative sense.

	Maze Arm left	Maze Arm right
Exposure	W(B)	W(B)
Test	B(W)	W(B)

FIG. 3. Design of the Kivy, Earl, and Walker experiment. On the exposure trial, the arms of the T-maze are both black or both white and the rat views them through glass partitions; on the test trial, the animal can choose between a black and a white arm.

To develop this idea further, it was necessary to specify the properties of a stimulus object that would make it attractive to a given individual. In somewhat different language, our task was to specify the properties of what the learning theorists variously called "goal objects," "incentives," or "rewards." Within classical motivation theory, such objects were characterized either by their ability to reduce a physiological need or by their having been associated with such need-reducing objects. The former class constituted the so-called *primary rewards,* the latter class, the *secondary* or *acquired* rewards. Between them, the primary and secondary rewards comprised the entire set of goal objects.

Now, it was our notion—and not ours alone, of course[7]—that this conceptualization of motivation and reward was incomplete; that there were goal objects other than those that reduced physiological need states.

[7] We were working within a tradition begun in recent decades by, among many others, Berlyne (1954, 1960), Harlow (1953), and Nissen (1954) and continued most recently by Munsinger and Kessen (1964), and Walker (1964).

Moreover, we believed that the slack could not adequately be taken up by reference to a vaguely specified set of acquired rewards.

Let me postpone completion of this level of discussion and return temporarily to the real world of rats and mazes, beginning with another brief look at Figure 3, in which the Kivy experiment is diagrammed. While that experiment was designed as a test of Glanzer's satiation theory and the results fit the satiation prediction, an alternative interpretation of what was going on in that experiment could be offered. It was my thought that what the rat was doing when it entered the white arm, after having been exposed to two black arms, was not avoiding further exposure to blackness; rather, on approaching the choice-point and noting a marked change in one of the goal arms—for example, the one changed from black to white–the rat responded to the change by entering the changed arm and exploring it. I refer to this interpretation as the *response-to-change* hypothesis.

But an alternative interpretation is not worth much if all it can do is predict experimental results that have already been obtained. To be taken seriously it must make unique predictions and preferably predictions that are opposite to those derived from the original theoretical position. What can the response-to-change hypothesis predict that will differentiate it from the satiation hypothesis?

As so often happens, the answer to that question came by way of a mistake I made while describing the Kivy experiment to a psychology class. What I did, in effect, was to reverse the stimulus conditions that prevailed in the Kivy experiment; my erroneous description would be represented by the diagram in Figure 4. Here, on the exposure trial the two arms are different—one black and one white, and on the test trial they are of the same brightness, both black or both white. My initial response to the error in presentation was to apologize to the class, erase the mistake from the blackboard, and do it properly. Several hours later, while I was perseverating on this error, it struck me that my erroneous diagram provided the design for an experiment that would answer the question: how can the response-to-change hypothesis be differentiated from the satiation hypothesis? What would each of the two hypotheses predict were the experiment diagrammed in Figure 4 actually run?

First consider the satiation hypothesis. On the exposure trial, the animal is satiated for black and for white; on the test trial it is offered a choice between, say, two black arms. The satiation induced on the exposure trial would not bias it against either the black arm on the left or the black arm on the right. In short, the choices made by a group of rats should be distributed independently of the rats' satiation experience.

The response-to-change hypothesis makes a different prediction. The

rat which saw black on the left and white on the right on the exposure trial and is then faced with two black arms on the test trial should be attracted to the *changed* arm—in this case the black arm that used to be white; that is, the arm on the right.

The experiment suggested by this analysis was run (Dember, 1956), using the Kivy apparatus and generally following his procedures except for the configuration of the stimuli on the two trials. The rats' behavior conformed to the response-to-change prediction, and this result has since been replicated by other investigators.

The outcome of this experiment greatly encouraged Robert Earl and me to pursue the theoretical developments that we had been working on.

	Maze Arm	
	left	right
Exposure	B(W)	W(B)
Test	W(B)	W(B)

FIG. 4. Design of the Response-to-Change experiment. Conditions are just the reverse of those in the Kivy, *et al.*, experiment.

Rather than dwell further on the details of where the ideas came from, I propose at this point to sketch the product of this thinking, which took the form of both some general hypotheses about motivation (that we called the Theory of Choice) and some specific experimental tests of these hypotheses.

The Theory of Choice[8]

From the experiments that I have described, from many that I do not have time to mention, and from the set of observations which form the base of those nonexperimental approaches to knowledge about man and his motives—such as philosophy and art—it is clear that the direction of an individual's behavior is at least partly under the control of the stimulus objects and events surrounding him. It is clear also that one need not be suffering from physiological imbalance, or anticipating that state, to become interested in those external objects; it is clear that

[8] The theory is presented more formally and in greater detail in Dember and Earl (1957), Earl (1957, 1961), and Musselman (1963).

these objects of interest need not be potential restorers of physiological balance, nor even associated with such servants of the homeostatic process, in order to exert powerful impact on the individual's behavior.

What are these objects and events? For a rat, a change in the brightness of a goal arm in a T-maze; for a monkey, a toy railroad train running on a table top; for a human baby, a rattle, its own fingers, plastic birdies swinging on a mobile over its crib; for the human adult, a painting, a puzzle, a pin-ball machine, a poem (I am tempted to add "a partridge in a pear tree," perhaps to preserve the alliteration, but maybe also because it typifies a class of objects, characterized by incongruity, that has considerable impact on the direction of behavior).

I have used the word incongruity; the items in the preceding list call to mind other terms, such as novelty, complexity, movement, uncertainty, ambiguity, and so on. There are specific objects and events, impinging on specific individuals at particular moments which might best be labeled by one of these terms, rather than another. For example, the rat entering the changed goal arm is responding to novelty; the toy railroad train is characterized, for the monkey, by movement; the puzzle by uncertainty, and so on. Rather than dwell on these fine distinctions, the Theory of Choice seizes on what they appear to have in common, as objects or events, and on the way that they influence behavior.

As objects or events, the items in the list share the property of being bearers of *information.* I use the word in the technical sense of Information Theory; these are objects or events which are unexpected, nonredundant, and which, when they occur, therefore reduce uncertainty. With regard to the perceiving individual, these highly informative objects or events have the effect of arousing his interest, directing his attention, consuming his time and energy—at least if he is free to let them.

The Theory of Choice asserts, then, that every object (let us drop the word, event, for convenience) can be assigned an information value, or a "complexity value," as the theory was originally proposed; moreover, the theory asserts that the complexity of an object is a motivationally crucial property of that object, so long as that object is functioning as a goal object, and not simply as a means to another end.

The next step in the development of the theory is hard to convey clearly without a lot of attendant discussion. It asserts that each individual can also be assigned a "complexity value." What we have in mind here is that for each individual there is some highest level of complexity, in the range of objects that he might encounter, which he is equipped to deal with comfortably and effectively. The individual's complexity level is called his "ideal."

Some objects would *for him* be excessively complex; some would be too simple; some would be just right, as the baby bear's bed was for Goldilocks. Objects that are too simple elicit boredom if they are un-

avoidable; objects that are too complex elicit fear, anger, and sometimes rage if one is forced to maintain contact with them.

Quite obviously, then, if the individual is free to choose, he will prefer to encounter objects of a complexity level that matches his own. He will *select* them from among others if they are made available to him; he will *seek* them out if they are absent; he will *work* for them; and he will even *learn* what he must do in order to obtain them, all of these (select, seek, work, learn) behaviors that in classical motivation theory serve as indicators of the presence of a primary or secondary reward.

While the theory expects that the individual will maintain considerable contact with objects of ideal complexity in preference to objects of much lower or higher complexity, it also recognizes that nonideal objects will also be sampled by the individual. Indeed, it seems to be the case, as evidenced by some results that Earl (1957) obtained in his doctoral dissertation, that the modal amount of time goes not to the ideal stimulus, but to one that is just a little more complex than the ideal.

We refer to that object as a "pacer," and attribute to it a special function. It is the pacer, if one is available, that enables the individual to change his ideal. As he maintains active contact with the pacer and eventually masters it, his own level of complexity grows, and he is ready for a new pacer and eventually a still higher ideal.

Of course pacers are not always available, and there are probably some neurophysiological limits to perceptual and cognitive growth, but, in general, the theory takes the optimistic position that under the proper circumstances an individual's complexity level can keep increasing and that learning can be achieved neither as a boring chore nor as a fearful burden. Indeed, the New Look in Motivation—of which the Theory of Choice is but a glance—has already invaded the classroom, or so I gather from a recent article by Robert Gross published in the *New York Times Magazine*, for Sunday, September 6, 1964. The article is about innovations in teaching techniques, and in one brief paragraph the author says "...Another element is a new consensus on motivation. Cumulative evidence from many sources—animal experiments, studies of how children learn, everyday observations of the way they really behave—has shown up the inadequacies of old notions of motivation, based on reward and punishment, by demonstrating that human beings are born with the desire to know, the urge to explore and to master their environment, to achieve . . . "

The ideas contained within the Theory of Choice are not especially novel. As I implied earlier, they can be found in the writings of many authors. For example, I was both dismayed and delighted to discover some very similar notions in the works of Herbert Spencer and Thomas Brown, two nineteenth-century philosopher-psychologists. The major innovation provided by the theory—I said this also about the New Look

in Perception—is its attempt at empirical verification, which in turn implies an attempt at operational definition of its concepts, such as complexity, pacer, etc., and a related concern for measurement.

Some Tests of the Theory

A brief account of two experiments will serve to illustrate both the problem of measuring the concepts of the theory and the beginning steps that have been taken to solve that problem. In addition, the two experiments will reveal how the theory can be generalized to cover the phylogenetic range from rats to college students and stimulus materials ranging from visual patterns to poetry.

The theory was first tested in an experiment with rats (Dember, Earl, and Paradise, 1957). A new piece of apparatus was constructed, the figure-8 maze, as illustrated in Figure 5. The rat, after several days

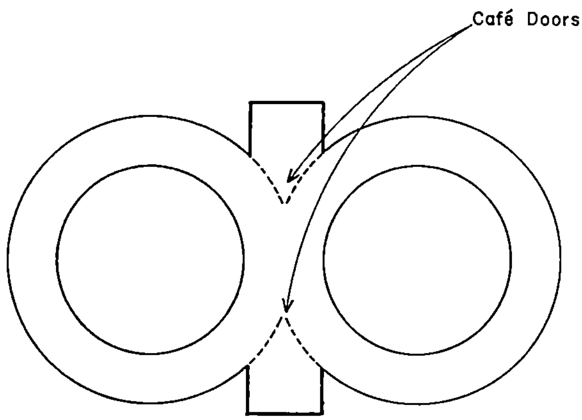

Fig. 5. The figure-8 maze. The rat is placed in one of the boxes at the juncture of the two loops, and enters the maze by pushing through one-way café doors.

of taming, was introduced into the maze and then left there for about an hour. One loop of the figure-8 was lined with horizontal black and white stripes; the other loop was lined with vertically oriented stripes. The two loops were equal in total brightness. However, as the rat moved through the vertically striped loop, it experienced more changes in illumination that it did when moving through the horizontally striped loop. We assumed that, for the rat, the greater the number of illumination changes, the greater the perceptual complexity. Note that our assessment of the relative complexity of the two alternatives was based on intuitive judgment, and though derived from many years of intimate contact with rats, our judgment is ideally no substitute for a more direct measurement.

Now, the theory enabled the following prediction: any change in the rat's preference for one of the loops, as indexed by the amount of time it spent in each loop over two consecutive daily one-hour sessions, would be in the direction *from less complex to more complex*. The basis of this prediction can be shown with the help of Figure 6. The rat that enters the maze with an ideal at C would have an initial preference for the more complex, i.e., the vertically striped, loop and would maintain that preference, as long as it kept behaving in the situation. Rats entering at A would initially prefer the less complex loop; in the course of sampling both loops, however, their ideals might grow until they are close enough to the complexity of the vertically striped alternative so that it became preferred. Once that happened, if it did, a fixed preference for

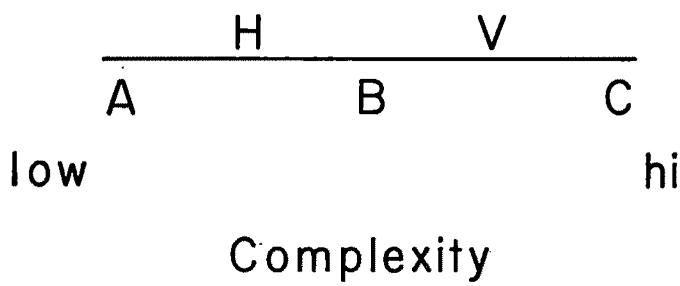

FIG. 6. Two stimuli, H and V, and three hypothetical rats, arranged according to their complexity values. See text for explanation.

the more complex loop would be evidenced. Rats entering with ideals at B might initially prefer either H or V, depending on which of the two they were closer to, but if their preference changed, once again the change could occur only from H to V.

The data collected from 17 rats conformed very well to the prediction, but the sample was small and the procedure somewhat informal, to be excused on the grounds that this was a "pilot study." Therefore, a second experiment was conducted, with a neater procedure—for example, 5 daily sessions were conducted rather than just 2—and with a change in the patterns lining the two loops. In this case, the horizontally striped loop was contrasted with a plain black loop for half the rats and a plain white loop for the other half. Here the intuitive judgment about which was the more complex seemed less equivocal; clearly the horizontally striped loop was now functioning as the more complex alternative. The same prediction was made—any change in preference would be from the less to the more complex loop, i.e., from plain to horizontally striped, but not vice versa—and again was convincingly verified in the data.

The theory so far has been successful with rats, but what about

people? I have already mentioned Earl's doctoral dissertation, in which the subjects were 12-year-old children, and the stimulus materials were puzzles which the children were allowed to work on. I am also aware of an attempt by some colleagues at the University of Chicago to run (literally) children in a human-size version of the figure-8 maze, complete with horizontally and vertically striped loops. There the results were a bit messy, but they probably should have been.

Closer to home is an experiment conducted by Richard Kammann (1964) as his doctoral dissertation at the University of Cincinnati. This is the study in which the stimulus material was poetry.

I think it is clear that the physiological-need approach to motivation would be hard pressed to predict which of a set of poems would be preferred by college students. What can the Theory of Choice, as a representative of the New Look in Motivation, contribute to this problem?

A poem, like any other stimulus, has a complexity value, which is the resultant of a whole set of formal, or structural properties of the elements of the poem—for example, its meter, rhyme scheme, etc.—as well as semantic properties, relating to the meanings of individual words and phrases. Also contributing to the complexity of a poem is the ambiguity of its total message—the meanings of the individual words may be clear, but the poet's intent may not. In addition to these formal and nonformal sources of complexity is the factor of familiarity. It seems reasonable to anticipate that even the initially most complex poem would lose in complexity as it became more and more familiar to, and better and better understood by, a particular reader. "A rose is a rose is a rose" is a very unlikely sequence of words, but not to someone who has heard that line a few times. This last consideration suggests that it might be futile to attempt an assessment of the complexity of a given poem without interposing the intended reader of that poem. In short, he is the one who should, in effect, tell us how complex that poem is for him. But how might that be done?

One could try a direct approach and simply ask subjects to rate poems according to their complexity. This seemed too simple-minded, however, and while it might work, its usefulness would appear limited. A somewhat less direct approach to measuring poem complexity was sought by Dr. Kammann and found in what is referred to as the Cloze technique.

Suppose I show you the letters CINCINNAT-, with a blank space for a final letter, and I ask you to guess what single letter belongs in that space. I believe you would have little difficulty in correctly guessing the appropriate letter. It is really terribly redundant, and I am sure that a piece of mail addressed to the city of "Cincinnat" would not be delayed any longer than usual.

Suppose, however, at the opposite extreme, I randomly selected a sequence of 9 letters and asked you what the 10th should be; the

redundancy of that last letter, and hence its predictability by you, would be greatly reduced.

We could play the same game with sequences of words, deleting say every fourth word in a passage and asking people to fill in the blanks. Their ability to make correct guesses surely would reflect the internal redundancies within the word sequence. This, in essence, is the Cloze technique. It was developed, incidentally, as a measure of readability to compete with the popular Flesch count.

Dr. Kammann saw in the Cloze technique a possible way of measuring the complexity of a poem. He argued that the higher the Cloze score associated with a given poem—the greater the number of blanks properly filled in by a subject, or group of subjects—the less complex it was as a total stimulus object.

A ________, a ringing health, ________ the king
of ________ our hearts to-day! ________ what proud song
________ follow on the ________, nor do him ________?
Unless the sea ________ harp, each mirthful ________
Woven of the ________ of the nights ________ Spring,
And Dawn ________ lonely listener, glad ________ grave
With colours ________ the sea-shell ________ the wave
In ________ eye and cheek, ________ is none to ________!

Drink to him, ________ men upon an ________ peak
Brim one ________ cup of crimson ________,
And into it ________ one pure cold ________ of snow,
Then ________ it up, too ________ to speak
And ________ —to the mountains, ________ on glittering line,
________ away into the ________-glow.

FIG. 7. One of the un-clozed poems (for the Eightieth Birthday of George Meredith) used in the poetry experiment. The deleted words, in order, are: health, unto, all, But, should, thought, wrong, were, string, lightning, of, the, and, of, and, brightening, there, sing, as, Alpine, immortal, wine, drop, crust, hold, rapturously, drink, line, surging, sunset.

The rest should now be obvious. A set of 15 poems was selected, which seemed *a priori* to cover a wide range of complexity values. Every fourth word was deleted from each of the poems, beginning with the second word, and these "unclozed" poems were given to college students, whose task it was to guess what word belonged in each blank space. One such fragmented poem is shown in Figure 7.

On the basis of the subjects' performance, the poems could then be ranked according to their complexity. At a later date, these same subjects were given the intact poems and asked, among other tasks, to rate them on a good-bad scale and to indicate which they would like to have the opportunity to memorize, to hear discussed by an expert, etc.

Among the many interesting results that were obtained, the two most pertinent were that the poems of intermediate complexity were

given the highest "goodness" ratings and that the subjects' own ability to guess the missing words in all the poems—what one might call the subjects' "clozability" score—was highly correlated with the complexity of the poems they said they would prefer to memorize and hear discussed by an expert.

This brief description fails to do justice to a very complex and clever piece of work, but it does indicate a promising approach to the measurement of the concepts in the theory. In addition, it may suggest the scope of coverage that the theory is seeking. In this latter respect, the theory is quite representative of those that comprise the New Look in Motivation.

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SEMANTICS, OPERATIONALISM, AND THE MOLECULAR-STATISTICAL MODEL IN THERMODYNAMICS

By JOHN R. DIXON and ALDEN H. EMERY, JR.

BOOKSHELVES creak under their thermodynamic load: *Thermodynamics, Engineering Thermodynamics, Thermodynamics for Engineers, Chemical Thermodynamics, Equilibrium Thermodynamics, Non-equilibrium Thermodynamics, Statistical Thermodynamics, Concepts of Thermodynamics, Analytical Thermodynamics, The Nature of Thermodynamics, Thermostatics and Thermodynamics, Thermal Physics, Thermophysics, Modern Thermodynamics, Elements of Thermodynamics, Equilibrium Statistical Mechanics, Heat, Thermodynamics, and Statistical Physics*, etc., etc., etc.

Students creak under their thermodynamic load: What *is* temperature?, Heat is that which..., Work flows when....., Energy, Internal Energy, Reversible, Irreversible, Specific heat at constant....., Adiabatic, Isothermal, Gibbs Function, Helmholtz Function, Fugacity, Activity, Enthalpy, What *is* enthalpy?, Flow work, Shaft work, Saturated Vapor, Ideal Gases, Van der Waals, Polytropic, Carnot, Mean speed, Most probable speed, Root mean square speed, Entropy, Availability, Onsager, etc., etc., etc.

Teachers creak under their thermodynamic load: Why are there so many thermodynamic books? Why are there so many thermodynamic equations? Why is thermodynamics mysterious to students? Why is thermodynamics such a controversial pedagogical subject? Why are there so many different kinds of thermodynamics?

How can students and teachers make sense out of this maze and avoid thermodynamic insanity?

The first step towards modern thermodynamic peace of mind is to realize that there are four, not two, basically different approaches to the subject of thermodynamics. It isn't sufficient to speak only of the "classical" and "statistical" approaches. Two of the four approaches are macroscopic (classical) and two are microscopic (statistical). One of the macroscopic approaches is called *operationalism* and is epitomized in the work of P. W. Bridgman (1927, 1936, 1941). Unfortunately, Bridgman's books are not suitable texts. Most texts, such as Van Wylen (1959), adopt the other macroscopic approach which is best called the *axiomatic* approach. Sometimes lip service is given to operationalism in these texts but the general approach is that of stating axioms and definitions from which

all else is deduced. Taken together, the operational and the axiomatic approaches are called the classical or macroscopic approach, but it should be remembered that their starting points are considerably different. Operationalism starts with experience and induces the laws. The axiomatic approach starts with definitions and axioms and proceeds deductively.

One of the two statistical approaches can be called the *classical statistical* approach. It is epitomized by Sears (1950). The statistical approach which develops thermodynamics out of *information theory* is found in Tribus (1961). The starting points of these two microscopic or statistical approaches are also considerably different.

Once it is understood that there are four extant philosophies of thermodynamics, the second step towards sanity is to realize that people and their experiences are macroscopic. This is obvious, but fundamental and perhaps even profound.

The following appears in a recently published book (El Saden, 1965): "Matter, as we perceive it with our senses, contains an enormous number of particles. . . ." Such a statement is nonsense and is an excellent illustration of what happens when man begins to think of his own mental constructs as "real." *Man is macroscopic.* Our experience with the physical world is macroscopic. The third step towards modern thermodynamic sanity, then, is to become aware again that the microscopic atomic-molecular world is a mental construct of men. It is a model. Experience and "reality," of necessity and by definition, are macroscopic.

It is often claimed that the statistical approaches to thermodynamics "explain" the classical results. Thus, the argument goes, statistical approaches are more "basic." It is important to understand that the "explanation" provided by a molecular-statistical model is within the framework of the model itself; that is, in terms of a mental construct. When a result is obtained from analysis of a statistical-molecular model that corresponds to or predicts a macroscopic observation, it is to be remembered that the observation *justifies* the model and should increase our confidence in and appreciation of the model. But such an experience does not say anything about the *reality* of the model. The model should remain a model; it does not lose beauty or strength or usefulness by remaining a model.

The fourth step for modern thermodynamic peace, therefore, is properly to understand the relationship between macroscopic observations and microscopic predictions. The observations justify the model. The model is useful in providing a mental construct which aids in "understanding" and interpreting the observation, but the "understanding" remains within the framework of the model itself. It is no less useful there because it can be used to suggest new experiments and lead into new directions where solely macroscopic arguments are unlikely to go.

Four steps to thermodynamic peace of mind have been outlined: (1) attention to the fact that there are four, not two, different approaches to the subject; (2) recognition of the fact that man is macroscopic; (3) realization that the atomic-molecular world is a mental construct of men; and (4) understanding of the proper relationship between macroscopic observations and molecular-statistical models. With these steps taken, it is possible to dispose of the idea that either the macroscopic or the microscopic view is the more "basic," or the more important, or the more useful. There is no need to establish a scientific status scale. Both views should be taught to students, together with the truth about relationships between them.

There remain many questions about the pedagogy of thermodynamics. If the principle is accepted that learning is a growth of experience and

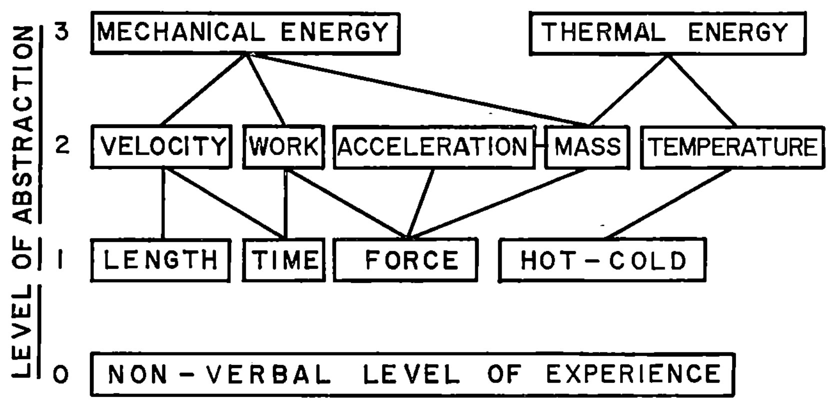

FIG. 1: Structure of concepts learned from experience.

best proceeds from the known to the unknown, then it is appropriate to examine the teaching of thermodynamics from the viewpoint of the experience of the students. In doing so, it is helpful to consider first the semantics and then the operationalism of thermodynamics. The semantic view is based on perception; the operational view on measurement.

Thermo-semantics

First look at the concepts involved in thermodynamics from the point of view of everyday experience. What are the relevant concepts that one gets before taking courses in science, from personal experience with the world? Concepts that are brought to the first day of thermodynamics class? See Figure 1.

At the bottom is raw non-verbal experience, life as it is lived. The first level of abstraction contains the concepts perceived directly; namely, length, force, time, and a crude sense of "temperature" as hot

or cold. These are perceptions made without the aid of instruments other than our own human senses. Thus, on this level, the crude temperature perceived is quite different from a reading on a thermometer. Time is not the reading of a watch, but the sensation of elapsed time, which is frequently much different from what the watch says. (Some other things are perceived directly, such as pressure, area, and volume—but these are of secondary importance in discussing thermodynamics.)

At the second level of abstraction are things not directly sensed, but which are created out of combinations of concepts at the first level. From the force it takes to lift something against gravity, an idea of mass is formed, but mass is not directly experienced. Velocity is not perceived directly, but deduced from the distance covered in a certain time. The time involved in our crude construction of velocity may be only a fraction of a second, but it is not instantaneous (try batting your eyes open and closed once quickly to determine how fast a car is going). The best that can be done in the perception of acceleration is the force with which it throws a person against the seat cushions (or dashboards). "Work" in everyday parlance is different from the scientific "work." Most persons consider that they are working hard if they are using their muscles hard (force) over a period of time. The perception of exhaustion correlates with this, rather than any distance involved.

On the third level of abstraction concepts are formed again from those below. An idea of mechanical energy is formed from the exertion it takes to get a car rolling, and perhaps also the speed that results. Likewise, a crude notion of heat is obtained from our experience with teacups and spoons, bricks and bathtubs and their relation to mass and temperature. And this is probably as high an abstraction as most people obtain from life experience with the concepts relevant to thermodynamics.

This has been a crude, qualitative, or only semi-quantitative analysis of the levels of abstraction that people build from their experience, unaided by science. It is analogous to the descriptions semanticists give for the way people abstract from their experiences. Science enters when measurements are made, and this is called operationalism.

Operational Thermodynamics

An operation is an activity. An operational definition does not describe a thing; it specifies how to measure it. Using operationalism, science has progressed far beyond sensory experience and a lot of concepts never experienced are now accepted as given and rudimentary. That is, the bases of science, though perhaps historically suggested by common human perceptions, are now broadened to include anything that can be measured that seems useful.

Furthermore, once the making of measurements is begun, a unit is needed for the number, and this means that special pieces of metal in

vaults and special meter sticks and special parts of our experience (such as pendulums, and boiling water) have a special importance in the scheme of things. They, and not direct human experience, become the primary, irreducible plane of reference. Man becomes less a taster of life and more a referee of operations and a numbers clerk.

To make sense out of all this, it is necessary to back up and take the look that P. W. Bridgman (1927) took some years ago. Operationalism, like semantics, starts with the proposition that a concept is to be understood by the operations involved in measuring it. In semantics, these operations are either perception or the process of abstraction. In science,

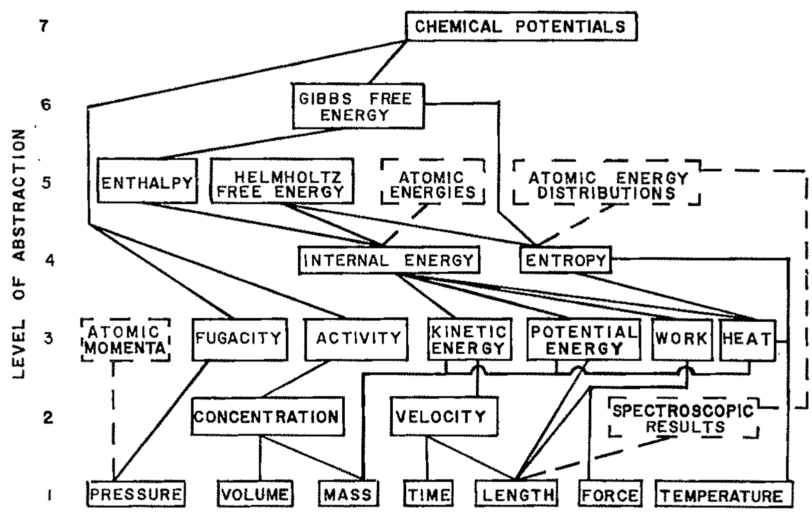

FIG. 2. Structure of operational thermodynamics.

they are either instrumental operations, such as using a pressure gauge, or a pan balance, or a Geiger counter, or a cloud chamber; or they are what Bridgman calls paper-and-pencil operations, such as calculating a velocity, or potential energy, or work, or internal energy, or the charge of an electron.

An analysis of thermodynamic concepts in this way, with the first level of abstraction occupied by things measured directly, is shown in Figure 2. This figure resembles Figure 1, because thermodynamics is an old science and started from simple human experience. But now there are some things on the first level that can be measured directly but cannot be experienced directly. Mass is an example. (Dotted lines on the figure enclose and connect concepts from other fields.)

The second level of thermodynamic abstraction contains some things that sometimes are measured directly, but more often are calculated from other things that are measured.

The third level of abstraction contains things which cannot be measured, but which have to be calculated. From this level up, thermodynamics is completely in the realm of paper-and-pencil operations. Since every concept is to be understood in terms of the operations which produce it, each step up becomes more abstract and, naturally, understanding of the higher levels becomes difficult. At the top of the abstraction ladder are the free energies and the chemical potential. (Actually, fugacity and activity are defined from chemical potential. However, it is finally possible to relate fugacity to pressure, and activity to concentration, both by means of an equation of state. Fugacity is then understandable as a modified pressure and activity as a modified concentration. Such understanding takes them away from the abstract.)

Now, how does the atomic-molecular model fit into this picture? Boltzmann statistics and information theory make the connection with two key equations of the form:

$$E = \sum_i n_i \epsilon_i$$

$$S = k \ln W$$

or

$$S = -k \sum_i p_i \ln p_i$$

where

E = energy
n_i = number of particles in i^{th} state
ϵ_i = energy of i^{th} state
S = entropy
k = Boltzmann's constant
W = thermodynamic probability of a macrostate
p_i = probability of particle occupying i^{th} state

The symbols E and S represent concepts in operational thermodynamics and the others represent concepts in the molecular or information model. If it is to be said that the molecular picture is more basic, and that one can derive the thermodynamic concepts from the molecular, then this would mean that the molecular concepts are lower on the ladder of abstraction. However, it is impossible to derive the molecular concepts from anything else and it is obvious that macroscopic man cannot experience molecular effects directly. Thus, there is a dead end on the ladder pointing down towards experience or direct measurement, which is not allowed. The situation really has to be the other way around; the thermodynamic concepts are used to define the molecular. The molecular concepts are more abstract, and are above the thermodynamic on the ladder of abstraction. From the standpoint of operationalism, then, the atomic-molecular view has to be explained by thermodynamics, rather than the other way around, as is currently popular.

Of course, the atomic-molecular model is useful in other fields, too, and

one reason for introducing the connection into thermodynamics is to show the unification of science. But even in these other fields, the atomic hypothesis is a high abstraction; it is to be understood by things lower than it on the ladder of abstraction, such as data on gaseous effusion experiments, or spectroscopic lines, or diffusion data.

Pedagogical Thermodynamics

How do the semantic and operational analyses of thermodynamic concepts bear on the problems associated with teaching thermodynamics? As noted above, this teaching is generally approached axiomatically or statistically. There are two major points to be made here in this matter. The first has to do with what the students already know when they come into a college-level thermodynamics course. They already know the so-called Zeroth, First, and Second Laws of Thermodynamics on the level of experience. They do not know, however, that they know. They cannot verbalize or symbolize the laws in a way that makes thermodynamics teachers happy and they haven't made the deductions and generalizations from the laws that lead to concepts like entropy. But they know the meaning of the laws because they will use them correctly in problems and they will not violate them when solving problems.

When asked, for example, what happens to two blocks of copper initially at different temperatures left alone together in an insulated container, they will all reply that the blocks will come to the same temperature. Of course, if asked how they know, they usually say "Because it is a law of nature." This is a lovely opportunity to point out that the opposite is true; that is, that it is a law of nature because it happens. It is clear, however, that they know the Zeroth Law and have a working, if not verbal, concept of equality of temperatures.

When asked to compute the final temperature of the blocks, or the velocity of a block after falling freely a distance h, or the temperature rise of a gas when stirred by a given rate of paddle work for a given time, they will correctly use conservation of energy. This time when asked how they know, a few brave student souls may venture forth with something like "Because it's consistent with our experience in these situations." Progress.

Finally, when given a simple water-water heat exchanger, they will not assume that the temperatures of two streams cross. They will laugh if told that a hotter copper block got hotter while a colder one got colder, all consistent with energy conservation. If told by the teacher that he once saw a block slide up a plane and get cold—consistent with energy conservation—they will walk out of the class. They know the Second Law, too, in the sense that they know that processes only occur one-way in nature. This is a good time to point out that the First Law, a con-

servation law, should be expected to lead to a property which is conserved (energy) but that the Second Law, a one-way law, should be expected naturally to lead to a property which only goes one way (entropy). After all, that would then be consistent with their experience, wouldn't it?

The first point to be made about teaching classical thermodynamics, then, is that students come to the course with a working understanding of the laws, but lack a verbal or symbolic structure of knowledge about them. The rather elaborate symbolic structure should therefore be carefully tied to their experience and they should be repeatedly reminded of the fact that their own experience, plus some thought led by the teachers, leads to all the consequences being discovered. It is neither mysterious nor a matter of memorizing a myriad of definitions or axioms.

The second point to be made about teaching classical thermodynamics has to do with the degree of abstraction involved. Starting with a strictly operational approach is a great aid here. The reason operationalism helps here is simply that it makes clear the degree of abstraction involved. At the outset the need for defining certain quantities in terms of operations (activities of the process of measurement) is made apparent. One cannot define length, mass, force, or time in words.

It should be noted that concepts on each level of the abstraction ladder must be defined in terms of less abstract terms and that proceeding up the abstraction ladder moves one farther and farther from actual experience. Generalizing, or developing and dealing with, abstract quantities is an extremely difficult mental activity. Other courses in engineering (the electrical engineering concept of a *field* is a possible exception) do not require anywhere near the degree of abstraction that is required in thermodynamics. By pointing out to students the extreme height of the thermodynamic abstraction ladder, the reason for the mystery surrounding a concept such as entropy is apparent to them. At least they are no longer mystified about why they cannot understand what entropy *is*. This also sets the stage for an "explanation" in terms of a molecular-statistical mental construct or model.

Summary

There are really four, not two, kinds of approaches to the subject of thermodynamics. Operationalism and the axiomatic approach are macroscopic approaches; statistical mechanics and information theory are microscopic approaches. Examination of the semantic and operational structure of thermodynamics and the realization that man and his experience are macroscopic show that the atomic-statistical model must be more abstract than certain macroscopic concepts. If introduction of the subject is to use and extend the experience of the students, then it should begin with operationalism, the only place where an understanding

of the concepts in terms of experience can be had. Teaching can then proceed to either the axiomatic approach, for the beautiful (if dull) clear and logical deductive development, or to one of the molecular approaches for an interpretation of thermodynamics in terms of the molecular model now in common use for such purposes.

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AMERICAN SCIENTIST, *53*, 1965

THE FINE STRUCTURE OF THE BACTERIAL CELL AND THE POSSIBILITY OF ITS ARTIFICIAL SYNTHESIS*

By ERNEST C. POLLARD

IN THE past decade a scientific revolution has taken place, which is perhaps the greatest scientific revolution of all. I refer to the achievements of a strangely assorted group of geneticists, biochemists, virologists, physicists, classical biologists, and even engineers, who have founded the strangely named, but powerful, subject of molecular biology. These individuals are not a homogeneous group; they are bound by little more than an intense intellectual fervor; they can disagree sharply with one another, and they accord respect to anyone with the greatest personal reluctance. These are all symptoms of men of science who have suddenly developed sharp insight and who are building a doctrine which will be used to interpret living things for the future, and probably the long future.

To set this revolution in perspective, let us look at a comparable revolution in physical science, the revolution which took place at the turn of the 20th century, when in a few years we witnessed the discovery of the electron, of X rays, of radioactivity and the atomic nucleus, of the interpretation of spectra, and of the nature of solid matter. In about 40 years from the first of these discoveries the claim could fairly be made that a complete understanding of normal inanimate matter had been attained by science. In this claim cosmology is excluded, and also the high-energy features of the atomic nucleus. What is meant is essentially this: that for anything which can be touched or perceived in the ordinary room, one can find, somewhere in the world, an expert, who can tell you the precise structure of that object, how to alter it to your will, or that it cannot be so altered, if it is inherently impossible to do so. Some latitude must be allowed in this claim, bearing in mind that expense and time could be involved, and we are cheerfully assuming that they represent no problem.

Now the suggestion I am making about the revolution in biology is that in 30 more years, or, again, 40 years from the start, it will be possible to name any part of an organism, to find someone somewhere in the world who can tell you its precise structure, how to alter it to your will, or that it is inherently impossible to do so. This revolution, unlike the revolution which concerned inanimate things, not only affects our en-

* A Sigma Xi-RESA National Lecture, 1964–1965 Series.

Support for the background research of this article came, to a considerable extent, from N.A.S.A.

vironment, making it easier for us to live in it, but it concerns ourselves as living beings. If we widen our imagination a little, we can see that we shall not only have a profound ability to control disease, far beyond our present ability, but we shall also be able to control factors which now are outside our powers. To show the kind of potential ability we might have, we should by then understand memory, and indeed intelligence, and we should, therefore, be able to cure our memory and intelligence defects, by going to the appropriate biological engineer, and getting the proper operation done on us. Clearly, the social revolution which could result is not to be belittled, and it is indeed latent in the growing power of interpretation given us by the molecular biologists.

If this claim is made honestly, then it should be followed logically to the conclusion that if we understand a cell we should be able to make one; hence the title of this lecture and the examination of what the task would entail, and what might be the discoveries made on the way, if any. So the goal of the lecture is to try to present the character of the structure and working of the bacterial cell in a way that will provoke thought about the problem of its artificial synthesis.

Three further things need to be said before undertaking this task. The first concerns the choice of the bacterial cell. Why not a virus? Why not a human cell?

A virus has been, to my mind, already synthesized. There are at least four laboratories in the United States alone where nucleic acids are being made from cell-free systems. One of these nucleic acids has presumably had the necessary length and complexity to provide the code for two enzymes and so one can say that a virus has been made. The problem is finding the necessary host, and this highlights the basic objection to choosing a virus to represent the achievement of construction of a whole cell: it is not, alone, fully representative of life. So, at least as far as I am concerned, I cannot feel that the true challenge has been met when we have made a virus. Now if we choose to set up the task of making a human cell, we are involved with something which is not normally found out of contact with 10^{12} other cells in some living being, with a cell which bears the characteristics of that sheltered existence, unusual size, and the presence of subunits which are part of its functioning—organelles such as mitochondria, for example. At the moment, it seems to be unnecessarily hard, though biological discovery may well show this conclusion to be wrong one day. However, for the present let us accept this view. The bacterial cell, which is quite capable of autonomous existence, under conditions which are most adverse, is certainly living* and certainly a challenge sufficiently great to us at present.

* Studies made by individuals interested in means for testing for life on Mars [1] indicate that nowhere on the surface of the earth can one select a gram of soil without also selecting 10,000 (minimum) living organisms.

The second preliminary thought concerns what we mean by the artificial synthesis of a cell. By this I mean that we could start from the elements of a cell; carbon, nitrogen, hydrogen, oxygen, phosphorus, and sulfur plus the trace factors, and assure ourselves that there was no mystery about completing each necessary step all the way from the elements to the newly living cell made from those elements, without necessarily actually completing more than one step at a time. If some necessity arose to marshall money and scientists to do the whole task, as was found to be the case for controlled nuclear reactions and radar, then it could be done. *Knowing how to do it* is what we are discussing.

The third preliminary thought is of great importance, especially to biologists. To make the point, I shall return to modern physics. The discoveries concerning the atom and the atomic nucleus, particularly the latter, a school of thought in which I began my scientific work, could never have been made with our eyes directly: they had to be made by hypothesis and the test of experiment or, in better terms, by *accurate imagination*. Thus, the fact that we can never see an atom of sodium doesn't prevent us from knowing the orbitals which characterize its structure, and if we want to construct models which represent our ideas of these orbitals we can do so; and such models are often very useful to students who are interested in the way in which atoms behave, particularly in relation to one another. Molecular biology has already shown that many of the essential operations of the living cell take place at small molecular dimensions, and so we cannot hope to see both the molecule and how it is working at the same time. Thus, we are once again forced to the position of the physical scientist, and must use accurate imagination to describe the cell, if we want to use enough detail to see the mechanisms at work.

This leads to the final introductory remark. In what follows we shall have to draw thought pictures, already known to the physicist and engineer as "schematic diagrams." They are to be interpreted as figures to aid thought, with those features which need to be considered placed in stress. They are not supposed to be accurate pictures of the particular feature of the cell involved.

It is planned to present some electron-micrographs to reassure the reader that actual pictures do exist, though even these can be understood only if the preparative procedures are known, and so they, too, while very impressive are also thought pictures in reality. Then, a development of schematic pictures of the operation of the cell will be made. Finally, the possibility of making a cell will be considered.

The Bacterial Cell: DNA and RNA and Their Location

In Figure 1, a very beautiful electron micrograph of the bacterial cell, *E. coli*, is shown. It was taken by Dr. Lucien Caro, now of the Oak Ridge

National Laboratory, who started his graduate work in my group at Yale. It is a thin section, and it shows the outline of the cell. The black dots which fill the majority of the cell are probably ribosomes, and the relatively clear part, which shows some evidence of fibrils and some structure in the fibrils, we can call the nucleus. Before we discuss the features of interest in this cell, an inventory of its contents, given in Table 1, is useful to have.

TABLE 1

OUTLINE OF NATURE AND CONTENTS OF A BACTERIAL CELL

Length, 3 microns, Diameter, 1 micron	
Volume: 2.2×10^{-12} cc	
Water 70%; and of what remains:	
DNA 3%	Protein 70%:
RNA 12%	Ribosomes (10,000)
	Enzymes
	Surface structural protein
Lipid 6%	
Phospholipid 4%	
Polysaccharides 5%	
DNA in one, two, or three continuous units	
Molecular weight 2×10^9	
RNA in three forms	
Ribosomes: RNA—protein particles, 3×10^6 MW	

To comment on this table, we can point out that the cell, at a diameter of one ten-thousandth of a centimeter is already very close to the wave length of visible light, so that as we look at it in the microscope we already, even for its outline, must consider that it is scattering and diffracting light and judge if our conclusions from what we see are correct. Any internal structure we "see" must definitely be considered as scattering centers and judged accordingly.

The cells appear to be very wet, with 70% water. But once again, remember that the hard-boiled egg at breakfast, with its firm denatured albumin, has 50% water, and isn't very "sloshy." So with that 70%, the bacterial cell hasn't too much excess water to act simply as a solvent. It isn't very "sloshy" either.

If we proceed beyond the water, then the major component is protein, distributed as indicated. The ribosomes are particles which involve RNA as well as protein and are vital to the process of protein synthesis as we shall see later. The enzymes are biological catalysts, which get all the chemistry of the cell to go, and the membrane of the cell has some remarkable protein in it, possibly only for structural purposes, possibly not.

Of the remaining components we can really concern ourselves only with the DNA and RNA. DNA, deoxyribonucleic acid, is the hereditary material of the cell, and RNA, ribonucleic acid, is the material which insures that the orders of the DNA get executed. Of great interest is the

molecular weight of the DNA: it is two billion, and this figure is a true molecular weight in the sense of Avogadro and Cannizaro; if one atom is removed from it, there is a change in observable properties. I do not know of any other material in Nature with so large a molecular weight, under these terms, and so we see one extreme showing up in living systems: this huge molecular weight for one component of the cell. RNA is large, but not so majestically large as DNA.

Now if we want to visualize DNA we can look at Figure 2 which is a remarkable picture of the DNA in a bacterial virus. It was taken by

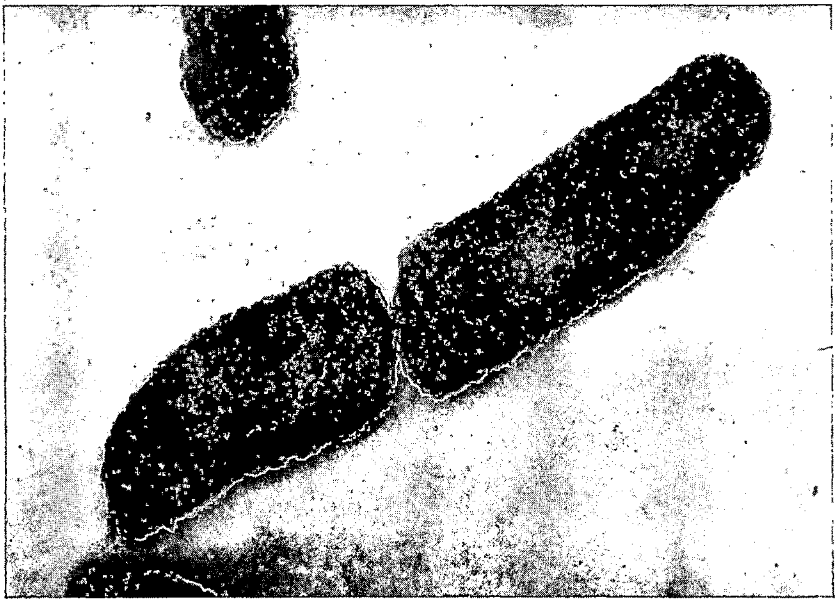


FIG. 1. An electron micrograph of a section of a bacterial cell of *E. coli*. This very beautiful picture was made by Dr. Lucien Caro of Oak Ridge National Laboratory.

Kleinschmidt, *et al.*, [2] by the very nice technique of osmotically shocking the virus on the supporting film used for electron microscopy. The DNA spills out, as can be seen, and is obviously a very long, continuous structure. Evidence is accumulating that this DNA is also in a continuous loop, so even the two ends which can be seen are perhaps two too many. Now this piece of DNA is only about one-twentieth of the actual length of the DNA in a bacterial cell. To get some idea of this structure, Figure 3, which is taken from work published by Dr. Cairns of the Cold Spring Harbor Laboratory [3] gives a fine impression. In this picture the scale is totally different and to visualize the bacterium it is necessary to compare it with one of the dots in the emulsion. The line at the bottom is about fifty times the length of the bacterium. The technique used to get this

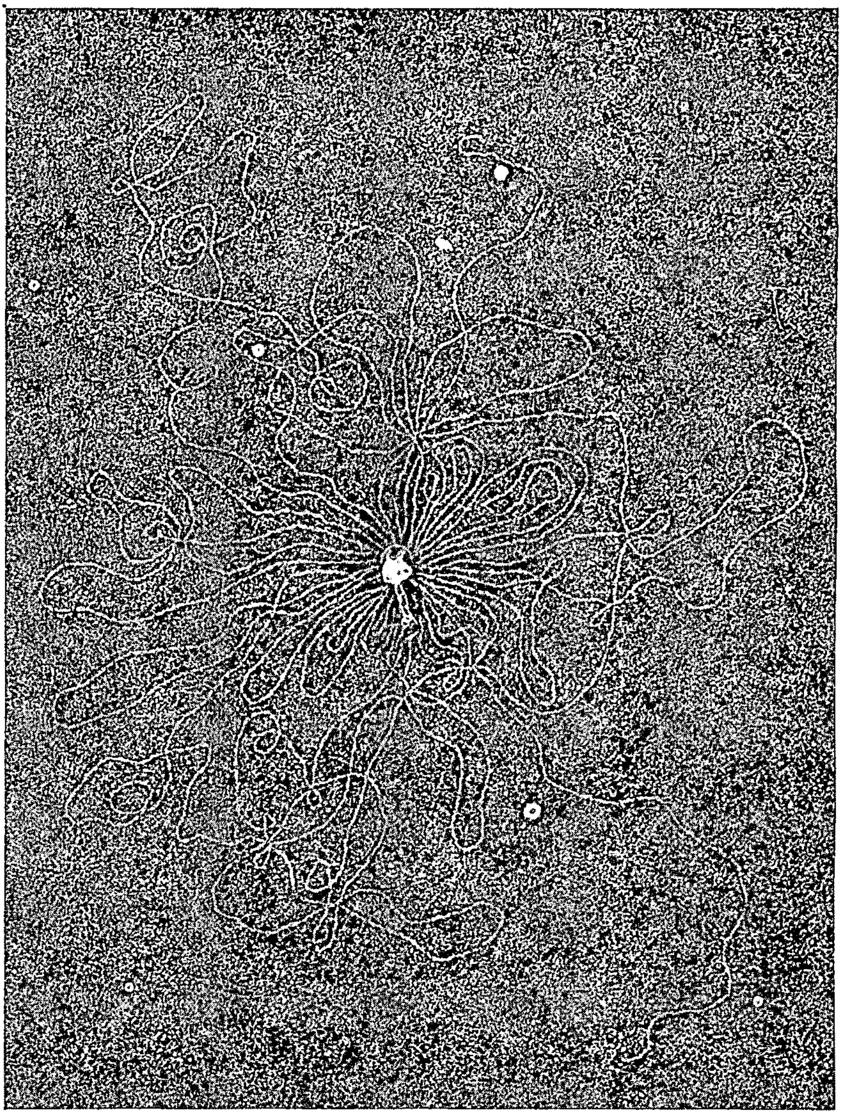


FIG. 2. DNA from a bacterial virus. It can be seen to be a single unit of truly great length. This very spectacular picture is taken from the work of Kleinschmidt, Lang, Jacherts and Lahr. Reference [2].

picture is very beautiful and is as follows: Advantage is taken of the base *thymine,* which is found only in DNA. It can be readily procured with a tritium label and the tritium, when it undergoes radioactive decay, emits a beta ray of very low energy which will cause the development of only one or two photographic emulsion grains. Thus, the presence of developed grains means the presence of DNA. What Dr. Cairns does is to

label the bacterium very fully with this kind of thymine and then with great care and skill, and he is *par excellence* the one who can do this, extract any DNA, place it in contact with a photographic emulsion, and let the image develop as the tritium decays. The result is what is in the figure, and it shows dramatically how the long continuous piece of DNA has been released from the structural form it must have had in the cell. Clearly, one of our problems is to "put" this DNA inside the cell: it is not simple.

In order to gain some idea of where to put the DNA we can consider some work done by Dr. Caro and Dr. Van Tubergen, working for Dr.

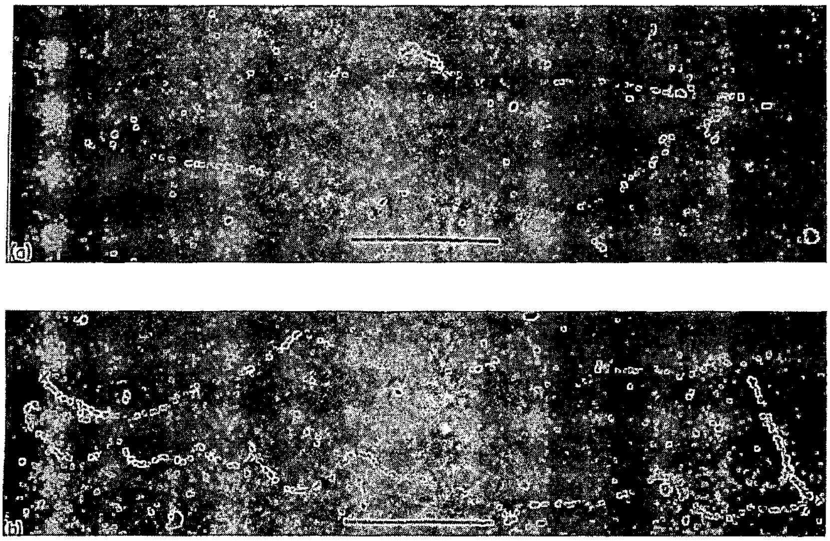


FIG. 3. An autoradiograph of the DNA taken from *E. coli* cells. This DNA has been labeled with H^3-thymine and extracted with consummate care by Dr. J. Cairns of the Cold Spring Harbor Laboratory. This appeared in "The Interpretation of Ultrastructure" Symposium of the Intetnational Society for Cell Biology, published by Academic Press, 1962.

Forro at Yale. This work was actually part of a Ph.D. thesis. Again what was done was to label the bacterium with tritium thymine, and in this instance to take random, but consecutive slices of the bacteria which had been imbedded in methyl methacrylate as if for electron microscopy. The slices were then studied by the same technique of radioautography that we have described, and the question asked as to whether all slices had about an equally probable chance of developing grains of emulsion, or whether some had no chance at all, because there was no DNA in that part of the cell and so no possibility of any radioactive decay due to thymine. What was found was that a considerable number of zero grains were present, and by analyzing the data the conclusion was reached that the DNA is confined to a small part of the bacterium which could easily be associated with the part in the middle of the electron micrograph of

Figure 1; and indeed, if one looks closely at this quite remarkable picture one can convince oneself that there are fibrils in the relatively empty part, and even that some sort of organization exists there. Thus, we must put the DNA in the nucleus of the cell.

The next question which naturally arises is the question of the location of the RNA. In Figure 4 we show a remarkable combination of electron microscopy and radioautography, again due to Dr. Caro. In this figure

FIG. 4. A section electronmicrograph of *E. coli* labeled with H^3-uracil and concomitantly radioautographed. The silver "worms" which are the developed grains caused by tritium decay are all outside the nuclear region. This superb example of technical skill is also due to Dr. Lucien Caro.

the label has been given as tritiated uracil, which is not found as such in DNA but is mostly in one or another form of RNA. In addition to this change, a very valuable technique of development of the emulsion has been used. Instead of waiting for the whole grain of silver bromide to develop, the development is halted quite early and fixation introduced. The result is that the inception of the latent image can be seen instead of the fully blackened whole grain, and a far better resolution is so obtained. It can be seen that the origin of the radioautographic images all lie beyond the central part we associate with the nucleus. Therefore, it seems reasonable to assign the RNA to a part of the cell which is outside the nucleus.

Inferences Regarding the Cell Structure
Preliminaries Regarding Ordered Synthesis

With these figures we really exhaust that part of the description of the cell which can be made reassuring with electron micrography and we have to embark on the process of imagining, in a guided way, the kind of structure which must be responsible for the various processes in the cell. To do this we need to have an idea of the most fundamental processes which are at work, and which must be considered to have their proper place in the cell. It is the character of these fundamental processes which has been elucidated by modern biology, and so we have to give a short account of this subject in order to be able to suggest ways in which the parts of the cell are located and how they work together.

We begin with a compact diagram of four kinds of nucleic acid, shown in Figure 5. On the left is the all important DNA, which is shown schematically and not realistically. It is in reality in a double helical pattern and so must twist and untwist for some of its functions. For our present purposes the helical structure is not important, and we have simplified the representation. The essential features are the double structure, both chains are polymers of Adenine (A), Guanine (G), Cytosine (C), and Thymine (T), which are linked through a deoxyribose sugar and phosphate and the requirement that, in the chain opposite, every thymine finds an adenine and every guanine a cytosine. In this way a preservation of the instructions is made, for while the two chains are quite different, the existence of only one also guarantees that the other can be made absolutely exactly. DNA also has the remarkable property of being fixed once made. Unlike the other components of the bacterial cell, DNA does not "turn over," but once it is completed remains as such, unless rather drastic external circumstances intervene. This permanence of the DNA is also necessary to make it effective as a hereditary codescript, to repeat the remarkably predictive words of Schrödinger [4]. We need to remember that one million or more bases in a continuous line form the actual molecule of DNA. Again we remind the reader that Schrödinger predicted that this codescript would have to be an "aperiodic crystal." His words do really describe almost exactly the strange kind of structure we see in these giant molecules.

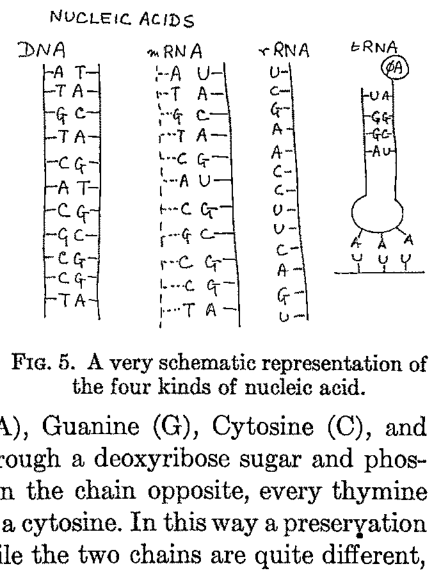


FIG. 5. A very schematic representation of the four kinds of nucleic acid.

The next kind of nucleic acid is messenger RNA, probably the most

exciting and strange material in nature. Intense study of this material will certainly be repaid by great scientific excitement, and as advice to a young scientist reading this article, I suggest that he or she settle down to a long occupation with it and its behavior. It is single stranded and it is made as a complementary copy of one strand of DNA, which has been indicated in the figure as a dotted strand. This means that the RNA is almost identical with one strand of DNA, and would be identical except for the presence of the base uracil, instead of thymine, and the additional oxygen in the sugar part of the polymer. This messenger RNA has a molecular weight of about one million, making it far smaller than the DNA, though by no means small, and it, together with a considerable apparatus, which we shall shortly describe, is the means by which the cell guarantees that the instructions on the DNA are used to make protein of the precise kind needed. In bacteria, the messenger RNA is not permanent. It has a half-life which depends on the condition of the cell (temperature and oxygen supply, for example) and which is about one minute at optimum temperature. This means that the amount of protein which can result from one molecule of messenger RNA is limited.

Since we have much more to say about this RNA later we can consider the other two forms. The ribosomal RNA which is shown next, reading from the left, is, in some ways, not yet understood. It is permanent, in the sense that it does not decay rapidly, and its known function is to form part of the ribonucleoprotein *ribosomes*. It is known that it is also made so as to be complementary to some stretch of the DNA, but it is not wholly clear to us as to the nature of this stretch. It is relatively bland in its function and because it is harder to study we may have one or two years to wait before we know much more about it.

The last form of RNA is transfer RNA, the little RNA, sometimes called "soluble" RNA. This is exciting at the moment because, just as I started to give these lectures, the remarkable work of Dr. Holley's group at Cornell became known [5]. The features of tRNA which are important to us are first, the provision for attaching a particular amino acid at one end of the RNA, and second, the presence of a triplet "code" of three bases, somewhere about the middle of the molecule, which are capable of establishing a firm relationship with three complementary bases on messenger RNA, and so of bringing the appropriate amino acid into place for incorporation into the chain of a protein. In the schematic diagram, the amino acid selected is phenylalanine, and the code for this is three adenines, which match up with three uracils on the messenger RNA. The tRNA has a secondary structure, which is thought to have a great deal of complementary base pairing within itself, as indicated. Holley's group have shown the entire sequence of 77 bases, and their work opens up a vast future in the determination of nucleic acid sequences. In their case, the tRNA carried the code for alanine. They show several possible final

structures for it, so that even when the whole sequence is known, the whole story is not in.

We need to remind the reader that there are 19 amino acids, for which the structures are nicely set out in the late Dr. Quastler's very perceptive book, *Information Theory in Biology* [6]. Joining these so that the amino groups and carboxyl groups coalesce with the exclusion of a water molecule, causes the formation of the peptide bond, and by doing this to the extent of perhaps 100 to 300 such amino acids we obtain the raw material of a protein. For it to be of any biological significance that we know at present, it must have a definite sequence, and this is assured by the "holy trinity" of DNA to RNA to protein.

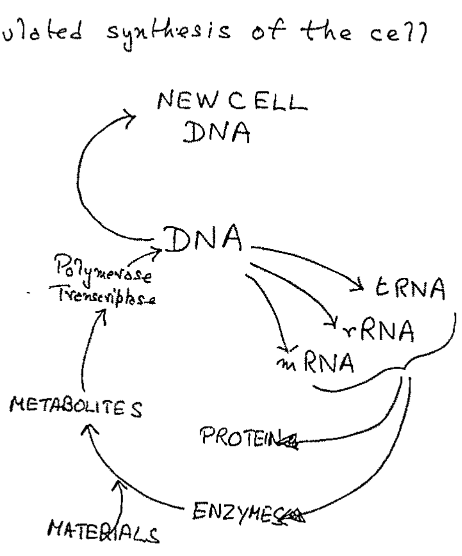


FIG. 6. A schematic diagram of the regulated synthesis of a living cell.

Ordered Synthesis in the Cell

We now come to the description of one of the absolutely fundamental processes in the cell, which must be described in such a way as to be imagined actually in the cell. To think about it, Figure 6 will help. It can be seen that DNA is at the heart of the process, and that it has two starting functions, one to make more of itself, indicated by the arrow which sweeps upward, and the other to make the general happenings of the cell possible. To do this, it must be closely related to the "manufacture" of messenger RNA, transfer RNA, and ribosomal RNA as shown. All three of these then conspire to form the protein, which is either structural, or enzymatic. The enzymatic part handles the supply of materials which

come in from the outside, and converts them into metabolites and, as a result, the two major "factories" of the DNA polymerase and the "transcriptase" are able to function. The cell clearly does much more than just what is shown in this figure, but we shall have quite a problem assimilating these into the confines of the tiny volume of the cell as it is.

We can start the process of seeing how these actually must be by looking at the suggestion made by Dr. Cairns as to the way in which DNA replicates (Fig. 7). Since DNA is of helical form, the management of it in any kind of way along its length is bound to require some spinning pro-

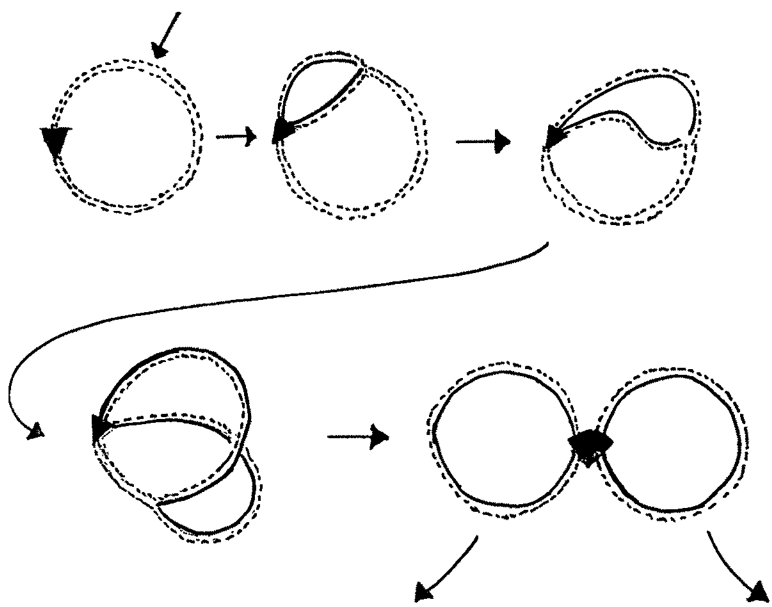
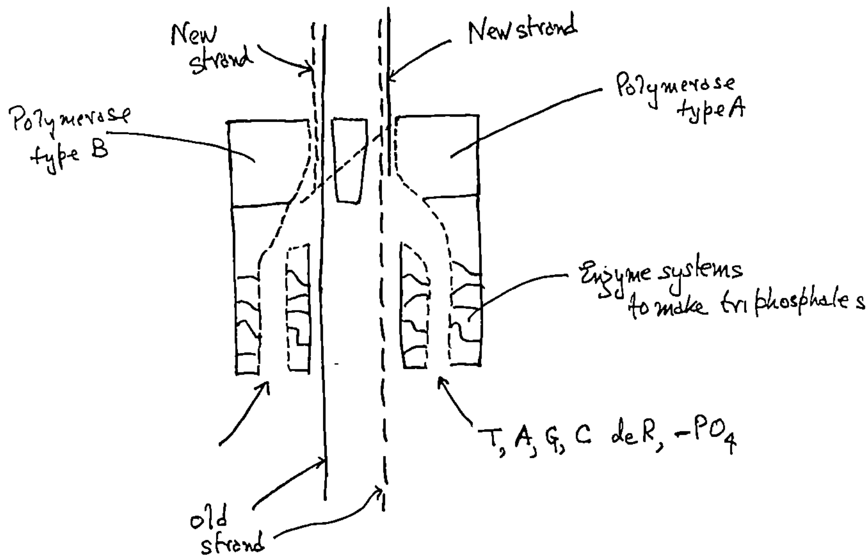


FIG. 7. Dr. Cairns' suggestion as to the mechanism of DNA synthesis.

cess. Dr. Cairns suggests that there is a "hypothetical" spinning apparatus which is at one end of the DNA, and we can start at that point. It is shown as the black triangle. The replication of the DNA, which has been shown to progress uniformly in a linear fashion by Yoshikawa and Sueoka [7] and Lark, *et al.*, [8] is in the form of a kind of "Y," and is shown by representing the new DNA as black and the old DNA as dotted. The synthesis progresses around the continuous DNA strand and, at the end, the spinning mechanism is supposed to replicate also; the two then separate into the DNA needed for the two daughter cells. This DNA is both old and new as can be seen.

To move on quickly to one of the "factories" of the cell, in Figure 8 is shown a schematic representation of what I believe must be happening.

The shape need not be in this form, for example, a shape more like a "yo-yo" might be even better or it could be much more spread out, but nevertheless this kind of thing is indicated. To comment on it: first, the DNA must separate some time, so it is indicated as separating just before it enters the factory. The actual point of synthesis is double and takes place where the new strands appear. To achieve it I have suggested that two polymerases, each of different specificities must be present to handle



The polymerase factory to make new DNA.

FIG. 8. A schematic diagram of the polymerase "factory," sometimes called the "growing point" for the DNA.

the difficult problem of synthesizing DNA which is presented differently in the two strands. The component parts which are immediately needed by the synthetic process are made *in situ* from the four bases, phosphate and sugar. This requires quite an assembly of enzymes which can make the four components and keep them at high concentration in the little region near the synthetic points.

In reality, the excitement of this kind of figure lies in the rate at which the DNA is moving through the factory. If one takes a pencil and sweeps it up the figure as fast as it can be moved, this is still about ten times too slow. It also has to be realized that the speed is definitely matched by the great accuracy, which is such that only one mistake is made in every 10^8 times when the conditions are at their best.

To justify the need for a "factory," the following piece of reasoning is offered. On Figure 9, the component parts which are assembled into nucleic acid are shown. The TTP, which is thymidine triphosphate, illustrates one of the components of DNA and it is assembled into the chain of DNA by breaking off the last two phosphates and joining the remaining one to the sugar of the next triphosphate to form the sequence "base—sugar—phosphate, base—sugar—phosphate . . ." which extends

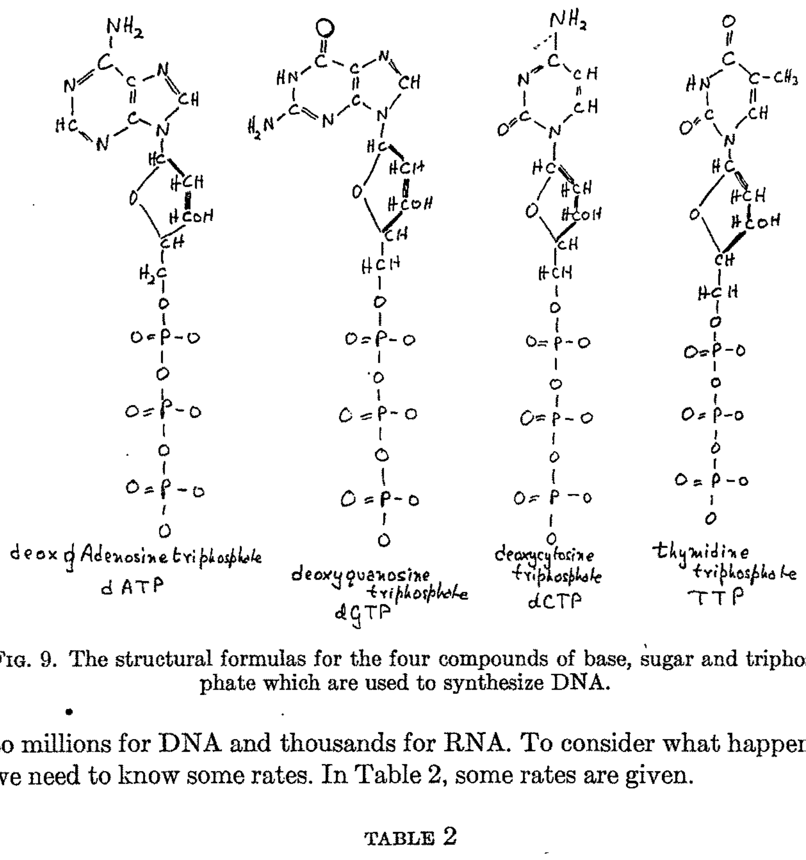


FIG. 9. The structural formulas for the four compounds of base, sugar and triphosphate which are used to synthesize DNA.

to millions for DNA and thousands for RNA. To consider what happens we need to know some rates. In Table 2, some rates are given.

TABLE 2

SOME RATES OF SYNTHESIS BY THE BACTERIAL CELL

DNA synthesis	3000 base pairs per second
mRNA "	As high as 100,000 base pairs per second
Protein synthesis	5–50 amino acids per second per polysome
Enzyme turnover	Often 1000 per second. It is very variable

Some comment on these is in order before they are used in discussion. The rate for DNA synthesis is that which is found both for bacteria when growing normally and optimally and for bacterial viruses. The rate for messenger RNA is hard to estimate because it is not certain whether all

the DNA is synthesizing messenger at once. If it is not, then the figure given is justified.

One very simple assumption can be made regarding the process of assembly of the DNA and this is that the triphosphate must simply collide with the right place. It need not do so with the right aspect, for it is likely that rotational brownian movement will correct the faulty collision very fast, nor is it necessary for the enzymatic process to take any time after the collision. Both these factors might take some time so the assumption made is truly an extreme representing the fastest that can possibly be done by normal chemical methods.

Now the rate of collision can be estimated in the same way as done by Smoluchowski and as described by the writer [9] and the very simple expression, the sole formula in this article is found.

$$\phi = 2\,\pi\,DRC$$

where D is the rate of collision at a target spot of total radius R (this is the sum of the radius of the impinging molecule and the spot to be hit) and C is the concentration a long way away from the point of collision. Now if we substitute the known value of ϕ, we have only to know D and R to be able to estimate the concentration of the phosphates which must be present. The value of D was measured by Dr. Lehman at Penn State, for representative substances [10] and the value of R can be estimated with fair accuracy as it involves the radius of the triphosphates. So the concentration can be found. Attempts in our laboratory to measure the actual concentration of the triphosphates have not been too successful because they are obviously very small. Deoxyadenosine triphosphate can hardly be present in more than one-twentieth of the amount needed and the general impression is that the amount is still less. On the other hand, thymine, as the free base, is present to about the right amount. This small concentration of triphosphate leads one to wonder what is at work. Is chemistry to be abandoned or is there some way out? One way is that taken in Figure 8, where it is suggested that the machinery to make the triphosphates is very close to the polymerases, so that the concentration of the triphosphates throughout the whole cell is not that which is needed to drive the reaction at the rate observed. More work needs to be done on these actual concentrations before the argument above can be taken too literally. However, the evidence is suggestive that there is some organization at work in assembling the DNA.

If we now move on to the process of protein synthesis, this can be considered by looking at Figure 10. This is a strange, straggly-looking figure, but it still can be used to give a lot of information. The long line with the short whiskers is the messenger RNA and the whiskers represent the bases which form the method of coding for the right protein. The first action is the attachment of a ribosome to the messenger RNA and this

promptly makes possible the attachment of a transfer RNA molecule with an amino acid on it at the end. The ribosome then moves on one "notch" and another transfer RNA comes into place with another amino acid held at the end, with its amino group and carboxyl group both held ready for the union and formation of the peptide bond. This may well be achieved by means of a peptide bond enzyme, if the evidence from Dr. Schweet's laboratory [11] is right; and as we believe it, we have shown a dotted line outline of this enzyme behind the carboxyl and amino groups,

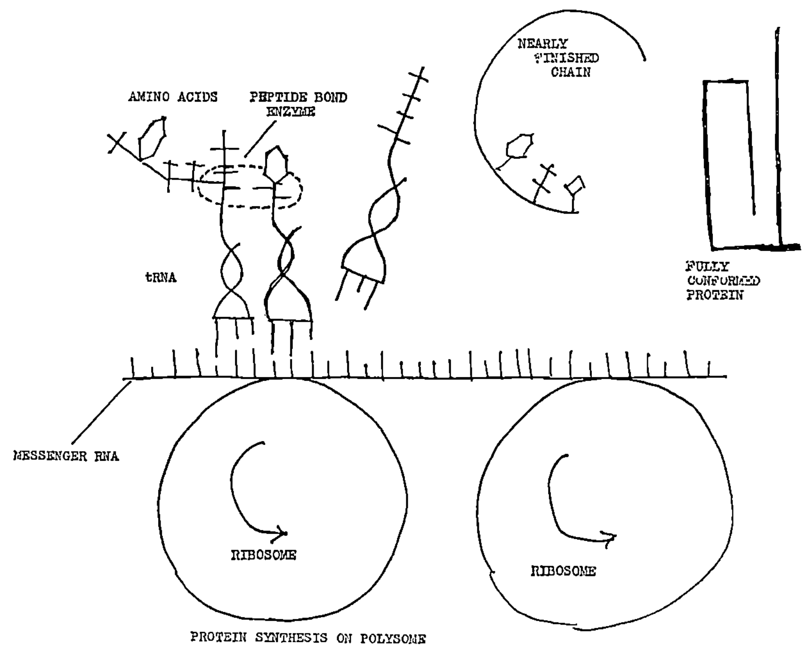


FIG. 10. A very schematic representation of the ideas involved in protein synthesis.

ready to function. Another tRNA molecule is shown as being on the way. While all this has been happening, another ribosome, in fact about five more, have been working their way down the messenger RNA and each one has been manufacturing protein. One which has nearly completed its chain is shown on the right, and the final action, after the whole chain has been finished, is to form itself into its final conformation, represented poetically on the far right. This is the form necessary for the enzymatic function to be effective.

Once again we have to think about rates. If the slowest probable value is taking place, then the ribosome is rolling along so that the clicks go at about the rate of a good typist typing each letter. It should be remembered that there are two typewriters going together at once: the ribosome

and the peptide bond enzyme, to say nothing of the five other sets of operators further down the messenger RNA. To keep the analogy, the output is at least as good, *per polysome,* as a good, efficient, and large office. If the rate is as high as it might be, then we have to think of the rolling of the ribosome and the operation of the peptide bond enzyme as like a low musical note; perhaps like the hum of the A.C. As this hum progresses, think of the protein as sweeping down the messenger RNA in tune with it. I find it most exciting, for it is not a single process at all, but involves at least two factors operating together.

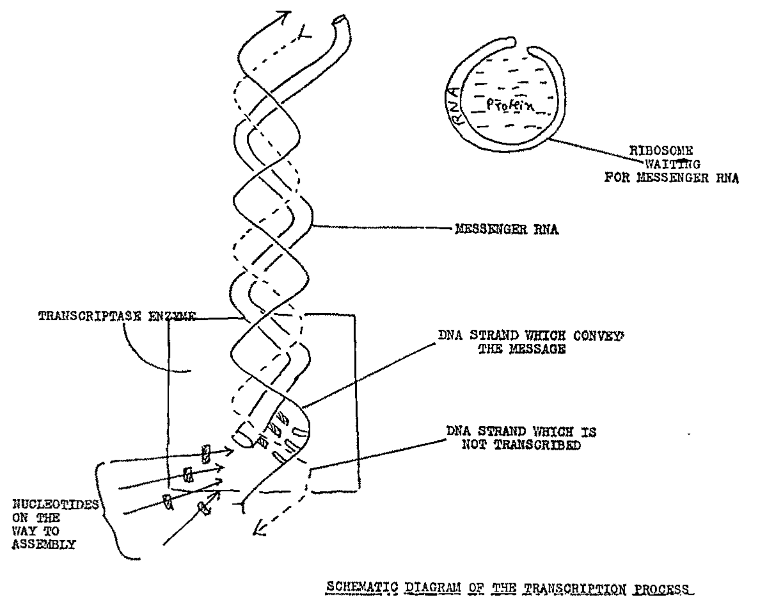


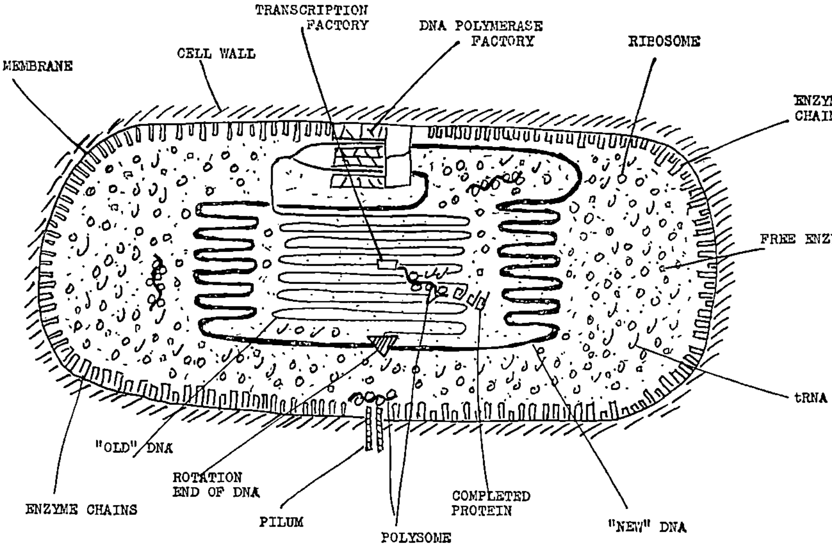
FIG. 11. The formation of messenger RNA by copying one strand of double-stranded DNA.

Our last factory is the one which makes messenger RNA. If we accept the evidence from Dr. Berg's laboratory, [12] then both strands of DNA must be present. Quite a variety of evidence suggests that only one of the strands is "copied." So in Figure 11 we have tried to show this. The messenger RNA is the tube-like object formed on the continuous strand of DNA with the dotted strand also present. The actual point of synthesis is shown, with the four bases making their way into the synthetic point. To hide abysmal ignorance, the shape of the enzyme has been shown as a simple rectangle. Again we need to think of speed. In this instance the enzyme is moving so fast that one can hardly dare to suggest a comparative speed. It is moving down the page, with sharp streaks of messenger following its motion, and its speed is such that it would be hard to "stop"

with the best electronic flash. Somehow, a ribosome must be involved with the messenger, and we have drawn one in the vicinity to remind the reader of that fact.

A Schematic Picture of the Cell

We are now ready to see what happens when we try to put these features into the confines of a bacterial cell. The result is seen in Figure 12. It obviously calls for comment, in fact it is meaningless without, as it is



SCHEMATIC DRAWING OF A BACTERIUM

FIG. 12. The cell of *E. coli* expressed schematically, embodying the ideas developed in the previous figures.

intended only to focus thought. We can take the DNA first. The "old" DNA is shown as the thin line, starting at the spinning mechanism of Dr. Cairns. It winds back and forth until near the top of the cell it divides and passes through the polymerase factory, which we have shown as attached to the cell membrane. The new DNA is shown as a thicker line and is here indicated as in two beginning daughter nuclei, getting ready to divide. It would probably be better to put these two daughter nuclei inside the old DNA so as to be pushing them outward as the cell progresses. The two ends return to the spinning mechanism.

To take the formation of messenger RNA and polysomes, we have great difficulty. The process is shown as forming on the DNA, and the polysome, transfer RNA, and finished protein are indicated. However,

the impression so gained is too quiet and simple. In reality, the whole of the outer part of the DNA must be covered with the apparatus which makes the messenger and polysomes, and almost the only way to convey the idea of this intensely active part of the cell is to suggest that the whole of the DNA is covered with a swarm of bees, actively making the products.

The component parts of tRNA, free enzyme, and ribosomes are shown at each end of the cell, although there is some doubt as to whether there really are many of these free agents in the cell.

Comment is in order on two other features of the diagram. The first is the small whisker at the bottom, one of the pili which have been studied notably by Maccacaro [15] and Brinton [16]. These are interesting because some of them are involved in bacterial conjugation, and some in the process of invasion by small RNA-containing viruses, as has been shown by Edgell in Ginoza's laboratory at Penn State. Each of these, at a conservative estimate, must contain 120 protein molecules, all identical, because a single mutation can remove the whole whisker. Now, according to Levinthal [17], the output of a single polysome is 20 protein molecules, after which the messenger RNA has decayed, and so more than one polysome must be involved in the synthesis of one of the pili. Since it is not easy to see how a very large object like a polysome can diffuse readily in the cell, it seems probable that a set of six or so messenger RNA molecules are made together very near to the place where the "pilum" is being formed. This argues a kind of deliberate action on the part of the molecular apparatus of the cell, and it is significant that this type of process must be taking place in other places. Put very strongly, the cell has a design for each of the pili, and so, very probably for many other of its features.

The last feature which needs comment is the presence of sets of enzymes near the surface of the cell. This kind of organized set of enzymes is found in the electron transport system of mitochondria [18], and evidence for something like it was found by Kempner and the author [19] some years ago at Yale. In looking at the radiation sensitivity of the ability of the bacterium to incorporate glucose into the macromolecular fraction of the cell, it was found that the sensitivity was far greater than that of one enzyme molecule. Now if the enzymes responsible for the whole process of assimilating glucose were all separate, then the inactivation of individual enzymes would not have much effect, for there are hundreds in the cell. On the other hand, if the enzymes are in grouped units together, then the inactivation of any one enzyme will destroy the operation of the group and the sensitivity is explained. Because the processing of metabolites should take place near the entry point of the materials into the cell, these systems of enzymes have been placed near the surface.

To continue the process of understanding the nature of the cell, two more strokes of the brush should be added to this picture. One is Figure 13, in which an attempt has been made to portray the section of the DNA right through the middle of the cell. It is roughly to scale, with the DNA being 20 Å in diameter and the separation between strands about 100 Å.

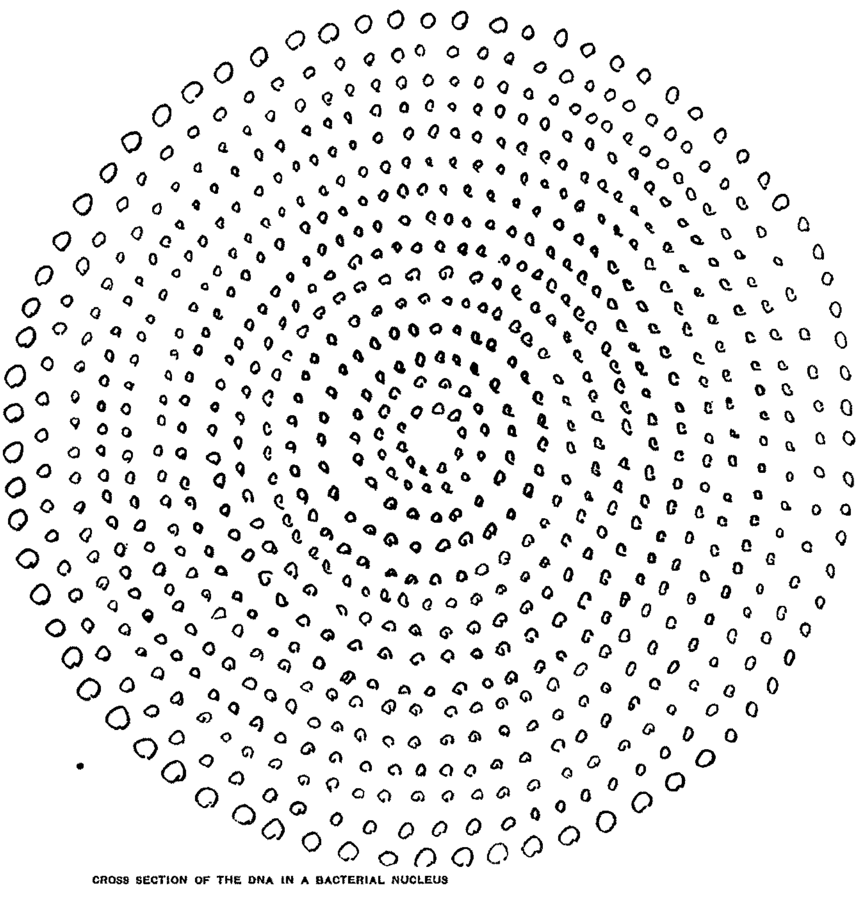


FIG. 13. An attempt to show a cross section of the DNA in the cell.

The figure is very laborious to draw and it is quite easy to get lazy about it. Nevertheless, it can be seen that a very large number of strands of DNA have to be shown in this cross section, in fact about 900 of them. The concentration of the DNA in this way is significant just alone, but two things need to be said about it in addition. That is that, at least on the surface of the DNA where this is moving into the polymerase factory, the DNA is actually in quite rapid motion. If one were to try to represent

it, then the reader should draw his finger in and out just about three inches on this scale at about the rate of one in and one out every two seconds. In other words, the DNA, as it moves into the replication apparatus, is moving at the rate of one micron per second. Furthermore, if the transcription process is primarily taking place in the outer layers of the DNA, then there is a very rapid peeling off of the messenger RNA all around the surface of this DNA. This remarkable activity associated with this many-convoluted kind of worm is again a very striking and curious part of the cell.

The last stroke of the brush is shown in Figure 14, in which the surface of the cell has been indicated. This must, once again, be thought of as

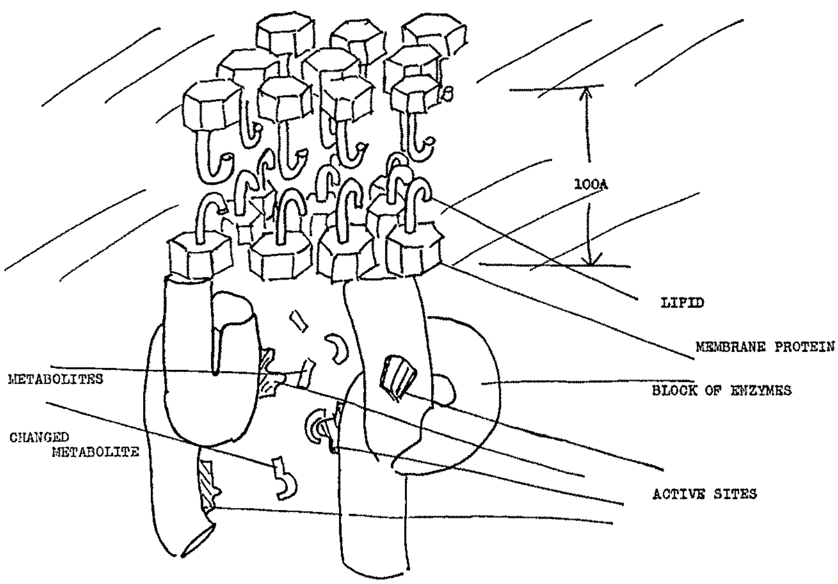


FIG. 14. The membrane and associated enzymatic structures.

schematic and is intended to indicate the association of lipid and protein with the lipid as the little hooked candy cane type of structures and the protein as the hexagonal larger units. This follows the suggestion of Dr. J. F. Danielli, and the separation between the layers is probably somewhere in the neighborhood of 100 Å as indicated. Many more of these should be drawn, but the fatigue of indicating them grew great and it is better just to rely on the idea of a schematic picture. In order to understand correctly what is happening, however, one should recall the dynamics of the operation of the cell; this means that, during the time the reader has been reading the words about the membrane, probably one more lipid molecule has appeared in the surface, and possibly one more combination of lipid and protein. One has to realize that the cell mem-

brane is not of the nature of a balloon, which expands without changing the amount of material in it, but instead is a mechanism which actually has more and more material arranged in this layered way as time goes on. So that again, as we have continued to write this paragraph, another unit of membrane has appeared in there. It must also be recalled that, in all probability, material for this membrane must be fed from the inside of the cell outward. How to do this is of great interest, and is not solved at the present time.

Below the membrane are represented the systems of enzymes which are supposed to synthesize the necessary metabolites. These systems of enzymes are drawn in a non-geometric way to stress the fact that they

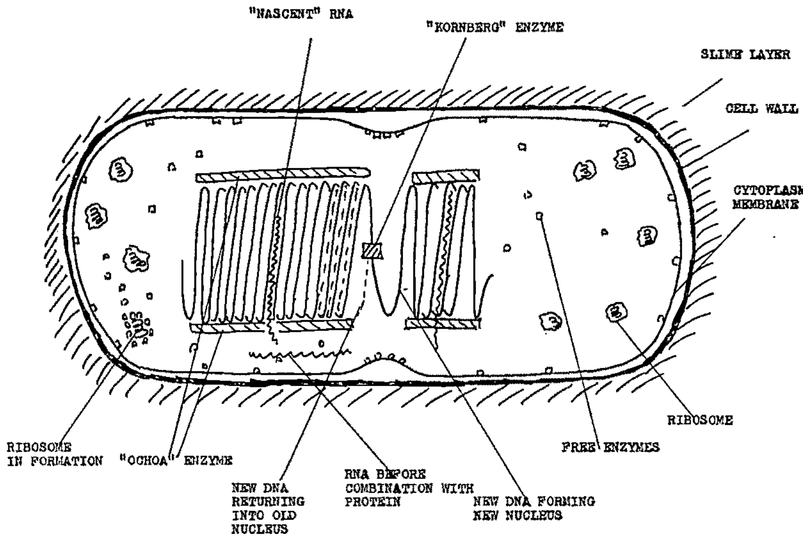


FIG. 15. An early (1960) representation of the cell for comparison with Figure 12.

are living systems; one must realize that they are in fact ten enzyme molecules or so put together. A few of the enzymatically active surfaces have been indicated, one of which, for example, will collect the curved molecule on its surface and there join to it the straight molecule to produce the sickle-shaped object further down. This is a representation of the process of the assembly of the metabolites from the materials supplied by the outside environment of the cell. In thinking about this picture one should also recall that something must be going upward to the membrane as well, so that this is truly a remarkable structure.

The author cannot avoid looking at the picture and realizing that it must not only contain insight about the cell, but probably insight about himself and that it might be valuable material for a psychiatrist in explaining the workings of his mind.

It could easily be wondered whether these imaginative pictures, which portray the nature of the cell, have any reality or not. In order to establish some basis for judgment, in Figure 15 is shown the picture which was drawn under similar circumstances in 1960 [20]. The comparison between this picture and the one in Figure 12 is of interest, because it affords some idea of whether these pictures do represent something which is moving forward to reasonably firm conclusions about the workings of the cell, or whether they represent something which is almost random and not really connected to reality at all.

In the 1960 picture, several features can be seen which are reasonably correct and several features are not so. For example, in 1960, the picture did contain one point of DNA replication, although it was not put on the cell membrane. This seems to have been before its time and quite reasonable. On the other hand, the DNA has ends, which is against the modern idea that DNA is a continuous loop. Also in 1960, the RNA is indicated as being made on the DNA, which is reasonable in the view of today's thinking, but in 1960 there was no concept of messenger RNA, and in fact our thoughts in these "old" days were that the RNA was made on the ribosomes and these ribosomes proceeded to make the protein without any assistance from messenger. This is not held to be true today. One can therefore see that these pictures do seem to represent a steady advance, and it does not seem to the author to be unreasonable to make the claim that if, in 1970, a group of scientists resident in Japan, Russia, France, the United States, and Sweden were brought together by a UNESCO conference to compare their pictures of the same cell, that these pictures would look remarkably alike and that they would really bear some relationship to the truth.

The author would therefore like to end this section on the optimistic note that the inferential method, or the method of accurate imagination, does have the ability to tell us something about the structure of the cell and that this schematic drawing which we have just produced and the discussion we have used is really something that we can consider firmly in our idea of how to go about attempting to synthesize such a cell.

Ways of Synthesizing a Cell

It is now our task to try to find ways in which this complicated structure could be synthesized. We can consider one very quickly and dismiss it. If it is necessary to assemble the cell we have just described as we would assemble a watch, that is to say to make separately every individual component, to put them together and then, so to speak, pat it on the back and it will start up, then I do not think we are ever going to do it. I do not believe we are going to be able to make the DNA in one long strand, or thread it through the polymerase apparatus, pull it out on the far side, loop it around, put the transcription mechanisms on the out-

side, encase it in its beautiful membrane with the enzyme structures close to it, and then pat it on the back and see it start. I believe we may definitely dismiss this right at the outset.

However, to take another analogy, there is a way in which we might think about it. Perhaps we could make the cell like we make a crystal. If we were to make a crystal of alum, then we make a concentrated solution of it, hang a small seed crystal inside and go away, and the following day we find we have a nice crystal and we say we have made it. In a certain sense, we have not made it, and yet we would think we had. What would have happened is that the crystal "wanted" to form, or in scientific terms there were forces which would develop the order which is present in the crystal, and these forces took over and made it under the right circumstances. The question arises as to whether there are forces which would take over and make the living cell if we started it in the same way that we could start a crystal forming. This is a tremendously important question which has roots in both philosophy and in the extent to which our knowledge of the laws of nature is still limited. Before discussing these philosophical points, it might be worth while to look at something slightly less formidable than the concept of actually making a start on a living cell and having it finish itself. What is much easier to contemplate is the idea that we might be able to make a primitive form of a living cell and then to hurry up its evolution. If we are going to make a living cell, and if the analysis here given is of any use, then the method which suggests itself as the best is the one just described. Find out what a primitive cell is like, make that, and then hurry up the evolution by deliberately producing the things we know will one day lead to the proper present-day bacterial cell.

There is some real hope that we might be able to find out something about the primitive cell in the recent work of Hoyer, Bolton, and McCarthy [21]. Their work contains a suggestion that the DNA in cells has been essentially overpreserved. That the necessity which is possessed by the cell keeping its DNA intact may actually have caused so great a restriction on the degradation or removal of DNA, that DNA has accumulated in many cells in such a way that we can look at it even though it is today not very functional. If this should prove to be true, and it is certainly an attractive hypothesis, then we will find stretches of DNA in present-day bacterial cells which represent much earlier stages in its existence. We might be able to infer from these what would be the most primitive we could "get away with" and from the inference we might be able to start the construction of the DNA necessary to operate as an extremely primitive cell. It could be that, in fact, cells in their very early stages were not built to use the "holy trinity" of DNA, tRNA, and protein. We would have to make our conclusions with regard to that and start trying to assemble the very simplest possible kind of thing, and

then by knowing the way in which mutations have occurred, by alteration and extension of the DNA, so add the necessary parts as to bring the cell into being much faster than the thousands and millions of years which it must have taken. It is this method which is suggested as the way in which the cell might be made.

It is now necessary to comment on what would be involved in that. It is legitimate to try to use intuition in making suggestions and predictions. The author well remembers an occasion when he was an impressionable graduate student in which Lord Rutherford outlined his theory of the atomic nucleus [22]. This was in 1927, five years prior to the discovery of the neutron. In this theoretical account, Lord Rutherford employed already outdated forms of quantum mechanics, he used the modification of the Bohr-Sommerfeld theory with half quantum numbers, and he used a great many clear infractions of the best theory of the day. It did not fare well, and I well remember the polite but insistent grilling to which this great man, at that time essentially twice a Nobel laureate, president of the Royal Society, recipient of the Order of Merit, and perhaps the greatest figure in science of his day, was subjected. I remember Lord Rutherford's face grew redder and redder until finally he brought his hand down on the table and he said "Gentlemen, I feel it in my bones that there are neutrons in the nucleus." It is hard to say whether his theory was a good theory or a bad theory in the light of the fact that he was right on this major premise. What we are asking ourselves is what do we "feel in our bones" with regard to the living cell and the problem imposed with regard to its synthesis. We have already discounted the idea that we could put it together like a watch, and we have suggested the best way would be to think about an early cell, to make it, and then to hurry up its evolution. The question is does the cell automatically contain something which has an orderly and regular behavior, even though it has complex components, even at the most primitive stage at which we could possibly recognize a cell? This is a very crucial question and one which is at the heart of the entire situation we are discussing. If it is indeed possible that the semi-chaotic behavior of chemical systems can lead progressively by ramification upon ramification to something as complex, as ordered and, one might almost say, as "thinking" as the cell we have just described, then we should indeed press resolutely forward to show that this is true. And if it is, then there is no question that the greatest triumph for our knowledge of our laws of nature will be in this successful synthesis by what we know at present of a living cell. On the other hand, if it should prove that all our attempts to do this fail, and yet we are faced with the reality of living systems in abundance growing and developing and behaving in the way they do, then we will be forced to contemplate something new and something different about life which is not present in our scheme of nature as it is known today. The question

arises as to whether this type of new thing, which is inherently shown at its maximum in living things, though one would hope that it penetrated the whole of nature, is something we must look for and first find before we can make a living cell *de novo*. This sharp philosophical question is not resolved today nor does it seem simple to suggest the way in which it will be resolved. There is only one good way to go about finding it, and that is by designing experiments resolutely in terms of the concept that the present laws of physics, and of physical chemistry are indeed adequate to explain all living things. Using this idea then push resolutely with this explanation and conduct experiments in those areas where the explanation appears to be marginal. Thus, it would seem to be wise if one is really interested in this kind of question to look not only at the structure of cells, but at the way the structure is changing with time. In other words, it is the rates of synthesis perhaps and not the actual accuracy of synthesis which may pose the challenge. Put more succinctly, since living things do contain the fourth dimension, we must study all four dimensions if we study living things with great care.

Whether these studies and this attitude will result in a completion of the project mentioned at the beginning of this lecture, namely the logical assembly in our minds, step by step, of the way to put together a living cell, or whether we will find steps that are missing that cannot be completed, is still open to question. It is because this question is open that the work is so exciting and so demanding and that one does feel this is indeed the great frontier in science today.

In one concluding remark, I would like to return to the analogy of the scientific revolution that occurred early in this century. It is true that the essential understanding of inanimate nature was obtained within forty years of the discoveries made at the turn of the century. However, in making these discoveries, even though great adaptations had to be made early, as in the case of the Bohr theory, the final confrontation of good accurate theory with the problem of two electrons in an atom led to one of the greatest surprises in physical science of all time, namely the idea that we must substitute an explanation in terms of probability for an explanation in terms of continuous certainty. This substitution did not diminish our skill and understanding: Rather it increased it. We are now able to make confident predictions about atomic behavior, about chemical structures which are based on this theory. Nevertheless, it must be conceded that the theory as it came out produced not only surprise but, it would be fair to say, shock.

The question that is fairly before us, and posed by the problem of the artificial synthesis of a living cell is the question as to whether life and its understanding will also bring with it to the scientific community, now as a whole, the same degree of shock as was produced then.

REFERENCES

A great many competent research workers have created the field of molecular biology which is partly described here. To do correct justice to the development of each topic is beyond the author and not really required in covering the subject of cell structure. To supplement the specific references given later, two general references are suggested here, from which a more complete appraisal of the subject can be obtained.

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This Nobel Prize address covers many topics with care and clarity and is very useful reading.

B. *Cold Spring Harbor Symposium,* 1963. Cold Spring Harbor Laboratory of Quantitative Biology. Volume *28.*

This most important volume is the closest one can come to a complete and authoritative account of the molecular biology referred to in this article.

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AMERICAN SCIENTIST 53, 1965

CAUSES OF RELATIVE SEA-LEVEL CHANGES[1]

By CHARLES G. HIGGINS

Anyone who has spent more than a few hours at the seashore along most coasts has seen that the level of the sea is not constant. Not only is it affected by ordinary ripples, waves, swells, and other minor undulations of the sea surface produced by frictional drag of winds blowing across it, but it rises and falls in response to many other influences. The most regular of these are the tides, generated by the gravitational attraction of the moon, to a lesser extent of the sun, and to a negligible extent of other bodies in the solar system (*see* review by Rossiter, 1963). The period and height of the tides vary from place to place and from time to time according to planetary and local factors which, though complex, are well enough understood to allow fairly accurate tidal predictions to be made and published far in advance for many ports and coastal stations.

SHORT-TERM VARIATIONS IN RELATIVE SEA LEVEL

Variations in barometric pressure, caused by daily and seasonal temperature fluctuations and changing weather patterns, also affect local levels of the sea. Where a barometric low is combined with an on-shore wind that blows constantly for several days across a broad expanse of water, the sea level in the vicinity of the low-pressure region may rise markedly. Along parts of the coast of the Aegean Sea, especially in long, restricted bays such as the Gulf of Evvia (or Euboia), such sea-level changes in response to barometric pressure and winds commonly exceed those produced by tidal effects (Kolokithas, in press). On September 8, 1900, low pressure and hurricane winds caused sea level to rise almost 5 m in less than 12 hr at Galveston, Texas (Sverdrup, *et al.*, 1942, pp. 544–545).

Where the water level has been raised by low pressure and winds at one end of a long, narrow basin, a *seiche* may be generated if the weather pattern changes abruptly. Seiches are long-period standing waves whose wave length is commonly either equal to or a multiple of that of the basin. The name itself is Swiss-French, reflecting earliest observations of such waves in Lake Geneva. Their effect can be observed in any long

[1] This is a revision of a paper to be published in the *Proceedings* of the Second International Conference on Underwater Archaeology, held April 15–17, 1965, at the Royal Ontario Museum, Toronto, Canada. This study is a result of work undertaken under financial sponsorship of the Geography Branch, Office of Naval Research, Contract Nonr-2211(02), Project NR 388-070. Reproduction in whole or in part is permitted for any purpose of the United States Government.

trough that is partly filled with water: when one end of the trough is raised slowly and then abruptly lowered, part of the resultant oscillation of the water level represents a seiche formed by the water traveling rapidly back and forth in the trough. At the south end of Lake Michigan, seiches, combined with storm surges and other gravity waves, have caused numerous drownings and considerable damage to harbor installations and boats (Donn and Ewing, 1956).

Other large occasional waves may be generated by submarine volcanic eruptions or by sudden dislocations of the sea bottom caused by submarine landslides or movements along faults associated with earthquakes. A graphic description has been given by Murata (1961) of the disastrous arrival at Hilo, Hawaii, of such a wave generated during the great Chilean earthquake of May, 1960. After traveling 11,000 km at a speed of about 700 km per hour, this wave, channeled in Hilo harbor, smashed against the town as a wall of water 6 m high. These seismic sea-waves, or *tsunamis* (originally a Japanese word), are commonly miscalled "tidal waves" in the popular press, doubtless in confusion with steep, abrupt tidal bores observable in some constricted inlets and river mouths (*see* Kuenen, 1963, p. 41).

Waves similar to tsunamis may be generated along steep coasts by rapid coastal slumps or landslides. There is no specific name for such waves, but one of these, apparently started in the Corinthian Gulf by a large slump near Aiyion, washed over part of the Greek city of Patras, some 40 km distant, in early 1963. The largest known of such waves occurred July 9, 1958, in Lituya Bay, Alaska. Apparently generated by a landslide or rockfall, in turn triggered by an earthquake, the crest of the wave—a gigantic splash—scoured away trees and soil 370 m above sea level near its origin (Roberts, 1960).

Comparatively minor daily and seasonal fluctuations in sea level also result from thermal expansion and contraction of sea water and from local or general fluctuations in the Earth's water budget. The latter have been cited by some writers to explain the considerable fluctuations in levels of the North American Great Lakes. These fluctuate 0.3–0.5 m seasonally, and the mean levels fluctuate 0.6–0.8 m over longer periods, claimed by some writers to be cyclic.

These and some other factors responsible for variations in marine water levels, together with the magnitudes of their effects, are comprehensively reviewed by Lisitzin (1963). All of these changes in water level, however, are transient; they represent merely short-term fluctuations or departures from mean water levels, which, averaged over a decade or a hundred years, are found to remain relatively constant. While such short-term fluctuations may be locally significant and important in connection with problems of coastal erosion and deposition and the protection and maintenance of harbors and waterfronts, they cannot account for such

features as submerged stream courses on the floor of San Francisco Bay or on the Sunda Shelf east of Sumatra, submerged ancient settlements in the Mediterranean Sea, raised and tilted marine erosion platforms or terraces along many coasts, or coral reefs more than 1000 m above the sea in the East Indies. Such features must reflect local or world-wide, abrupt or slow, cumulative but absolute changes in the relative positions of the land and sea.

LONG-TERM VARIATIONS IN RELATIVE SEA LEVEL

Some writers (Gutenberg, 1941, Kuenen, 1950) believe they can identify in the tide gauge records of some ports, a rise of mean sea level of 10 to 20 cm during the past century. Such rates of relative submergence cannot be world-wide, however, for some of the high-tide monuments installed 150 years ago along the Baltic Sea now stand more than 1 km inland, indicating a relative rise of the northern Baltic coast of more than 100 cm per century. Kaye (1964) has found that sea level at Boston was at about the present level in the mid-nineteenth century, but was about 15 cm lower around 1900. Clearly, different processes must be operating—or at least operating at different rates—in various parts of the world. Thus, to understand what may be happening or may have happened in the past at any particular coastal site, it is essential to know what sorts of processes may affect relative sea level, both world-wide and local. Many of these processes and their effects have been reviewed in detail by previous authors, perhaps most completely by Jelgersma (1961, pp. 9–16) and Fairbridge (1961, pp. 99–113).

Processes that affect relative sea level can be divided into three groups: those that affect the volume of the oceans or ocean basins and thus cause world-wide changes in sea level, those that cause local changes in water level, and those that cause local changes in land level.

Causes of World-Wide, Absolute (Eustatic) Changes of Sea Level

Volume of the ocean basins: If the size or shape of any of the world's interconnecting ocean basins is altered so that its volume is increased or decreased, the level of the sea rises or falls proportionately but equally all around the world. Such an absolute, uniform change of world-wide sea level is called a *eustatic* change. The volume of an ocean basin may be altered by at least four general processes: volcanism, sedimentation, crustal deformation, and isostasy.

Volcanism: Construction of large, submarine volcanos, such as the submerged parts of the Hawaiian Islands, or outpouring of large volumes of lava on the sea floor would diminish the volume of the basin and displace the water, causing sea level to rise. Such an effect would be comparatively slight, however, even if there were no compensating isostatic subsidence of the ocean floor beneath the newly added weight of the

lava or above the evacuated crustal or sub-crustal lava chamber.

Sedimentation: According to Gilluly's compilation of earlier work (1955, p. 14), 13.6 km^3 of sediment are eroded from the continents and deposited in the ocean basins every year. As with submarine volcanism, this would have the effect of raising the sea level—by less than 4 mm in 100 years—although compensatory subsidence of the crust beneath the added weight of the sediments would tend to diminish this effect.

Crustal deformation: Warping or faulting either up or down of the submerged floors, shelves, and other parts of the ocean basins could account for much greater and more rapid volume and sea-level changes than the comparatively negligible effects mentioned above. Marine geologic and oceanographic studies within the last two decades have disclosed evidence suggesting that parts of the ocean floors are being broken by active faults and warped up or down over broad areas. As yet, these effects are not sufficiently well mapped and understood to allow us to make a reliable estimate of their net effect on sea level.

Isostasy: When a sizable load is placed on or removed from the Earth's crust, the crust sinks or rises in compensation by a process of adjustment called isostasy, explained in more detail later in this paper. Isostatic sinking of the ocean floors under volcanic or sedimentary loads may diminish or even negate the effect of such loads to decrease the volume of an ocean basin. Similarly, isostatic responses to changes in the volume of water in the oceans may diminish the effects such changes may have on world-wide sea level. Robin (1962) has estimated that melting of all the Antarctic ice could raise world sea level 60 m, but that compensatory sinking of the ocean floors would reduce the effective sea-level rise to 40 m. Since isostatic adjustments are thought to operate slowly, requiring as much as 5000 to 10,000 years to accomplish only half of the total adjustment, compensation is not immediate. Thus, where a load is rapidly added to the ocean floor, sea level may first rise in response to the decreased volume of the basin and then may fall back in response to isostatic sinking of the ocean floor under the load.

Amount of water in the seas: The other chief factor influencing eustatic sea level is the total volume of water occupying the ocean basins. This varies seasonally and perhaps cyclically over longer periods as mentioned above, but is also affected by several longer-term factors.

Variations in water volume with temperature: Seasonal warming and cooling of sea water may cause minor fluctuations in its volume, but responses to general increases and decreases in mean atmospheric temperature during glacial maxima might be expected to produce slight but long-term volume changes. Munk and Revelle calculated that "an increase in the temperature of all the ocean water by 1°C. would raise the sea level by 60 cm!" (1952, p. 830). Depth and distribution of oceanic temperature changes during glacial epochs are not well known. Emiliani

(1955) proposed that surface-water cooling may promote increased vertical circulation, thereby distributing cooler water to considerable depths; however, his oxygen-isotopic temperature studies suggested that, while equatorial Atlantic and Caribbean surface waters were as much as 6°C. colder during glacial maxima, eastern equatorial Atlantic bottom temperatures were only about 2°C. lower and equatorial Pacific bottom temperatures were about the same as at present. This, in turn, suggests that the average temperature of the world's seas may have been only one or two degrees lower during the last glacial maximum, and that the effect of subsequent warming has raised sea level only a meter or so.

Addition of juvenile water to the seas: Each year more water is added to the Earth's surface supply from the Earth's interior. Most of the gas erupted by volcanoes is water vapor, and some of this is juvenile water—that is, it is brought to the surface for the first time from sources deep within the crust or subcrust. Other juvenile water, released by crystallizing igneous bodies far underground, rises quietly to the surface in hot and cold springs.

The volume of new water added yearly to the Earth's surface is difficult to estimate, but Conway's (1941) indirect computation that 4 to 8.8×10^{22} g have been added since Jurassic time probably represents a maximum. This amount of water would have raised sea level about 170 m within the past 140 million years—an average of about 1 m per million years.

Effects of glaciation and other long-term variations in the Earth's surface-water budget: Of the total 1.4×10^9 km^3 of water on and in the upper part of the Earth's crust about 97 per cent is in the seas. Of the remaining 3 per cent, about two-thirds lies on the land in lakes, ponds, marshes, and streams or moves slowly underground as ground water in the upper, porous parts of the crust, and about one-third is stored in snow banks and glaciers. Only about $\frac{1}{1000}$ per cent of the Earth's water is in the atmosphere, and an unknown fraction is free or chemically combined with other elements in plants, animals, and their organic products, and in weathered and hydrous minerals (figures from Sverdrup, *et al.*, 1942, p. 15, and Kuenen, 1963, p. 14). Seasonal climatic changes affect the amount of water that is, in effect, extracted from the oceans and stored on the land and in the atmosphere, and one should expect that longer-term, major changes in climate or in size and configuration of the continents should also be reflected in changes in the surface-water budget.

The most notable climatic event in the Earth's geologically recent history is related to the recession of the great glaciers and ice caps of the last glacial maximum. Tremendous amounts of water were stored in these ice masses, and this effectively reduced the volume of the seas and lowered their level. Many writers have attempted to determine, in various ways, how much the sea level was lowered during the last glacial

maximum; most conclude that it was lowered about 100 to 150 m below the present level. As the glaciers have melted and receded, their water has returned to the oceans and sea level has risen. Much water is still stored on the lands as ice—enough to raise sea level at least 15 m higher (estimates run as high as 90 m [Fairbridge, 1962, p. 111])—suggesting that eustatic sea-level rise attributable to glacial melting is an actively continuing phenomenon.

The rate of glacier melting during the past 20,000 years has not been constant, however; during the general recession there have been periods of renewed glacier growth. Hence, if one supposes, as many writers have done, that changes in post-glacial relative sea level are attributable chiefly or wholly to glacier melting, then one should expect to find variations and fluctuations in the rate of rise of post-glacial sea level, and one should even expect to find evidence that the sea level has fallen at times. Evidence for such fluctuations and reversals seems to be absent at many coasts that have been studied (see Jelgersma's excellent summary of her own and earlier work, 1961, pp. 42–71), suggesting that, at these places, glacio-eustatic changes may be overshadowed or masked by the effects of one or more other factors that affect relative sea level.

Causes of Non-Uniform Changes of Sea Level

The factors considered above provoke uniform changes in world-wide sea level. Some others affect the level of the sea non-uniformly or only locally.

Changes in the Earth's rate of rotation: Tidal effects between the Earth and Moon result in an exchange of energy which causes the Earth to rotate more slowly and the Moon to revolve faster. Slowing of the Earth's rotation would have the effect of decreasing its equatorial bulge. If the oceans responded immediately to this decrease and the crust responded much more slowly, the net effect would be to lower the relative sea level in the low latitudes and to raise it in the polar regions. The rate of slowing of the Earth's rotation through tidal friction and loss of energy to the Moon is very slight, and any resultant changes of sea level during historic time would probably be undetectable, but Eardley (1964) has suggested that this process and its effects may account for differential sea-level change of 180 m within the past 100 million years.

The Earth's rate of rotation is also affected by the distribution of mass within it and on its surface. Movement of a mass from a lower to a higher elevation at the same latitude would increase the Earth's moment of inertia and decrease its angular velocity. Reverse movement would have an opposite effect. We know little or nothing about subsurface redistribution of mass within the Earth, but several of the surficial processes previously mentioned could be expected to affect the Earth's angular velocity: eruption of volcanoes and lavas, uplift of mountain

belts, and even expansion of sea water through post-glacial warming would all be expected to reduce rotational velocity; on the other hand, crustal subsidence and transportation of sediments from higher elevations to lower ones would tend to increase it.

Movement of masses pole-ward or equator-ward also affects the moment of inertia. Thus, seasonal and longer-term changes in oceanic currents of varying densities, redistribution of sediments, and lateral dislocations of crustal masses would affect rotational velocity.

Most of these effects are negligible, and some of them tend to cancel each other. However, redistribution of ice and water accompanying glacier recession since the last glacial maximum may have had a significant effect on the Earth's rotational velocity. Most of the vast quantity of water removed from the oceans was not only elevated onto the land, but was concentrated near the polar regions. Thus, during the glacial maxima, the Earth should have rotated more rapidly and had a correspondingly greater equatorial bulge. As the glaciers melt and their water returns to the oceans, the Earth's rotation is slowed and its equatorial bulge decreases, with the net effect that sea level rises at a slightly faster rate in the polar regions than in the equatorial belt.

Although these various results of changes in the Earth's rate of rotation have theoretical significance and perhaps even practical implications for the Earth's geologic past, their effect in historic time has been negligible. During the past 3000 years the length of the day has increased only about 0.04 sec (Brouwer, 1952) in response to the combined effects of all the factors that influence the Earth's rate of rotation.

Changes in position of the Earth's axis: An angular shift in the position of the Earth's axis of rotation would cause a corresponding shift in the position of the equatorial bulge which in turn would have the effect of raising the relative sea level in some places and lowering it in others, provided that the Earth's crust did not respond immediately to the new stresses. Fairbridge (1962, p. 130) has calculated that a 1-degree polar shift would lower the sea level near the new poles and raise it as much as 260 m at the new equator.

Axial displacements, like changes in the moment of inertia, could result from redistribution of mass on or within the Earth. The positions of the poles have been observed to vary annually by several feet, possibly in response to seasonal changes in distribution of air, ice, and water on the surface. However, these masses are negligible compared with the vast bulk of the Earth, and the effects of even major changes in their distribution would be comparatively slight. Munk and Revelle (1952) calculated that a eustatic sea-level change of 1000 m would cause the poles to move only 16 km. Lateral migration of sizable masses of the crust—so-called continental drift—could effect larger axial displacements, and perhaps have done so in the geologic past; but most evidence suggests that the

continents and the poles have not been far from their present positions during the past several million years. However, as the exact positions of the poles during this period are not known, it is possible that periodically they may have been displaced enough to produce measurable differential changes of sea level.

Changes in atmospheric and oceanic circulation: World weather patterns are thought to have been different during times of glacial maxima. Changes in configuration of continents and land barriers also affect atmospheric and oceanic circulation and may act to shift major oceanic currents. Lisitzin (1963, p. 35) summarizes reports that stations on opposite sides of the Gulf Stream have differences of mean sea level of about 0.5 m. Hence, major changes in prevailing winds, barometric pressure areas, and ocean currents could cause local sea levels to change by as much as one-half meter or more.[2]

Causes of Changes of Land Level

All of the ups and downs of the land that may be reflected in relative sea-level changes are essentially local. Some may affect broad areas thousands of kilometers wide, but none is of world-wide extent. Several processes are involved, and the possible actions or interactions of all of them must be evaluated at each coastal site where a change in relative sea level is thought to have occurred.

Crustal deformation: Where crustal deformation, or tectonism, takes place by fracturing, faulting, bending, or flowage of rocks along narrow zones or in mountain belts it is referred to as *orogenic* ("mountain-making") deformation. The areas affected are generally tens of kilometers wide and hundreds of kilometers long. On the other hand, where deformation is expressed as gentle upswelling or subsidence affecting broad areas of the Earth's surface hundreds or thousands of kilometers in diameter, such deformation is called *epeirogenic* ("continent-making").

The results of historic orogenic movements, most commonly associated with active faults, have been observed and measured in many places; elsewhere such movements may be occurring, but at rates too slow to be detected. Gigout has suggested that areas affected by orogenic forces in pre-glacial times, as evidenced by deformed late Tertiary strata, are particularly likely to have been subject to post-glacial orogeny (1956, p. 76; also see Fairbridge, 1962, p. 112), but this rule furnishes, at best, only a partial guide or key to areas that may now be undergoing orogeny. Results of broad epeirogenic doming or downwarping are even more dif-

[2] In a paper presented at the 1964 Geological Society of America annual meeting at Miami Beach, Florida, W. W. Easton suggested that seasonal changes in storm paths and differences in erosion processes acting on the shore may account for concurrent formation of two or more wave-cut platforms either above or below mean sea level. This would give the *appearance* of a sea-level change where, in fact, there had been no change.

ficult to recognize except over long periods because these processes operate very slowly.

Volcanism: Fluctuations in the volume of molten rock, or *magma*, underground beneath active volcanic areas, are reflected in elevation or depression of the Earth's surface in their immediate vicinity. After massive eruptions, which deplete the magma chamber, broad tracts surrounding the volcano subside, and, if near sea level, may be inundated. The classic example of submergence and re-elevation, by at least 6 m, of the Roman "Temple of Jupiter Serapis" at Pozzuoli, not far from Mt. Vesuvius, is probably attributable to these causes, but the similar history of the somewhat older sanctuary of Hera at Vravrona (or Brauron), Greece, should probably be attributed to crustal deformation or other causes, as there is no evidence of recent volcanism in that area.

Slumping: Along cliffy coasts, waves attack and undermine sea cliffs causing their upper parts to slide or tumble, either as single pieces or as loose masses of rubble, down to the shore where they are further broken up and washed away. In this manner, the sea advances and the cliff recedes, in places at so rapid a rate that ancient coastal settlements may be obliterated (Higgins, in press). One of the most dramatic results of coastal recession is the island of Helgoland in the North Sea: its periphery is now less than 5 km in length, but is estimated, on the basis of historic rates of reduction, to have been about 70 km in 1300 A.D. and about 200 km in 800 A.D.

Where fractures or bedding planes dip seaward, or where thick, relatively homogeneous, impermeable rocks form the sea cliff, slumping may occur: a single large block slides seaward along a curved fracture, or slip surface. Where downward movement is less than the width of the block its surface commonly remains intact, preserving original relationships of surface vegetation or structures, but abuts against the steep, arcuate cliff of the exposed slip surface. Buildings, streets, and walls of coastal settlements carried underwater by large slumps may thus suffer relatively little damage. With greater or faster downward movement, the block is likely to break into pieces, becoming an avalanche or mudflow, depending on the rate and character of the movement. In such cases, surface structures are likely to be destroyed and buried in the resulting rubble. Most slumps are relatively small, but records of slump blocks and complexes more than 1 km wide are not uncommon.

A classical example of submergence by slumping occurred in 373 B.C. near Aiyion, Greece, when an earthquake triggered a slump that carried much of the city of Eliki (or Helice) into the Corinthian Gulf, where, according to the ancient historian, Pausanias, many of its remains were still visible in the water several centuries later.

Collapse or subsidence of caverns: Roof-failure, either through sudden collapse or gradual sinking, over man-made excavations in mines or over

natural openings dissolved in limestone, gypsum, or salt may account for some instances, probably rare, of coastal submergence. I know of no historic examples, but some of the semi-submerged sink-holes in limestone along the Adriatic and Aegean coasts may have formed by cavern collapse in pre-historic times as suggested by the Greek name, *vouliagmeni* ("sunken place"), for such features.

Compaction of sediments: Subsidence of areas underlain by unconsolidated sediments may result from compaction of marls, clays, and muds. About one-half the volume of newly deposited water-laid mud consists of space occupied by water. If this water is forced out by changes in the hydrologic conditions or by loading (as a result of additional sedimentary overburden or erection of man-made structures) the remaining sediment compacts into a denser mass with smaller volume, the total thickness is reduced—by as much as 40 per cent in some sediments—and the surface sinks. Similar results follow decrease in mass of marl or clay by leaching of its soluble salts or oxidation of organic matter that may have been deposited with it. The latter is a leading factor in post-depositional compaction and subsidence of peat-bog deposits, which may thus shrink to as little as 10 to 15 per cent of their original thickness (Bennema, *et al.*, 1954). The celebrated gradual subsidence of Venice, Italy, may owe in large part to compaction of the sediments forming the islands on which the city is built.

Sandy and gravelly sediments are much less subject to compaction, but in some cases water-filled loose sands may spread laterally or be hydraulically "piped" out from under a sedimentary overburden, which then subsides. This mechanism has been advanced to explain the extensive slumping and subsidence that affected a coastal area of more than one square kilometer in the Turnagain district, Anchorage, Alaska, during the 1964 Good Friday earthquake. In this particular instance little of the land sank beneath the sea, but along flatter coasts more extensive submergence might be expected to result from similar processes.

Isostatic adjustments: The Earth's crust is thought to be in delicate equilibrium, so that where a sizable load is added to it, it sinks. This slow sinking may be accommodated by lateral flowage of subcrustal rock away from the area under the load, perhaps producing a low annular bulge at the surface around the sunken loaded area. This process is called isostatic adjustment. The rate of adjustment is thought to vary according to the dimensions of the load and the dynamic viscosity of the subcrustal material, and the rate gradually diminishes as adjustment is approached. This is a very slow process; estimates of the time required to accomplish only 63 per cent of a total adjustment range from 4000 to 14,000 years (Crittenden, 1963, pp. 21–24); hence, regions continue to sink long after their loads were first applied.

Examples of loads that may upset isostatic equilibrium are lava fields

and large volcanic complexes, accumulations of sediments in large deltas and basins, continental glaciers and ice caps, and large, deep lakes. Volcanic and sedimentary loads are generally added rather slowly, so that the areas affected may be subsiding—and will continue to subside after deposition ceases—but the amount of isostatic imbalance at any given time may be slight. The comparatively sudden and massive loads of ice added to the continents during times of glacial maxima, however, caused these areas to sink more than 300 m under their loads in many places. The similar, though smaller, loads of water in Ice-Age lakes, such as Lake Bonneville, in western U. S., also produced isostatic sinking.

Decrease of load has an opposite isostatic effect. There is evidence that mountain belts rise as they are eroded, and very likely the continental shelves rose during glacial maxima, when all or part of their water load was removed.

Most of the evidence for modern isostatic adjustment comes from areas responding to changed post-glacial conditions. In Fennoscandia, broad up-arching in response to decrease of ice load has raised parts of the northern Baltic region more than 250 m since the glaciers began to recede about 20,000 years ago. Historical records show that this area is still rising as much or more than 100 cm per century. Northern North America has also risen in response to glacial unloading within the past 12,000 years, as shown by tilted post-glacial shore lines of the Great Lakes. Crittenden (1963) has shown that the shoreline of glacial Lake Bonneville is 64 m higher in the center of the basin than at the lake's outlet, presumably as a result of isostatic uparching after the lake dried up [less than 12,000 years ago, according to Broecker and Kaufman (1965)], and that precise leveling and the westward slope of the Bonneville Salt Flat suggest that the center of the basin is still rising.

Until recently, it has generally been supposed that post-glacial isostatic influences on relative sea level were restricted to coasts near areas that had been glaciated. A few writers (e.g.—Munk and Revelle, 1952; Emiliani, 1955; Robin, 1962) have made off-hand references to possible isostatic adjustments of ocean-basin floors in response to changes in volume of sea water during glacial epochs. However, Crittenden's study of Lake Bonneville has suggested to A. L. Bloom and me, independently, that, if the water load in that lake was sufficient to cause isostatic adjustment that may still be continuing, then the loading and unloading of continental shelves during glacial fall and post-glacial rise of sea level must have provoked similar, though smaller, isostatic responses that may also still be continuing.

Bloom (in press) has shown that varying isostatic responses to sea encroachment may explain some discrepancies among records of coastal submergence within the last several thousand years at several sites along the U. S. east coast. Maximum total adjustment could exceed 45 m in

some places, but both the rate and the amount of coastal sinking in response to post-glacial sea-level rise must vary from place to place according to the width and slope of the shelf, the configuration of the coastline, the local subcrustal density and viscosity, and the degree of isostatic adjustment achieved when the sea level was lowered during the last glacial maximum. These variables make it difficult to predict the degree to which isostatic effects have contributed or are still contributing to local changes in relative sea level, but such effects must affect all the world's coasts, not merely those near regions formerly glaciated.

CONCLUSIONS

Any attempts to explain fluctuations of relative sea level indicated by tide gauge records within the last century must take into account almost all of the causes of such fluctuations discussed above. On the other hand, those most likely to have affected any particular coast in a grossly measurable way within the past 20,000 years are: glacio-eustatic sea-level fluctuations and most of the causes of land-level changes. Proper evaluation of all these effects, especially those of isostatic responses to loading and unloading, vastly complicates the task of interpreting the history of relative sea-level changes at any particular coastal site, and this helps to explain why such interpretations are so frequently contradictory.

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AMERICAN SCIENTIST 53, 1965

THE GENETIC CODE, II*

By THOMAS H. JUKES

THE problem of the amino acid code is now widely regarded as having been essentially solved. Experiments in 1964 and 1965 in various laboratories, carried out for the most part with synthetic polyribonucleotides, lead to the conclusion that long sequences of bases in deoxyribonucleic acid (DNA) spell out instructions which are transcribed into ribonucleic acid (RNA) for subsequent translation into proteins by means of the code. This procedure is a central mechanism of heredity, evolution, and life.

The instructions depend solely on the order in which the four bases adenine, guanine, cytosine, and thymine (A, G, C, and T) are arranged sequentially in DNA where they exist in combination with deoxyribose and phosphoric acid as deoxyribonucleotides. DNA consists of long strands of deoxyribonucleotides combined by phosphate ester linkages in the direction of 3′ to 5′ between the deoxyribose groups. Each molecule of DNA is formed by two such strands held together in parallel by hydrogen bonding between AT and GC pairs over its entire length. The strands "run" in opposite directions with respect to the 3′, 5′ deoxyribose linkages. The transcription of the "instructions" takes place when one of the two strands is used as a template in the synthesis of a single strand of RNA, and the sequence of bases in DNA is transcribed into RNA so that T, C, G, and A in a DNA strand direct the respective incorporation of A, G, C, and U (uracil) into single strands of RNA. Three characteristic forms of RNA are known, termed ribosomal RNA, transfer RNA (sRNA) and messenger RNA (mRNA). The last-named bears the information for the translation of the genetic message into sequences of amino acids. These are joined together by polypeptide linkages to form proteins which give rise to all the phenotypic characteristics of living organisms. Since there are 64 permutations of 4 different objects taken 3 at a time, there are 64 ways in which A, C, G, and U in mRNA may occur in a consecutive sequence of 3 nucleotides, termed a "triplet," joined together by 3′, 5′-phosphate ester linkages. The "triplets" may be abbreviated in the general form XYZ or XpYpZ, p denoting the 3′ to 5′ phosphate bridges which are implied in XYZ and indicated in XpYpZ. About 60, perhaps 62, of the 64 triplets are codes for the 20 amino acids used in the biological synthesis of proteins (Table 1), so that an amino acid may obviously have more than one code. Two of the 64 triplets are

* Supported by grant NsG 479 by the National Aeronautics and Space Administration to the University of California. "The Genetic Code, I," appeared in *American Scientist*, June 1963, p. 227.

called "chain-terminating" triplets because they are considered to function as signals for the intervals or "gaps" that terminate the polypeptide chains of protein molecules; these chains start with a free amino or acetylamino group and end with a free carboxyl group.

The "triplets" exist in living cells as the functional units of the long single-stranded molecules of messenger ribonucleic acid. These molecules combine loosely, perhaps by ionic bridges of Mg^{++}, with intracellular particles of ribonucleoprotein termed ribosomes, which have a molecular weight of about 3 to 4 million. Each typical ribosome consists of two subunits, the "30s" subunit which binds the mRNA strand over a distance

TABLE 1

AMINO ACIDS THAT TAKE PART IN PROTEIN SYNTHESIS AND THEIR ABBREVIATIONS

Amino Acid	*Abbreviation*
Alanine	ala
Arginine	arg
Asparagine	asN
Aspartic acid	asp
Cysteine	cys
Glutamic acid	glu
Glutamine	glN
Glycine	gly
Histidine	his
Isoleucine	ilu
Leucine	leu
Lysine	lys
Methionine	met
Phenylalanine	phe
Proline	pro
Serine	ser
Threonine	thr
Tryptophan	try
Tyrosine	tyr
Valine	val

of about 30 nucleotides [1] and the "50s" subunit to which the growing polypeptide chain is attached by means of a molecule of transfer RNA (sRNA) [2].

There are a number of different molecules or "species" of sRNA. Each is specific for an amino acid, which is attached to one end, and for a coding triplet in mRNA, which becomes loosely attached to the sRNA molecule on the ribosome during the formation of a polypeptide bond. Each molecule of sRNA consists of 70 to 80 ribonucleotides ending with —CCA; the terminal adenine-ribose group functions as the carrier of the amino acid in ester linkage with ribose. As each amino acid is added to the growing chain, its sRNA molecule is shed, and another sRNA mole-

cule takes its place, bearing the amino acid whose addition to the polypeptide is specified by the next coding triplet in the messenger, reading from left to right in the conventional formula for RNA.

The appropriate sRNA molecule, carrying with it the specified amino acid, is guided into place by a reaction which selects an sRNA molecule with a sequence which is complementary to the coding triplet "next in line" in mRNA. A suggested identification of 3 complementary coding triplet sequences in sRNA molecules has been discussed by Crick [3]. These are IGC in a yeast alanyl sRNA [4] (I=inosinic acid); IGA in a yeast seryl sRNA [5]; and IAC in a yeast valyl sRNA [6]. These would complement the following mRNA coding triplets, read in the opposite direction: GCC, alanine; UCC, serine; and GUC, valine; by pairing through hydrogen bonds, since A complements with U, C with G, and hypoxanthine (H) (the base present in inosinic acid) with C. Furthermore, as pointed out by Crick [3], I may be ambiguous in its complementarity so that, for example, IGC would pair with GCU, GCA, and GCG as well as with GCC. This would substantially reduce the number of species of sRNA required for protein synthesis.

The identification of the sequences of bases in the coding triplets was accelerated by the report from Nirenberg's laboratory [7] that synthetic preparations of the trinucleotides GpUpU or pGpUpU "recognized" valine sRNA. This discovery was made by combining ribosomes with either of these trinucleotides and then testing the "impregnated" ribosomes for their ability to bind sRNA molecules, each charged with its specific C^{14}-tagged amino acid. Only valine sRNA was found to adhere to the GUU-impregnated ribosomes under appropriate conditions. The ribosomes and sRNA were prepared from the cell contents of *Escherichia coli*. Shortly thereafter, Leder and Nirenberg [8] reported that, under similar conditions, UGU bound cysteine sRNA, and UUG bound one of the leucine sRNA's, termed leucine fraction II. These findings gave heightened significance to the discovery that GUU bound valine-sRNA, because, as will be explained below, valine and cysteine are genetically related through a series of single-amino-acid mutations. Therefore, the sequences GUU for valine and UGU for cysteine enabled a number of predictions to be made for the sequences in the coding triplets of other amino acids. Several of these were substantiated in Nirenberg's laboratory when other synthetic trinucleotides were tested by the ribosome-sRNA binding procedure. For the most part, each trinucleotide was specific for a single amino acid, attached to its appropriate sRNA "adaptor" molecule [9, 10].

It was impressive that the information thus obtained fell into a coherent pattern in which the first two bases of the triplet had a greater degree of specificity than the third: for example, the trinucleotides ApUpU and ApUpC both bound isoleucine sRNA. This had been fore-

shadowed by the finding that in many, but not all, cases, 2 or 3 synonymous codes for an amino acid contained a common base pair or "shared doublet" [11, 12]; a concept which was expressed by Eck [13] in a prediction that the amino acid code would prove to be a "two-and-a-half letter" code.

Another approach to the assignment of coding triplets was made by using "block" copolymers of ribonucleotides [14, 15]. The term refers to a repeating sequence, usually of either two or three bases, such as AGAGAG . . . or AAGAAGAAG . . .. Such "block" copolymers were prepared by appropriate chemical and enzymatic syntheses and were used as "synthetic messengers" in cell-free polypeptide-synthesizing systems of the type originally described by Nirenberg and Matthaei [16]. The copolymer AGAGAG . . ., which contains an alternating succession of AGA and GAG triplets, coded a polypeptide consisting of alternate arginyl and glutamyl residues [14]. However, AAGAAGAAG . . . produced either polyglutamic acid, polyarginine or polylysine, the specificity evidently depending upon how the polypeptide got started. Ribosomes could attach at various points along the polynucleotide chain, and the point of attachment would influence the starting point at which the "message was read." This could be (*a*) as AAGAAGAAG . . . which would be translated from left to right as a repeating sequence of AAG triplets, (*b*) as AGAAGAAGA . . . which would be translated as a sequence of AGA triplets, (*c*) as GAAGAAGAA . . . which would be translated as a sequence of GAA triplets. However, when the results were compared with the assignments made by the "trinucleotide-sRNA binding system" described by Nirenberg's group [7, 10] it became possible to infer that GAA=glu; AGA=arg; AAG=lys; and GAG=glu. Similar experiments with other copolymers showed that UCU=ser, CUC=leu, ACA=thr, CAC=his; GUG=val and UGU=cys [14]. Other studies with "block copolymers" indicated that AUU=ilu; UAU=tyr; and AUG=met [15].

Simultaneously it was reported that a copolymer consisting of ApApAp . . . ApApC coded a polypeptide with the formula lysyl. lysyl . . . lysyl.asparagine COOH, thus implying that the message was "read" from left to right [17], in contradiction of a previous report [18]. The finding showed that AAC was a code for asparagine.

Interest also centered on the search for triplets that signaled the end of a polypeptide chain and the release of its terminal carboxyl group from ester linkage with the final sRNA molecule in the series. Studies with "amber" and other mutants of bacteriophage T4 [19, 20] led to the conclusion that UAA and UAG were the triplets with this function. Some support for this was reported by Nirenberg's group in that the trinucleotides UpApA and UpApG failed to bind any aminoacyl-sRNAs in their system [10].

A striking experiment on the nature of the code was reported by Terzaghi and co-workers [21]. By treating a bacteriophage with an acridine dye, a base may be either deleted from or inserted in the internal sequence of a DNA strand. Either procedure shifts the reading of the genetic message "out of phase" on the right hand side of the change, because the sequence of bases in messenger RNA is read in consecutive groups of 3. As a result, the protein corresponding to the disturbed genetic region may be biologically useless since the portion of the protein molecule on the right-hand side of the change will contain a different series of amino acids from that found in the normal or wild type. Of course, if the change brings about the formation of a UAA or UAG triplet the polypeptide chain will be terminated at that point. Terzaghi and co-workers found that an enzyme, lysozyme, made in cells infected by normal bacteriophage T4, could no longer be made by 2 different mutants of which one was a deletion mutant and the other an insertion mutant. The mutants were then crossed with each other and a crossing in DNA took place between the deletion in the first mutant and the insertion in the second mutant. The new hybrid bacteriophage made a new type of lysozyme that was 50% effective enzymatically. The normal (wild) and new lysozymes were then compared by analysis of their polypeptide chains. Wild-type lysozyme (undamaged) had a peptide with this sequence

. . . thr lys ser pro ser leu asN ala ala lys . . .

The lysozyme that was 50% effective was the same in all respects except that the corresponding peptide region was

. . . thr lys val his his leu met ala ala lys . . .

The interpretation of this result, translated into terms of mRNA coding triplets is as follows:

the original sequence between lys and ala is coded by

. . . AGU.CCA.UCA.CUU.AAU . . .
. . . ser pro ser leu asN . . .

Removing the first A and inserting a G at the end gives

. . . GUC.CAU.CAC.UUA.AUG . . .
. . . val his his leu met . . .

These triplets and their assignments are listed in Table 2. No other codes in the table will similarly explain the results.

The findings in this intellectually satisfying experiment leave little room for doubt as to the triplet nature of the code. Simultaneously, the results show the interchangeability of synonymous coding triplets for

amino acids. The codes for the pentapeptide sequence in the hybrid include triplets that end with A, G, U and C.

The various findings discussed above enable the amino-acid code to be written in the form shown in Table 2. The synonymous occurrence of U and C is abbreviated as "b", of U, C, A and G as "d," and of A and G as "e." Three amino acids have six codes; 5 have four codes; and 10 have two codes. Isoleucine is coded by AUU, AUC and possibly AUA, the last-named assignment being inferred from mutations and from the finding that AUG, but not AUA, bound methionyl-sRNA [8, 13]. The reservation must be made that 6 of the 64 assignments in this table are deduced by homology rather than by direct experimental evidence; these are AGG (arg); AUA (ilu); CCG (pro); CUA (leu); GGC (gly); and UGA (try), so that these assignments are provisional.

Forty-four different mutations consisting of the replacement of one amino acid by another have been described in protein molecules. Early

TABLE 2

THE AMINO ACID CODE

UUb phenylalanine	CUd leucine	AUb isoleucine AUA isoleucine	GUd valine
UUe leucine		AUG methionine	
UCd serine	CCd proline	ACd threonine	GCd alanine
UAb tyrosine	CAb histidine	AAb asparagine	GAb aspartic acid
UAe gaps	CAe glutamine	AAe lysine	GAe glutamic acid
UGb cysteine	CGd arginine	AGb serine	GGd glycine
UGe tryptophan		AGe arginine	

U = uracil; C = cytosine; A = adenine; G = guanine; b = U, C; d = U, C, A, G; e = A, G.

studies with the code, as reviewed previously [12], enabled most of these to be interpreted as single-base changes in the coding triplets. The connection between valine and cysteine mentioned above is through the following series of mutations: val/gly; gly/arg; arg/his; his/tyr; tyr/cys. With codes of GUU for val and UGU for cys, this series corresponds to GUU/GGU; GGU/CGU; CGU/CAU; CAU/UAU; UAU/UGU, and it was found that the trinucleotides GGU, CAU and UAU indeed bound the sRNA's for glycine, histidine and tyrosine respectively [10]. Furthermore, the code in Table 2 enables each of the single-amino-acid replacements to be written in terms of a single-base change in a coding triplet, with one exception, that of asparagine/arginine [22]. Forty-three of the 75 possible single-base changes that can lead to single amino acid changes in the code shown in Table 2 have been described in terms of mutations occurring in at least one protein at one or more locations. This is summarized in Table 3. Further studies will presumably lead to more dis-

coveries in this series of relationships. In addition to the summary in Table 3, all possible single-base interchanges between the "amber" ("gap") triplet UAG and amino acids have been described [19, 20]. UAG (or UAA) was found to give rise to lys, glu, glN, try, ser, leu and tyr following chemical mutagenesis [19]. References to the single amino acid mutations were listed elsewhere [23], with the exceptions of the arg/try mutation in malate dehydrogenase of *Neurospora* [24], and the his/asp replacement in hemoglobin at position β-143, cited in [25]. The mutation at the α-16 locus of hemoglobin A was found to be lysine/glutamic acid rather than lysine/aspartic acid [25].

TABLE 3

AMINO ACID INTERCHANGES THAT CAN OCCUR AS THE RESULT OF SINGLE-BASE CHANGES IN THE CODING TRIPLETS

Amino Acid	*Possible Interchanges**
Ala	*Asp glu gly* pro ser *thr val*
Arg	Cys *glN gly his ilu leu lys* met pro *ser thr try*
AsN	*Asp* his ilu *lys ser* thr tyr
Asp	Glu *gly his* tyr val
Cys	*Gly* phe ser *tyr* try
GlN	*Glu his* leu lys pro
Glu	*Gly lys val*
Gly	*Ser* try *val*
His	Leu pro *tyr*
Ilu	Leu lys *met* phe ser *thr val*
Leu	*Met phe pro ser* try val
Lys	*Thr* met
Met	*Thr val*
Phe	*Ser tyr* val
Pro	*Ser thr*
Ser	Thr try tyr

* The italicized examples have been reported to occur in mutations.

A comparison of homologous residues in the polypeptide chains of the various hemoglobins and myoglobins in terms of the code in Table 2 enables some tentative conclusions to be drawn with respect to the frequency with which purines and pyrimidines become replaced by each other in the globin genes during evolution. The approximate result is as follows: transitions (purine/purine and pyrimidine/pyrimidine interchanges), 293; transversions (purine/pyrimidine interchanges), 548. This is a proportion of about 1:2 which may reflect the fact that statistically there are twice as many possibilities for a purine-pyrimidine base pair in DNA to exchange with a pyrimidine-purine base pair (for example, A:T can exchange with either T:A or C:G) as with a purine-pyrimidine base pair (for example, A:T can exchange only with G:C).

The comparison of the hemoglobins also indicates that there are 21 base differences between the genes coding for the α chains of human hemo-

globin A and horse hemoglobin, and 34 such differences between the two corresponding β-chains, the disparity perhaps mirroring the more rapid rate of evolutionary change of the β chain than of the α chain [26]. In terms of the entire mammalian genome of approximately 3 billion base pairs in a haploid cell, the average of these two differences would correspond by extrapolation to a rate of evolutionary change of the order of one base pair per year in the total DNA.

Differences in the base composition of the DNA of various bacterial species have for some time been known to exist on a taxonomic basis. The subject was reviewed by Marmur, Falkow and Mandel [27]. Higher plants and animals have DNA containing approximately 38 ± 10% G + C, but in microorganisms the G + C content ranges from 30% to about 75%. This wide range may be rationalized on the following basis; most of the base composition of DNA apparently reflects the composition of messenger RNA, only a small proportion of the DNA being used in the production of ribosomal and transfer RNA, and two-thirds of the base composition of messenger RNA will directly reflect the amino acids which are coded. The remaining one-third, representing the third base in each coding triplet, can be all G + C or all A + U, or in any intermediate proportion, depending on whether the organism uses one or the other type of synonymous coding triplet for the amino acids. It would appear that organisms typified by *Micrococcus lysodeikticus* and the *Streptomyces* genus have evolved toward the use of coding triplets ending in G or C since their DNA contains around 75% G + C. In contrast, many of the *Clostridia* have DNA containing about 32% G + C, which implies that they have evolved toward the use of coding triplets ending in A or U. A hypothetical illustration of this is as follows: Let the following short polypeptide represent a sample of bacterial protein:

ala gly val thr ser leu gly lys ilu ala

This may be coded as follows

GCCGGCGTGACCTCCCTCGGGAAGATCGCC

with a base composition of 73% G + C, or as follows

GCTGGTGTTACTTCTCTTGGTAAAATTGCA

with a base composition of about 40% G + C. Now suppose the peptide is changed by 4 single-base mutations to a composition of

val gly val thr ser phe ser lys ilu thr

This may be coded as follows

GTTGGTGTTACTTCTTTTAGTAAAATTACA

with a composition of only 27% of G + C.

There are 8 "quartets" of coding triplets represented by the general

notation XYd, each of which is assigned to a single amino acid (Table 2). It has been speculated that these are the vestigial remnants of a primitive code which consisted of 16 such "quartets" (Table 4) and coded for only 15 amino acids and a "gap" [23]. The third base conferred no specificity in this code, and this is still the case with the 8 "primitive" codes (Table 4) for thr, pro, arg, leu, ala, gly, val and ser. It is further speculated that evolutionary changes brought about the introduction of codes for "newer" amino acids: asN, met, glN, tyr and try, which were then incorporated into proteins by the coding mechanism. These 5 amino acids are known to be formed biosynthetically from asp, cys, glu, phe and ser, respectively. It became necessary to expand the code to in-

TABLE 4

POSTULATED EVOLUTIONARY CHANGES IN THE AMINO ACID CODE

(d = A, C, G, U; b = C, U; e = A, G)

"Primitive" Codes		*"Recent" Codes*		*"Primitive" Codes*		*"Recent" Codes*	
AAd	lys	→AAb	asN	GAd	asp(?)	→GAb	asp
		→AAe	lys			→GAe	glu
ACd	thr			GCd	ala		
AGd	arg(?)	→AGb	ser	GGd	gly		
		→AGe	arg	GUd	val		
AUd	ilu	→AUb	ilu	UAd	gap(?)	→UAb	tyr
		→AUA	ilu			→UAe	gap
		→AUG	met	UCd	ser		
CAd	his	→CAb	his	UGd	cys	→UGb	cys
		→CAe	gln			→UGe	try
CCd	pro			UUd	phe	→UUb	phe
CGd	arg					→UUe	leu
CUd	leu						

clude the "newer" amino acids. This was done by increasing the specificity of the coding triplets so that a differentiation could be made between a purine (pu) or a pyrimidine (py) in the "third" position; for example, AApu is the code for lysine and AApy for asparagine. This infers that lysine "lost" 2 of its codes. Such an increase in specificity could be accomplished by modifying the amino-acid recognition sites in the appropriate sRNA molecules so that the "new" amino acids were now received by sRNA's from their respective activating enzymes. The "charged" sRNA's then delivered the new amino acids to the polypeptide chains during protein synthesis. During these changes, it is postulated that two "new" codes were acquired by leu from phe and two by either

ser or arg from arg or ser (Table 4). The "quartet" represented by GAd became divided between asp and glu; these 2 amino acids are quite similar in their properties, and perhaps the "primitive" proteins used them interchangeably so that GAd meant "either asp or glu." The case of met is of interest; so far only one code, AUG, has been assigned to it, and the triplet AUA is inferred as belonging to ilu by examining mutations although direct evidence for this assignment is not yet available. Perhaps methionine is the "newest" of the amino acids used in protein synthesis. Similarly, it seems definite that tryptophan is coded by UGG, but it is less certain as to whether it is also coded by UGA.

The new knowledge of the genetic code opens the door to fresh interpretations of the facts of genetics and evolution. The task of determining the sequence of bases in a known gene is beyond the scope of today's laboratory techniques, but intensive studies are being made of the amino acid sequences in the polypeptide chains of proteins. The results of such studies may be compared with the coding assignments for the amino acids. These comparisons will enable further conclusions to be drawn regarding the mutational and evolutionary changes that have taken place in DNA.

The complex mechanism by which the genetic message is translated into proteins through a sequential train of biochemical events is obviously a "one-way" process; one cannot imagine amino acids detaching themselves from a protein, combining with sRNA, and lining up in order to synthesize a messenger strand which would return to the nucleus to make a new gene. In the irreversibility of the coding procedure lies a key to the non-inheritability of acquired characters which has so long been a source of frustration to those who wish to transmit somatic changes into succeeding generations, and to plant breeders who would grow bananas in Moscow. But, were there once protein molecules of "non-living" origin that gave rise, by some process long since extinct, to an ordered sequence of bases in a nucleic acid which then was replicated? Such an event could perhaps have led to the selection and perpetuation of the first genetic information.

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AMERICAN SCIENTIST, 53, 1965

SIZE AND CYCLE — AN ESSAY ON THE STRUCTURE OF BIOLOGY*

By JOHN TYLER BONNER

For some years I have been concerned about the problem of the relation of genetics, evolution, and development. It has always seemed that their conventional relation is to some extent static and contrived, while in fact they must be closely integrated and part of one scheme. A problem with so many interrelated facets can be looked at a number of different ways, and here one will be chosen which it is hoped will help to reveal some of the fundamental relations. The method holds no dispute with any of the basic tenets of modern biology; this is not an attack on either the facts or the theories of biology today. Rather it is a regrouping of those facts and theories in such a fashion that new and deeper insights may possibly be achieved.

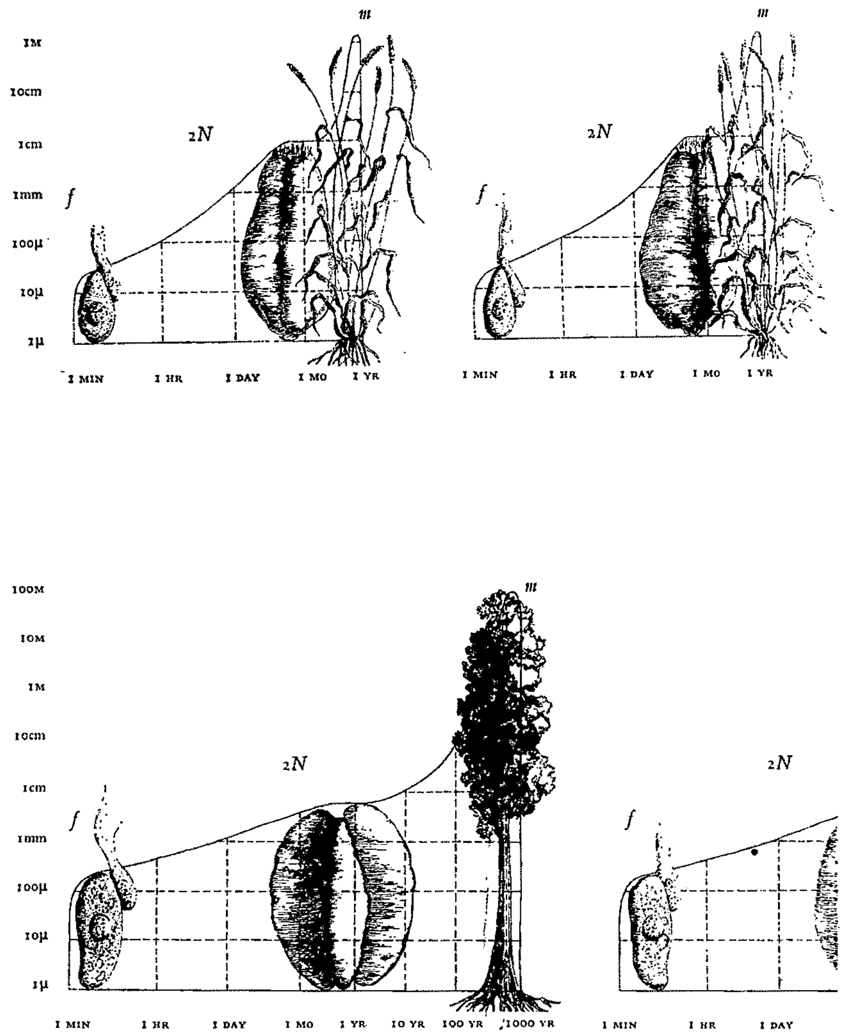
Part of our present difficulty is that so many of our ways of looking at biology have slowly grown out of the past that we are infiltrated and imprisoned by a massive tangle of traditions and conventions. What we teach today is part biology and part history. There is nothing wrong with this but we do not always know where one begins and the other ends.

The view taken here is that the life cycle is the central unit in biology. The notion of the organism is used in this sense, rather than that of an individual at a moment in time, such as the adult at maturity. Evolution then becomes the alteration of life cycles through time; genetics the inheritance mechanisms between cycles, and development all the changes in structure that take place during one life cycle.

When looked at this way the size of the organism in the cycle takes on a particular significance. Size is correlated with time, for in general large organisms have long cycles and furthermore the different parts of the cycle can be readily classified on the basis of their size characteristics.

The life cycle is a summation of all the molecular or biochemical steps, one following another in a well-ordered sequence. The difference between two cycles is a difference in the nature of the steps. They differ in their structure, their composition, and therefore the life cycle is a qualitative unit. A change in size of the organism, on the other hand, does not necessarily require that there be a qualitative difference in the steps, but merely more steps. Therefore the life cycle is quality, and size is quantity. One is a statement of composition of matter and the changes in that composition; the other is merely a statement of quantity. Just as in

* This article is the introductory chapter of an essay on the structure of biology in a book entitled *Size and Cycle*, published in September 1965 © Princeton University Press. We are indebted to the author and the publisher for permission to reprint this chapter of the book and the accompanying illustrations of the range of size in two life cycles, that of wheat and that of the *Sequoia gigantea*. The artist is Patricia Collins.



Life cycles of wheat (above) and the giant sequoia (below). These are drawn on a logarithmic scale so that the sizes of the organisms at different points in their life cycles may be compared. f, fertilization; m, meiosis; 2N, diploidy. From original etchings by Patricia Collins.

chemistry it is vital to know for any reaction the nature of the ingredients as well as their amount, in biology we are concerned with size and cycle.

In this book biology will be examined within this frame, and the question may fairly be asked what are we to gain from this approach? What will we have that we do not have at the moment?

I hope first that we will see more clearly what are the prime questions to be answered by the experimental biologist so that the mechanism of the forward progression of the life cycle may ultimately be understood. Secondly, I hope that we will see more clearly what are the prime questions to be answered by the environmental or evolutionary biologist so that the mechanism of evolutionary progression and ecological success may ultimately be understood. Finally, and most important of all, I hope that it will be possible to show that these two great areas of future inquiry and research are not separate, but firmly woven together into one large fabric; biology is not two disciplines, but one.

It may be helpful to examine in more detail how the view taken here contrasts with the traditional one. Even though the same thing is merely being said a different way, the difference is important and should be understood clearly.

In standard biology the classification of organisms is based primarily on the following four criteria:

1. In the first place animals and plants are put into groups on the basis of their structure or morphology. The principal criterion for groupings such as species, genera, and all the larger, more inclusive groups is structure. This is particularly obvious in the case of asexual organisms, but in all cases it is appreciated that differences in structure have a genetic basis and that differences between species must be thought of as differences in gene pools. But the objects that are identified and classified are the construction and the shape of the parts of an organism. This is the basic currency of modern biology, though it is understood that this currency has genetic and evolutionary significances that are not always stressed, just as when one mentions a sum of money one need not mention where it came from or how it will be used. The objects of biology organisms are identified and classified on the basis of morphology.

2. An organism is traditionally identified as an adult. Many authors have pointed out this curious fact. Although some organisms are classified at least partially on their larval characteristics, by and large the object that is classified on the basis of its structure is the adult. As de Beer (1958) and others have pointed out, this has been a matter of necessity in paleontological studies, because usually it is the adult alone that is preserved, but the tradition is far more deep rooted than this. From the very earliest times, from Aristotle, from Linnaeus, and many others we find the structure of the organism automatically equated with the structure of the adult. I would suspect, for instance, that the average

reader would assume that in the preceding paragraph I was referring to adults only, even though I do not explicitly say so. This is so firmly engrained in our way of thinking that it can only be consciously repressed. Perhaps it has something to do with the fact that we ourselves are adults, and it is difficult not to be subconsciously self-centered. Perhaps it is merely a matter of convenience; it is in fact the most practical solution to the problem. Whatever is the reason, it is a deeply set tradition.

3. Since Darwin and Mendel it has been recognized that variation is essential for evolution and natural selection, and furthermore it is fully appreciated that sexuality is a prime means of providing variation. As a result, sexuality has been raised on a remarkably high pedestal and is generally assumed to be the only source of variation (even though many biologists know and have argued otherwise); sexuality has become a prime basis for the classification of organisms. This has occurred in two ways. One is rather artificial in that the sexual parts of an organism, such as the construction of the flower, are often used as systematic criteria. The other is that in sexual organisms the inability to interbreed is considered the ideal criterion for the identification of two populations as separate species. Again, if we turn to explanations, it is hard to see why sexuality has become so conspicuous in our system of classification, although this is a problem for which Freud might provide a ready answer.

4. Finally, besides being structural, adult-centered, and sexually oriented, our traditional system of classification is basically concerned with phylogeny. Again this is not a new point, and to counter it many biologists, especially ecologists, have been interested in convergence and in functional adaptations to a particular environment. But on the whole blood relations have been far more important than structural-functional similarities. For instance the fact that the wing of a bat is related to the forelimb of a quadruped through descent is considered of greater significance than the fact that the butterfly, the bird, and the bat can all fly with quite different structures which have had different evolutionary histories. Homologies are the basic stuff of biological classification; analogies are intriguing curiosities. In this notion, more than any other, we see our strong desire to bind history and biology. Evolution can only be thought of as a process in abstraction; our first desire is to see the phylogenetic tree all neatly laid out so that the begats go back to the first moment of life on earth.

This is partly because natural selection, and our whole conception of evolution remains the most encompassing and the most useful theoretical frame that exists in biology. All the four aspects of classification that we have discussed are held in place by the firm grip of natural selection. Structure is the object of selection; the adults are adapted, by selection, to a particular environment; sexuality is the means of variation so necessary for selection as well as an isolating barrier in the formation of

species; phylogeny is the life tree that is shaped by natural selection.

In modifying these four points, as I shall now do for the proposed alternative method of organizing biology, it must be emphasized that I have no quarrel with the significance of natural selection, nor with its grip on any scheme of classification or organization. My point will be simply that the traditional view is too restricted, and that a broader one will more accurately reflect the true state of affairs. But by taking this broader view something is lost as well as gained. The old system is illogical partly because it is useful, and what the new system gains in inner consistency it loses on the practical side. It is far easier to classify adults rather than life cycles. Adults can be skinned and dried, stuffed in drawers with neat labels attached; but storing a life cycle is a relatively impossible task. Even the whole approach to the problem of the identification of species is practical rather than logical, for as Mayr (1963) points out, in sexual forms one uses the criterion of the lack of interbreeding, while in asexual forms one uses morphological criteria. Nevertheless, though it may be impractical and cumbersome in the field and in the laboratory, a broader perspective may in fact be simpler and may shed more light at the source of all the problems. Let us now reconsider each of the four points mentioned above.

1. Along with morphology or structure, let us add size. As has already been mentioned, and will be stressed in more detail later, structure is quality, while size is quantity. But in a flash, the sensible, practical biologist will say what nonsense, for how can one classify animals on the basis of size? In any one genus, for instance, one may find an extremely large range of sizes and therefore this could hardly be a useful criterion to distinguish genera. One can only agree, and I would not ask the systematist to use size any more than he now does. But this is not the point here, for I do not wish to use size to characterize taxonomic groups but rather to characterize particular life cycles. Size may be an important difference between two species in one genus and have consequences which permeate into its ecology, its reproductive activities, its evolutionary progress, its development, its physiological activities. In fact size is as important as morphology, or to return to our original words, quantity can have as much significance as quality.

2. Not only the adult, but the whole life cycle will be considered the organism. This is an ancient notion, for philosophers have often pointed out that an *individual* conventionally means an organism in a short instant of time—in a brief time-slice. For example, if we refer to a "dog" we usually picture in our minds an adult dog momentarily immobilized in time as though by a photographic snapshot. The philosopher is quick to point out the shallowness of this convention, for is the dog not a dog from the moment of the fertilization of its egg, through embryonic and foetal development, through birth and puppyhood, through adolescence

and sexual maturity, and finally through senescence? As a matter of fact even the decaying carcass of a dead dog is still a dog, although we imagine ourselves clear in our minds as to the difference between the alive and the dead. Therefore we can say that in a more general sense an individual live dog may be defined as that which fits in the whole span between fertilization and death, although later we shall consider the problem of a precise definition of a life cycle.

Stress on the whole life cycle has already been provided by the developmental geneticist, for he has seen the great significance of the fact that genes do not express themselves just in the final stages of the formation of the adult but all during the course of development. Furthermore it is well recognized that certain genes do not act until senescence, and therefore the thought that genetic effects appear during any stage of the life cycle is quite accepted. The ecologist also has been aware of the significance of the different stages of the life cycle. But here we are making the even more radical suggestion that for all considerations the life cycle as a whole is pivotal.

3. Variation control can be achieved in a number of other ways besides sexuality, a matter which will be considered in detail later. Though this is true, it must be admitted that sexuality is by far the most important method. Since we are not concerned here with traditional taxonomy and species identification, its significance there is outside the present discussion.

Sexuality frequently plays a key role in the discussion of life cycles, but again it will not be emphasized here. For instance the position of meiosis in the life cycle will determine what part of the cycle is haplophase and what part diplophase, but whether the genome is represented once or twice (or more) seems to have remarkably little effect upon the construction of the organism itself. For instance, by experiment it is possible in the moss to prevent fertilization and produce haploid sporophytes without any significant morphological change. And again the ploidy can be changed by experiment in many plants and animals without radical structural modifications. Because of these facts, and because asexual organisms also have life cycles, the number of chromosomes in any part of a cycle is unimportant as far as the progression of the cycle itself is concerned. As will be shown, its significance lies rather in the control of the amount of variation; it is one of many factors which contribute to this control.

4. It has already been stressed that here we are not interested in phylogeny or practical systematics, and that the system of classification to be used is based on the life cycle of organisms. Therefore analogy rather than homology, and convergence rather than ancestral ties, will be emphasized. The characteristics of a particular cycle are significant, rather than how they arrived through history. This is not to say that the

historical information is neither interesting nor important; it is simply not of concern here. We do not care whether a particular functional adaptation is convergent or phylogenetically related; rather the nature of the functional adaptation will be examined. It is therefore not so much a question of how it got there, but what it is.

The only thing that will not change in our comparison of the two approaches to the organization of biology is the role and significance of natural selection. As before, everything lies within its firm grip, but now instead of saying that merely structure is the object of selection, we add that quantity or size is also. Before we said that adults were adapted, and now we add that all the other stages of the life cycle are adapted too. Before we said that sexuality was the means of producing variation for selection, and now we say there are other lesser means as well. Before we said that phylogeny was at the center of all systematic considerations, but in our new systematics the immediate existence of a life cycle, with its functional adaptation to its surroundings and its corresponding structural changes, is of prime importance.

So far, all I have done is to point out some of the main features of the approach to be used here and contrast them with traditional ones. Now we must delve more deeply into the approach and examine the method in detail.

AMERICAN SCIENTIST, *53*, 1965

A STRATEGY FOR MARS

By CHARLES RICHARD WESTON

Each month a few more rockets poise briefly alongside their gantries before leaping into space. Even with the launching of the first modern rocket it was clear that man someday would send his instruments and ultimately himself into space and to the planets. Goaded now by our innate curiosity, our sense of adventure, and our politically motivated desire to demonstrate United States technological superiority, our rockets have begun plumbing the void and sounding the surface of our planetary neighbors. Granting the capability for and inevitability of planetary exploration, the biologist should be asking how biology might be represented in these missions; what experiments could be done, and how they should be implemented.

Thus far, biology in space has fallen into three distinct areas. One is relevant to the problem of putting men in space. In order to guarantee that astronauts will be able to function properly in space and return safely, a considerable amount of research is necessary into the effects of acceleration and vibration during launch, and into the influence of weightless conditions while in orbit. Broadly this can be characterized as human physiology. A second area of study with a more general biological scope could be termed environmental biology; it refers to phenomena which can be studied exclusively in space, in particular, the biological effects of cosmic radiation and weightlessness. Since the evolution and adaptation of all organisms on the surface of the Earth have taken place in a gravitational field, it is reasonable to expect that normal functioning and development—in some cases at least—depend upon the presence of this gravitational field. In fact, such cases already are known, e.g., the orientation of plants with respect to the gravitational field and the response of renal and coronary activity to hydrostatic pressure developed in the circulatory system as a result of gravity. We can expect more to appear with further study. The third area of biological interest concerns what we can learn about the origin and evolution of life by studying the rest of our solar system. Logically, the entire universe would be included in this study, but our rocketry and communication technology restricts us in the foreseeable future to nearby planets and interplanetary space whence we can receive telemetered results in reasonable lengths of time. After careful consideration of the known environmental conditions of the planets, only Mars appears as a serious contender as an abode of life.

The time is approaching, no doubt in the coming decade, when spacecraft will land on Mars. To the biologist this has a special importance, for it will allow him to test some assumptions about the origin and evolution of life which cannot be tested on the Earth now that living organisms have transformed its surface. Moreover, the demonstration of life would

add a new dimension to biological thinking.

The remainder of the discussion will be concerned almost exclusively with this third aspect of space biology. I intend to examine briefly the potential of Mars to contribute significantly to the advance of fundamental biological inquiry, to consider some of the pros and cons in the controversy over the possibility of Martian life, but especially, I would like to dwell upon the special problems to be faced in preparing a program of remote exploration of an unknown planetary surface for organisms of unknown ilk by means of automatic instrumentation.

Biological Interest of Mars

Let me begin by assuming that life does not exist on Mars. What, we must ask, would be the significance of such a discovery? What biological experiments could we do on a world in which life is conceivable but does not exist? An answer to these questions requires that we review briefly the theory of life's origin: It is believed that, during the early evolution of the Earth's crust and atmosphere, chemical and physical changes took place which resulted in the abiological formation of organic compounds. Indeed, in the last fifteen years experimental evidence has been adduced to demonstrate just how, in the primitive Earth's atmosphere, organic compounds could have been produced abiologically (see Miller 1955, Oró 1960, Ponnamperuma, *et al.* 1963). Prior to the emergence of life, these compounds would have accumulated in the seas to form a "dilute soup"—raw materials for a biochemistry. From such a mixture life must have arisen; at some point these chemicals were joined in a complex structure having the ability to metabolize and to reproduce itself. Thereafter, the history of life became that of organic evolution and diversification of species as elaborated by Darwin and his successors.

The largest gap in our theories about the origin of life is that step between an accumulation of complex organic compounds and the first reproducing organisms. On Earth, all evidence which may have existed on the early chemical accumulation has disappeared, as these compounds have long since succumbed to the voracious metabolism of the ubiquitous microbes. If any such organic compounds are being produced today, they immediately would suffer the same fate. Thus, we cannot on Earth study the preconditions of life. However, we could learn a great deal on a planet with a history similar to Earth's but which never passed beyond the prebiological state. The study of a virgin planet would afford a unique opportunity to learn about the early stages of the evolution of life. Mars, if it is lifeless, may afford just such an opportunity, a chance to study the chemical evolution of a planet before it is transformed by living creatures.

The more exciting prospect, of course, is that Mars may have its own indigenous life. If this is the case, we will have entirely independent

reference points upon which to build our theories and speculations. I can think of no issue more challenging to the philosophical position of men than the possibility that life can evolve throughout the universe given the proper physical and chemical milieu. In addition, the purely scientific problems are fundamental to biological thought. Some biologists have immediately seen that basic tenets of biology could be put to test if an indigenous flora and fauna existed on other planets, e.g., the universality of the protein-nucleic acid chemistry of life, or the ubiquity of sexual mechanisms. We already know innumerable exceptions to the generalized mitotic and meiotic cycle: Might one of these be ascendant on Mars? The added perspective of an alien world would be invaluable in checking the theories which we have proposed to explicate life and its working. We may hope to learn to what extent the observed patterns of life around which we build our theories are merely accidental and apply only to Earth, or to what extent they are universal and can be extended to other planets. Note that the discovery of Martian life would be only the first step. In fact, if no more were learned than its presence, it would be an interesting but essentially trivial discovery, since such knowledge alone would provide no basis for generalization. It would, however, open the way to the really challenging job of retracing on Mars the early steps of biological exploration on Earth, but with the added possibility of an interplanetary comparative biology. The study of a Martian biota would have all the zest of the exploration of a newly revealed continent, with a special intrigue resulting from the novelty of the Martian environment.

Life on Mars

The likelihood of detecting life on Mars is compounded of the probabilities that there is an indigenous life, and that, being there, we can detect it. Let me begin by making clear my belief that no amount of speculation or Earth-based observation can resolve the first issue. The most we can accomplish on Earth is to exclude the possibility that life in any of the forms *we know* could survive on Mars. Certainly, when a major objective in going to Mars is to determine to what extent life may have developed on Mars differently from on Earth, it is illogical to conclude *a priori* that an environment that is marginal or unduly hostile to terrestrial forms is necessarily unfit for organisms which have evolved under the selective pressure of this alien environment. Nonetheless, a number of people upon viewing the Martian environment have concluded that it is incompatible with life. In considering their objections I would like to apply the following axiom to decide whether a particular environmental parameter on Mars can be deemed incompatible with life: Where *any* terrestrial form can be shown to survive in a given environment, it must be concluded that natural selection in an alien en-

vironment will have assured the survival of adequately adapted organisms. As a corollary, I suggest that where no terrestrial form has been shown to survive an environmental extreme, the burden of proof rests upon those who maintain that it is, therefore, unsuitable for life. Considering the long history of discovery of organisms with unique adaptive capabilities—especially among microorganisms—there is always the lingering possibility that subsequent investigation will reveal an adaptable form, and thus, only the strongest arguments based on a principle of exclusion can be convincing.

Let us then examine the Martian environment and its suitability for life.

Our best estimate of the surface temperature of Mars indicates that during summer months the temperature will be above freezing for about four hours each day, reaching a peak of perhaps 30°C. Each afternoon the temperature plunges precipitously below zero where it remains for some twenty or so hours. Such conditions are approximated few places on Earth, and nowhere that life abounds. And yet, this diurnal freeze-thaw cycle, which at first appears so particularly disadvantageous for life has been shown experimentally not impossible for life at all. Young, *et al.* (1964) have shown that the terrestrial bacterium *Aerobacter aerogenes* can survive a diurnal freeze-thaw cycle where the medium is thawed for only fifteen minutes out of each day. Applying the principle of demonstrated adaptability, we must conclude that the low Martian temperature is not a strong argument against the presence of life.

Likewise, we can easily dismiss the suggestion that absence of oxygen in its atmosphere must preclude life on Mars. The insistence by some that life demands oxygen is simple ignorance of our own biology. Coming as it does from biologists in some instances, it is particularly unfortunate. As any microbiologist certainly knows, there is a great host of organisms which thrive upon or even require the absence of oxygen. The truth of this is found in wine. Oxygen is the winemakers' bane, for it encourages the production of vinegar by oxygen-loving bacteria. Only in the absence of oxygen does yeast transform grape juice into wine. Even higher plants (e.g., cucumbers) have demonstrated unsuspected tolerance of anaerobic conditions during part of their life cycle (Siegal, *et al.*, 1963). We recognize, of course, that in the absence of an oxygenated atmosphere constraints upon the biochemical reactions are to be expected, but we cannot conclude that life is impossible.

Occasionally, it is suggested that plants and animals, as we know them anyway, could not live under the low atmospheric pressure of Mars. However, many organisms easily tolerate low pressure, with the significant effects of low pressures on living systems generally acting indirectly by modifying the environment. For instance, low pressures necessarily result in lower partial pressures of one or more atmospheric

constituents, and it is principally the partial pressure of a gas which determines its availability for metabolic use.

Moreover, the rarified atmosphere on Mars presents little protection against ionizing and ultraviolet radiation. The Martian surface may be subjected to virtually unattenuated cosmic radiation which would produce by high-energy interactions a peak level of secondary ionized particles in the soil (in contrast to Earth, where the peak production of secondary particles is in the upper atmosphere). However, the ion level predicted (O'Gallagher and Simpson, 1965) is in the order of that recommended for prolonged exposure of industrial workers; obviously not fatal. As on Mars today, terrestrial proto-organisms must have been subjected to ultraviolet radiation greatly in excess of the present terrestrial level, for geological studies indicate absence of an ultraviolet shield of gaseous oxygen and ozone in the primitive Earth atmosphere. If our proto-organisms were heterotrophic, as seems likely, they could have survived in sheltered nooks and crannies where they were not exposed to light. The evolution of absorbing pigments in the superficial layers of an organism would have provided shielding, and thus open irradiated environments to habitation. Perhaps this may have been the original role of pigments which later evolved into a photosynthetic apparatus. Secondarily, the pigments would have been used to capture and utilize the radiant energy which was initially a menace. We can imagine, in any case, where terrestrial organisms in their emergent struggle for survival found refuge from ultraviolet light, Martian organisms would have been equally successful.

More important than the foregoing considerations, low atmospheric pressure means lower vapor pressure, and thus boiling temperature of water. At 10, 25, and 50 mb (suggested Martian pressures) the boiling points are respectively 7, 21, and 33°C. Inasmuch as vital functions of life are confined between the freezing and boiling points of water, the range of vitality will be restricted on Mars.

If it is to proceed at an appreciable rate, the chemistry of life must transpire in a liquid solvent—water for all cases we know. Mariner IV photographs failed to reveal patterns of erosion which might indicate large amounts of water during the age of the present surface. The intensity of cratering has led to conflicting interpretation as to what this age might be, ranging from a few hundred millions of years to nearly the age of the cosmos. Measurements designed to detect atmospheric water spectroscopically have only recently been successful. They have indicated a mere fraction of the water present in our own air. However, the paucity of water in the atmosphere or the absence of large bodies of water on the surface does not necessarily imply that water is not present on the surface. Rather, it is a reflection of the low night temperatures. During the night, excess water above the vapor pressure will crystallize and pre-

cipitate as ice or snow, while in the day there will be only a limited time to re-establish equilibrium between ice and vapor. The soil heated directly by the sun could have liquid water in the interstices of the soil particle surfaces and a water-vapor saturated pore structure during the warmth of the day. This would be an ample environment for microbial growth.

There can be no doubt but that the scarcity of water and its narrow liquid range is the single most damaging argument against Martian biota. Still, only a detailed study of the surface can eliminate the possibility of localized concentrations and show if water was available on a geological (aerological) time scale.

Need for Haste

Biologists who have proposed experiments for Mars are understandably impatient to get a program under way. They have been rebuked on occasion for not being willing to wait until the technological problems of landing larger payloads have been solved. With larger payloads many experiments complementing one another can be run, thus increasing the value of each experiment, while obtaining greater assurance of some successes. The argument is that more reliable, sounder, and simply more experiments can be done if we are patient. The brutal fact is that we cannot wait. We have in practice probably about ten to fifteen opportunities to explore Mars for life as I shall elaborate below. This limitation follows from the necessary condition that a biological investigation must proceed on the assumption that anything living on the surface is indigenous to that surface. This means that, in the course of our investigations, terrestrial organisms must not be introduced. In the last section I tried to support the conclusion that Mars could support life, but my reasoning has another important implication, i.e., terrestrial organisms could survive, and perhaps in some cases reproduce on Mars. Even a very few viable terrestrial microbes reproducing on Mars would interfere with biological studies. If ever they are introduced, or even if there is a suspicion that they might have been introduced, future studies on Martian organisms will be rendered ambiguous or impossible to interpret, since, thereafter, there will always be doubt as to whether any results reflect the activities of Martian or terrestrial life forms. This means that, in the course of exploration, scrupulous attention must be taken to decontaminate any vehicles which will land on the surface.

In view of the present world political situation we have to face two possibilities. One is that we ourselves may contaminate Mars, and the other is that the Russians may contaminate the planet. Faced with the frustrating task of designing electronics capable of sterilization, our own engineers and scientists may ask (are, indeed, already asking) that exceptions be made to the established spacecraft decontamination pro-

cedure (baking the entire spacecraft at 135°C for 24 hours immediately prior to launch). Granting of waivers for alternate decontamination procedures on any items will seriously jeopardize sterility of the capsule. We must also recognize the possibility that before the end of this century there will be spacecraft capable of sending an astronaut crew to Mars. At that time it will very likely be difficult to "stem the tide of progress" and prevent them from landing. What is crucial is that it is impossible to decontaminate a man in such a way as to assure that there are no bacteria or other microorganisms on his surface or in his tissue. Every individual has an intestinal and intracellular flora which cannot be removed by any known method, and we must assume that some organisms will escape from an astronaut's suit into the environment. Therefore, we are faced with a deadline before which we must execute our biological experiments, i.e., the first manned landing.

The prospects of Russian contamination are more difficult to assess, but the indications are not encouraging. The Russians have said that the United States was further along in considering sterilization problems, and that they, the Soviets, were not prepared to discuss the matter. Some of their most noted biologists have expressed scepticism even that sterilization conditions can be met. On their lunar landings they have treated their spacecraft with steam and formaldehyde for decontamination. Since it has been demonstrated that microorganisms can survive inside electronic components (Miller, 1962), and since the sealed inner parts of a spacecraft would not be reached, it is obvious that neither steam nor formaldehyde is adequate to decontaminate a spacecraft. In any case, the Soviet space program is a military program and, as such, the biologists do not have a final say in how it will be run. Hence, we are forced to proceed on the most pessimistic assumption that the Soviets will not be as conscientious about spacecraft sterilization as we have been. In that light, we already have cause for concern over the fate of the Soviet probe Zond II, which was last contacted halfway to Mars on a heading to bring it within 500 miles of the planet.

Let us, though, for a moment assume that all due precautions had been observed in decontaminating both our and Soviet spacecrafts. We still could not have absolute assurance that no single resistant microbial spore would survive. The practical and conceptual difficulties of achieving complete sterilization have been considered elsewhere (Jaffe, 1963). I only wish to make the point that, due to the statistical nature of microbial response to sterilization treatments, there is always a small but finite probability of contamination. The presently stated goal of one chance in 10,000 of a surviving organism seems acceptable, but it must not be taken as absolute sterility.

Clearly we are not in a position to bide our time as long as we cannot feel that the Soviets share our concern over the conservation of the

information which is available on Mars, information which is irreplaceable once the planet has been contaminated. Nor can we remain complacent when we realize that each landing, however well treated to prevent contamination, decreases the faith we can have that the planet is still unpolluted. Furthermore, if one accepts the assumption that once the capability exists, men will be sent to land on Mars, our opportunities to study an uncontaminated planet are distinctly limited. By the end of this century we will have most certainly landed on the red planet. With Earth/Mars conjunctions occurring only once in 25 months, there remain only 16 opportunities between today and the year 2000. To insure reliable results, and perhaps to get any results at all with existing limitations, it will be necessary to take maximum advantage of each and every conjunction as it presents itself.

Some Problems in Experimental Design

One of the most tantalizing possibilities of the biological exploration of Mars is that an independent evolution of Martian organisms would imply evolution on the boundless planets of the universe. Paradoxically, our detection procedures based as they are on terrestrial standards, will not settle this possibility. Their success would even reduce this possibility in favor of the Arrhenian hypothesis of panspermia or Ander's (1963) suggestion that chips expelled by meteorites striking the Earth might carry life to our neighbor planet. For if we find an enzyme which activates an automatic assay, or a "bug" which metabolizes the substrates we tender, we must conclude that these pathways or this enzyme are held in common by a Martian counterpart of our terrestrial model. So we must choose between experiments which would work on Earth but not discriminate between alien biochemistry, or experiments which we cannot demonstrate workable, but which—by working—would demonstrate a difference. There is admittedly no theoretical limit to the experimental pathways which we might try that *wouldn't* work on Earth; but, alas, who shall advocate experiments which won't work!

Although it would take me too far afield to discuss how we might resolve this dilemma, I would like to make a few points. Anyone who has considered the problem must soon reject the notion that morphological distinctions will resolve the question of a distinct origin for Martian denizens. We are too familiar with examples on Earth of the vagaries of evolution to reach such a conclusion. Indeed, I suspect we would be mightily disappointed to be greeted on Mars by the same taxonomic faces; our intuition and our myths demand monsters.

Not so obvious, but true I believe, is that functional similarities will no more establish generic affinities than will dissimilarities prove the converse. The ability to metabolize is shared by men and microbes, but their chemical tactics in demolishing a molecule frequently are com-

pletely disparate, and the ultimate fates of the constituents are equally unrelated. This view can actually be presented as the engineers' black box problem: In an experiment devised to identify input and output we cannot deduce the pathways within the box. For a given input and output we can propose a multiplicity of biochemical pathways.

No doubt the strongest possibility for determining whether a terrestrial or Martian life has a distinct origin is that of finding important differences at the molecular level. One of the oldest suggestions is that, in contrast to terrestrial biochemistry, only D-amino acids might participate in Martian protein synthesis. Another suggestion has been that an alternate sugar substitutes for ribose in nucleic acids. In other words, we might look for distinctions among what we know as universal chemical attributes of life, e.g., porphyrins, purines, pyrimidines, etc., but nonetheless a recognizable biochemistry.

A recurrent suggestion is that Martian biochemistry would be altogether and completely unique in our experience. While I tentatively concede this possibility, I prefer for the moment the more conservative view that the suitability of carbon as a catenating atom and the unique claims of water as a solvent for living systems is not entirely derived in our ignorance and limited perspective, but, indeed, has a more or less universal application.

Negative Results and Their Implications

Let me acknowledge at this juncture, considering the number of assumptions that must be made in designing a life detection experiment, the relatively low probability that any early attempt at life detection will give positive answers. Recognizing as I do this possibility, it is necessary to consider what the significance of these negative answers would be.

Inherent in the mission are two possibilities for false negatives. On the one hand, the instruments may merely fail to operate correctly. As long as we know the cause, such a negative clearly is insignificant. Thoughtfully planned monitoring functions in the instrument can reduce the likelihood of instrumental failures passing unnoticed. The other possibility, of course, is that there may be living organisms on the surface of Mars, but their metabolism, their chemistry, or their ecological habits are such that the conditions which are supplied in trying to detect their presence are not adequate to stimulate their growth or metabolic activity.

Most of the suggested life detection procedures suffer from one conceptual limitation. They are limited by the assumption we must make that we are looking for life which is based on a chemistry similar to that of terrestrial organisms.* In the light of what we know about living organisms and the chemistry of life this assumption seems quite plausible.

* Excepting of course microscopic and other "looking" experiments; but they present a unique set of problems which deserve a more thorough treatment elsewhere.

In fact, no one has seriously proposed alternate assumptions which would warrant continued study. However, it is not inconceivable that some modifications in the chemistry, perhaps ones we would consider minor, would yet influence chemical behavior of a Martian organism enough to invalidate the assumption made in setting up detection experiments.

Naturally, in the event of negative results we would like some clues to guide future experiments. What new media might be used? What temperature or pH? Well-designed experiments would in themselves narrow the possibilities for further attempts, and thus, although they be negative, they need not be without significance. Answers to the questions above require a greater knowledge of the Martian environment. As more environmental information becomes available from Earth-based observations and orbiting and landing missions, we may well get the information necessary to plan affirmative experiments.

Finally, the answers may be negative because Mars, indeed, is lifeless, and negative answers the only possibility. It is not obvious, though, how we can determine whether negative answers imply that the experimental assumptions were false, or that the basic hypothesis of life is nullified and that future experimentation should be concentrated on pre-life studies. I do not know how to approach this problem. I have only my suspicion that, in the absence of a rational resolution, sceptics will find their scepticism confirmed and believers their faith unshaken by negative results.

Payload Selection for Mars Missions

Faced with the possibility of contamination even from carefully treated experiments, and a limited number of opportunities in any case, it seems imperative to get biological experimentation under way immediately. However, how can we justify biological investigation on the first landings when there is barely enough information to plan an experiment, and the resultant expectation of positive results is so low? Ought we not concentrate in early missions on elucidating the environment in preparation for further studies? Faced with a choice between an immediate need to begin the biological studies which are intrinsically long shots and environmental studies which have a high probability of yielding information (but information which may or may not be applicable to subsequent investigations as I shall discuss below), the question resolves itself into one of strategy. In other words, how should the effort be divided, especially on the first mission, between low risk/low yield and high risk/high yield experiments?

At the one extreme, if primary consideration is given the arguments on the importance and urgency of biological studies, the mission could be composed entirely of the biological experiments. This approach would have the advantage of testing a diversity of life detection concepts, and

so increasing the chances of getting positive results from one of them. However, when you consider that the probability of each experiment yielding positive results is extremely low, the chance of negative results remains relatively high, since the sum of several very low probabilities is still a low probability.

Negative results carry with them what might be called a tactical penalty. The biological analysts might conclude that these negative results would not negate the value of the mission, and should not carry the implication that further experimentation can be stopped. Yet to be realistic it must be recognized that, to an outsider who is not aware of the limitations imposed by the assumptions the biologist is forced to make, and who has not been thoroughly informed of the purpose of each experiment, it will seem strange that experiments will be continued and more money sought to continue a series of experiments which have been "unsuccessful." The popular press has already taken the statement that the Mariner flyby pictures do not show any life to the conclusion that there is no life. The press has largely ignored the fact that our satellites at 150 miles do not see life on Earth (Sagan and Murray, 1965), so how could Mariner IV at 7500 miles have seen anything? Misinterpretations of this sort carry the risk that funds for furthering of the biological program in space will be cut off by an early negative. It is easy under these circumstances to appreciate the viewpoint that a positive result of a more trivial question is preferable to the negative results of a particularly significant question.

The immediate scientific penalty is equally high. For, while it may be true that certain experimental possibilities could be excluded by negative results, it is also a fact that no basis would have been established to guide the design of future experiments.

In recognition of the fact that the limited data available at this time about the surface composition of Mars make it virtually impossible to design experiments certain to be compatible with the environmental conditions of Mars, it has been suggested that any early payload of sensors to land on the surface of Mars should be devoted exclusively to determinations of the chemical and physical environment. These measurements would include temperature, soil pH and salinity, ultraviolet flux, air pressure, etc. That is to say, all of the information which we would like to have right now for the design of our experiments. These measurements—so the argument goes—would serve to reduce the number of assumptions necessary in setting up the life detection experiments.

Obviously, this approach takes little or no cognizance of the argument that it is imperative to get biological results while the chance of contamination is at a minimum. However, even if there were no question of contamination, there are still weaknesses in the major argument in favor of initial landings of environmental studies, that is, the argument that

design of biological experiments subsequently could benefit from the greater knowledge of the Martian environment. It seems self-evident that the more we know about an environment, the better we will be able to anticipate the special needs of its inhabitants. And yet, I would offer the following challenge in regard to any suggested life detection concept: What measurements would alter the choice of experimental conditions, substrates, or media? For example, what might we look for in the Martian environment which would then tell us that we must use this, that, or any special chemical in the design of an experiment? Is there even any reason to expect that the range of choices will be narrowed by an environmental analysis? This is certainly not to suggest that we do not wish to make an analysis of the environment. It would seem *a priori* that any measurement has potential for being useful, but it is not clear from our present vantage what measurements would be important. Thus, we must conclude that environmental measurements, although having a high probability of yielding positive results, might yield useless information.

Even if we should get results from an environmental measurement which suggested changes in our experimental procedure, we still would be faced with an awkward decision; measurements made on one landing may not be applicable to subsequent landings. These measurements may not be valid since environmental differences might be great enough from one landing site to the next or one season to the next that one could not be sure that the second-generation experiments were being introduced into a similar environment. Consider the problem turned around: Imagine the perplexity of a Martian trying to deduce the nature of the terrestrial surface having information from successive landings within a hundred-mile circle in central California. Samples might come from alkaline deserts, arid mountain peaks, or littoral forests. Mars probably has a more uniform topography judging from Mariner IV photographs, but the criticism stands. In essence, the criticism is one of sampling technique, that is, not a large enough sample can be taken to assure that a representative environment has been measured.

Unfortunately, even if we learned a significant bit of information and were confident that it represented an adequate sample, it could not be used on the next flight to Mars because of the exigencies of rocket technology. By the time results from a Martian experiment are received, design of the next vehicle will have been frozen. The task of equipping and launching a space probe is so large and requires so many resources that planning must start years in advance of the mission. The goals must be firmly established and the instruments prepared on the order of two years in advance of intended launch; a lead-time which has been lengthening with successive launchings. Since the opportunities to launch to Mars occur at twenty-five month intervals, almost with the launch of one spacecraft it is too late to change the next, whereas relevant data

would be forthcoming only after an 8–9 month gestation in interplanetary space. This means that each set of experiments must be in some degree self-sufficient to assure that information is not lost for lack of supporting measurements.

In light of the foregoing discussion I think it is manifest that exploitation of the biological opportunities on Mars will be best advanced neither by strictly biological nor by strictly environmental studies, but by a program combining the two approaches. In order of priority, we wish first to probe for signs of life or of pre-life evolution, and second to provide supporting measurements of the environment which can either elucidate positive results or guide redesign of biological experiments if the results are negative. However, realization of the first priority should not exclude the second, since supporting measurements are not merely adjuncts but necessary to the biological inquiry. Certainly, a significant fraction of an experimental capsule landing on Mars must be biological probes, but this may be the minor fraction (less than half) with the remainder devoted to environmental "back-ups."

To summarize: Mars presents a unique biological challenge, but one with conceptual uncertainties which are as formidable as the technological difficulties. To maximize our rewards we must utilize every opportunity to land *both* biological experiments and associated environmental sensors on Mars. We must recognize the conceptual limitations in the biological search, but by sending a balanced payload we can be prepared to profit from the mission whatever may have been the vagaries of Martian evolution.

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AMERICAN SCIENTIST, 53, 1965

THE CHEMISTRY OF PROPELLANTS[1]

BY IRVIN GLASSMAN

PROPELLANT chemistry or selection has not changed with the advent of the space age,[2] but the requirements imposed upon chemical propellants have. This paper concerns itself with the question of whether chemical propellants can meet these demands or not and what factors, fundamental and technological, govern the choice of specific propellants.

In these times when so much is said about the needs for ultra high energy propulsion, one must necessarily reflect on the role of chemical propulsion in the space age. Regardless of the popularity of the so-called advanced propulsion schemes, all reasonable evidence to date leads one to conclude that chemical propulsion will be the major energy source for the next decade or two, particularly for the prime movers. Not only are chemical systems the most advanced in terms of the state of the art, but they also possess certain characteristics which are very important for the early exploration of space. Being the most advanced in the engineering sense and from the very nature of their high thrust to weight ratio chemical rockets are the only power plants that presently supply the high thrust levels for the attainment of escape from the earth and minimum time trajectories between satellites and planets. Such characteristics as the production of oxygen and water, auxiliary power, thermal energy, etc., are further attributes of the system.

The question of mininum energy orbits versus minimum time orbits is yet to be resolved. However, when one considers all the possible physiological and psychological hazards of space travel and the benefits of obtaining new data quickly, one tends to pay the higher price of chemical systems in order to obtain the benefits of travel times shorter by almost an order of magnitude.

Support of the basic conclusions reached in the above paragraphs is outside the scope of the material to be presented here. Nevertheless, the ideas put forth do show that the astronautic system will be important, if not the most important, users of chemical propellants in the space age. Later, it will be seen that, for these chemical systems, maximum performance remains a primary requirement.

The other important space age systems which use chemical propellants are obviously those concerned with terrestrial weapons. Here, the primary requirements are maximum simplicity, ease of operation, and low relative cost of mission.

[1] This article as an A.C.S. Lecture was first given in November, 1958, and was written as a Princeton University report in June, 1961. It is up-dated by a series of footnotes.

[2] Sputnik I was launched October 4, 1957.

The cost of a military weapon to accomplish a particular mission and the development program which precedes it is so great that it becomes an important factor in the selection of the power plant and fuel. As a pertinent example, consider the current development of the F-1 engine. This rocket is to deliver one and a half million pounds of thrust. Examining its propellant consumption rate will suffice to make the point regarding cost. A designation of the performance of a rocket engine is its specific impulse, the pounds of thrust delivered per pound of propellants consumed per second. Current operational values of propellants of the type used in the F-1 are about 250 lb sec/lb. Simple division of one and a half million by 250 shows that 6000 pounds of propellant per second are consumed. Three tons of chemicals are consumed per second! Thus, for a three minute operation test, about one million pounds are consumed and the total cost of the propellants alone could range from a million dollars down to one hundred thousand dollars depending upon whether the average cost was one dollar a pound or ten cents a pound.

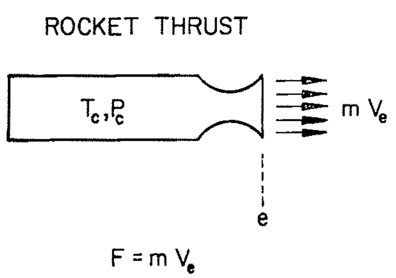


Fig. 1

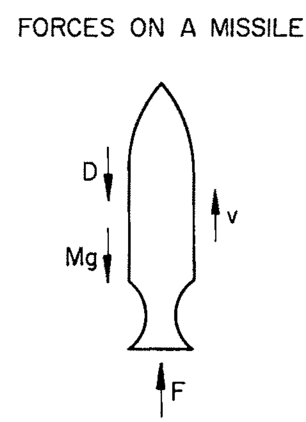


Fig. 2

Obviously, the total cost of a mission must be related to performance as well. If one propellant is cheaper than another, but more is consumed to accomplish the same mission, then the total cost of the cheaper propellant may be greater. Thus it is necessary to discuss the performance parameter to be used throughout this paper, the specific impulse. A simple development from ideal rocket motor theory is given. Although elementary, this development may give greater insight into the discussion to follow.

Consider a simple tubular chamber rocket motor with an axisymmetric converging-diverging nozzle as shown in Figure 1. The thrust F that it develops must be equal to the mv_e of the exhaust, where m is the mass

flow rate of the propellants and v_e is the velocity of the exhaust gases. If the pressure at the exhaust is equal to the ambient pressure then

$$F = mv_e, \quad I_{sp} = \frac{F}{mg} = \frac{v_e}{g} \tag{1}$$

From the application of momentum and energy principle and the assumption that the specific heats, c_p, of the combustion gases remain constant throughout the nozzle expansion process, one obtains

$$\frac{1}{2} v_e^2 = \Delta H = c_p(T_c - T_e) = c_p T_c \left(1 - \frac{T_e}{T_c}\right) \tag{2}$$

where ΔH is the enthalpy change across the nozzle, the subscript c refers to the chamber and the subscript e to the exit of the nozzle. If one assumes isentropic flow, then

$$\left(\frac{T_e}{T_c}\right) = \left(\frac{P_e}{P_c}\right)^{\gamma - 1/\gamma} \tag{3}$$

where γ is the ratio of the specific heats. Further

$$c_p = \frac{\gamma}{\gamma - 1} \frac{R}{MW\, g} \tag{4}$$

MW is the molecular weight and R the universal gas constant. Substituting Equations 2, 3, and 4 into Equation 1 yields

$$I_{sp} = \left\{\frac{2}{g} \frac{\gamma}{\gamma - 1} R \frac{T_c}{MW} \left[1 - \left(\frac{P_e}{P_c}\right)^{\gamma - 1/\gamma}\right]\right\}^{1/2} \tag{5}$$

which makes it readily apparent that the average molecular weight of the combustion products of a propellant system is equally as important as the temperature which that system generates.

In order to find other parameters of importance one must examine the exterior ballistics of the rocket as well. For example, treat the simple case of the vertical ascent of a missile. Figure 2 depicts such a missile and the forces acting upon it. Summing these forces, one obtains

$$F - D - Mg = Ma = M \frac{dv}{dt} \tag{6}$$

To simplify the analysis, it is assumed that the drag D and the mass forces Mg are small compared to F. This case would actually be that of a rocket ascending in a dragless atmosphere far from the surface of the earth. The acceleration a is then

$$a = \frac{dv}{dt} = \frac{F}{M} \tag{7}$$

Since the mass consumption of propellants in a rocket is constant, the mass of a rocket decreases linearly with time; i.e.,

$$M = M_0 \left(1 - \frac{\alpha t}{t_p}\right) \tag{8}$$

M_0 is the initial mass of the complete missile, t_p is the time of powered flight, and α is the ratio of initial mass of propellant to the gross mass of the vehicle. Thus

$$F = \frac{M_p}{t_p} \cdot g \cdot I_{sp} = \frac{\alpha M_0}{t_p} g I_{sp} \tag{9}$$

where M_p is the mass of propellants. To obtain the velocity at the end of powered flight, one integrates Equation 7 after substituting Equations 8 and 9, as follows

$$\int_0^{v_p} dv = \int_0^{t_p} \frac{F}{M} dt = \int_0^{t_p} \frac{\alpha \dfrac{M_0}{t_p} \cdot g\, I_{sp}}{M_0 \left(1 - \dfrac{\alpha t}{t_p}\right)} dt$$

$$= \int_0^{t_p} \frac{g I_{sp}}{\left[1 - \left(\dfrac{\alpha t}{t_p}\right)\right]} d\left(\frac{\alpha t}{t_p}\right)$$

or

$$v_p = -g\, I_{sp} \ln (1 - \alpha) \tag{10}$$

For multistage rockets with the same α, Equation 10 becomes

$$v_p = -ng\, I_{sp} \ln (1 - \alpha) \tag{11}$$

where n is the number of stages.

Inherent in α is the average bulk density of the propellants. The more dense the propellant the greater is the value of α; however, an explicit dependency cannot be evolved since structural considerations influence α as well; but density can be an important factor for a given mission.

Other characteristics required of propellants are that they:

(a) permit reliable systems to be developed. Complex controls required of a specific propellant combination may make that combination unsuitable for a particular mission due to reliability.

(b) be liquids within the military specification range of -60 to 160°F.

(c) have good heat transfer properties for regenerative cooling. A rocket is cooled by passing one of its propellants through the cooling jacket and then into the combustion chamber. Thus, not only are the walls of the rocket motor combustion chamber kept at a sufficiently low temperature that thin structural members may be used, but there are no overall heat losses as well.

(d) be non-toxic. The products of a propellant combination should meet this requirement as well.

(e) be hypergolic. Hypergolicity is the property of spontaneous ignition under ambient conditions.

(f) be non-corrosive. The products should be non-corrosive and non-abrasive also.

The actual various performance requirements for space missions are too numerous to be detailed here, but, as an example, a quick approximate calculation of the escape velocity can be made.

The kinetic energy of a missile must be great enough to overcome the work required by gravity

$$\tfrac{1}{2}Mv^2 = M \int_0^\infty g\,dx \tag{12}$$

Assuming an inverse square law for gravity,

$$\frac{v^2}{2} = \int_0^\infty g_0 \frac{R^2}{(\mathrm{R} + \mathrm{X})^2}\,dx$$

$$\frac{v^2}{2} = g_0 R$$

$$v_{\text{escape}} = \sqrt{2\,g_0 R} \cong 36{,}700 \text{ ft/sec} \tag{13}$$

where R is the radius above the earth and x is the height above the earth. For a low in (circular) orbit satellite ($x \ll \mathrm{R}$)

$$v_{\text{satellite}} \cong \sqrt{g_0 R} \cong 26{,}300 \text{ ft/sec} \tag{14}$$

Some interesting presentations of space requirements have been given by Altman and are shown in Figure 3. The structure factor β is given by

$$\beta = \frac{M_{\text{empty weight}}}{M_{\text{empty weight}} + M_{\text{propellant}}} \tag{15}$$

The weight of the payload is included in the term α which is defined as

$$\alpha = \frac{M_{\text{propellant}}}{M_{\text{total weight}}} \tag{16}$$

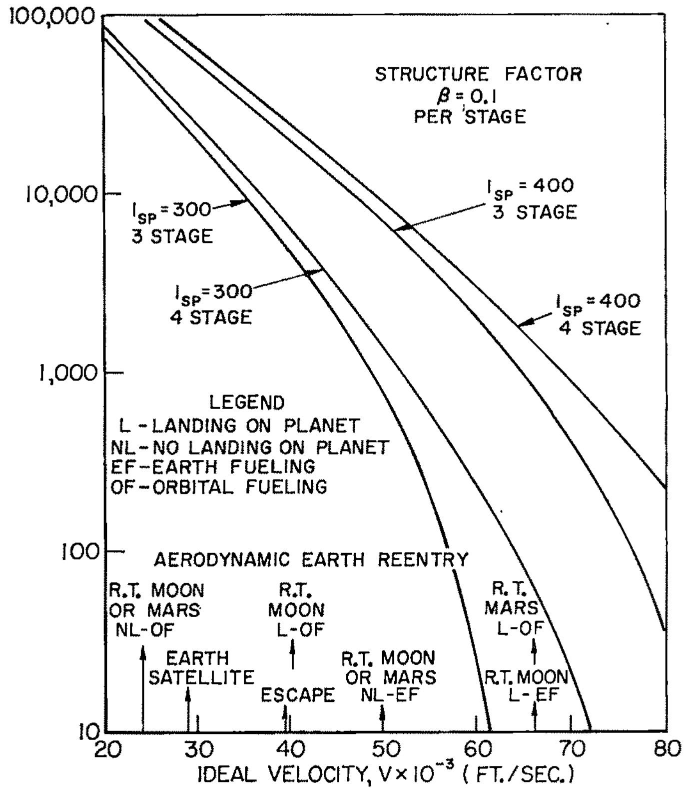
From Figure 3 one can see that important space missions can be accomplished with multistaging rockets having specific impulses of the order of 300–400 lb sec/lb.[3] The question then arises: are specific impulses of this order available and if so in which propellant systems? Equation 5 points out that one wants to maximize T/MW; that is, have energetic systems which have low molecular weight products.

Although many liquid propellant systems have been reduced to standard practice in the United States, four have commanded the greatest attention. They are nitric acid-aniline, acid-hydrocarbon (gasoline, J.P. or R.P. fuel), liquid oxygen-alcohol, and liquid oxygen—R.P. fuel. Perhaps two others will be added to this list shortly, they are N_2O_4-mixed hydrazine and liquid hydrogen-liquid oxygen.[4] Other

[3] Figure 3 represents what can be accomplished with vehicles whose gross weights are of the order of one million pounds. Present day systems weigh much more.

[4] The Gemini booster's are Titan II's whose propellants are N_2O_4 and a 50–50 mixture of hydrazine and unsymmetrical dimethyl hydrazine. The upper stage of the Surveyer (lunar soft-landing spacecraft) is called Centaur which uses a rocket with liquid oxygen and liquid hydrogen as propellants.

combinations used occasionally are hydrogen peroxide-mixed amine fuel, hydrazine hydrate—N_2O_4, white fuming nitric acid (WFNA)-unsymmetrical dimethyl hydrazine (UDMH), ammonia-liquid oxygen, etc. The most widely used combination is liquid oxygen-R.P. fuel. Gasoline is not used because of its expense compared with jet propulsion (J.P.)



USEFUL PAYLOAD WEIGHT CARRIED BY ONE MILLON POUND MISSILE
(ALTMAN, 9TH. I.A.F. CONGRESS PREPRINT)

Fig. 3

fuel, a low grade kerosene cut developed for air-breathing jet engines. R.P. (rocket propulsion) fuel is the same as the J.P. cut except the aromatics are removed to prevent coking in the cooling jacket. Further, R.P. fuel is a narrow-cut petroleum so that there is a smaller density variation from batch to batch. Of the four systems that have been most widely used, oxygen-hydrocarbon is obviously the most energetic. In fact, it is apparent that the oxygen systems will give higher performance than the corresponding acid systems.

At 300 psia chamber pressure and sea level exhaust, the oxygen-R.P. combination has a specific impulse of 264 secs (lb sec/lb), and if the exhaust pressure is lowered to vacuum conditions the impulse approaches 336 seconds. Since liquid oxygen is apparently a good oxidizer, it is natural to seek other fuels to be used with it which will give still higher performance than that mentioned above for the normal hydrocarbon.

There are over a hundred elements in the periodic table and thus the possible fuel combinations become fantastic. However, the basic purpose of specific compounds in propellant systems is to introduce certain elements into the system. The amount of energy released by a given propellant combination is equal to the differences in the heat of formation of the products and reactants; i.e.,

$$Q_p = -\left[\sum_{j,\,\mathrm{prod}} n_j(\Delta H_f{}^0)_j - \sum_{k,\,\mathrm{react}} n_k[(\Delta H_f{}^0)_k\right] \tag{17}$$

where Q_p is the energy release in the chamber, $\Delta H_f{}^0$ is the standard state molar heat of formation at a given reference temperature such as 298K, and n is the number of moles of each j product or k reactant. The heats of formation of the products are large negative values and even the introduction of the most energetic reactants only affects the overall energy release slightly; to state this postulate more correctly, the first term on the right-hand side of Equation 17 is always much greater than the second. Thus, a rapid method to determine the most energetic fuels is to evaluate their heats of combustion. For rocket considerations, one must realize that the heat of combustion of concern is that per unit weight of fuel and oxidizer since the oxidizer must be carried in a rocket system. The normal heat of combustion considered is that per unit weight of fuel.

An interesting plot facilitates comparison of the heats of combustion. If one plots the heat of combustion of the elements in kcal/gm of fuel and oxidizer as a function of atomic number, he will note a certain periodicity. The choice of the atomic number as the abscissa is strictly a convenience. Figure 4 is such a plot based upon liquid oxygen as the oxidizer. It is to be noted as well that for the elements the ordinate is simply the negative of the heat of formation of the corresponding product oxide.[5] Further, compounds can be placed on this figure as well. Of particular interest here will be the hydrides and the hydrocarbons. In general, all such compounds must fall between the elements from which they are constituted. Thus, all hydrocarbon heats of combustion fall between C and H, the various hydrides between the particular parent element and hydrogen, etc.

[5] Or the heat of combustion of the element calculated on the basis of kcal/g of fuel and oxidizer.

Figure 4 immediately predicts that, of the elements with atomic numbers greater than 10, only Mg, Al, and Si and their hydrides would be worth considering. Mg, Al, and Si being metals can be adopted in liquid systems only as slurries. Some of their hydrides or alkylates may be liquids; however, no matter in what form they are introduced into liquid propellant systems their performance will not be higher than hydrogen. The reason is that these metals give values that are not much greater than hydrogen, and form condensed oxides. Condensed particles cannot be expanded through the nozzle and thus an impulse

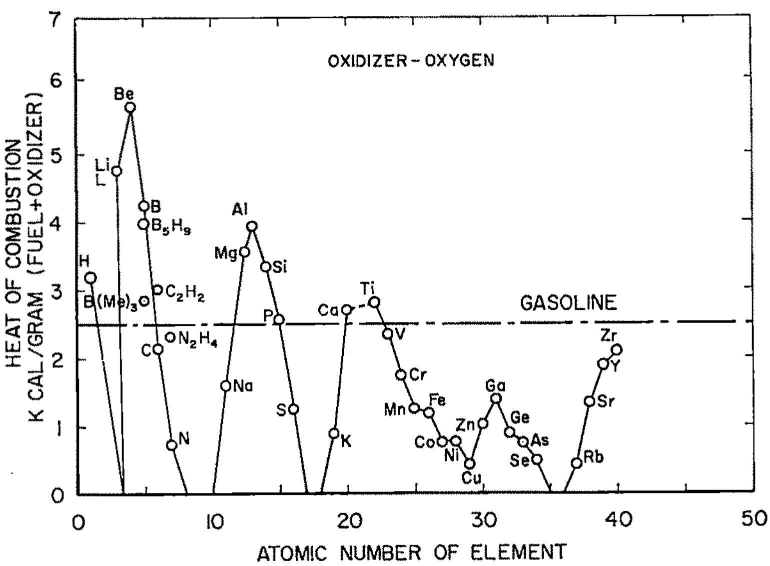


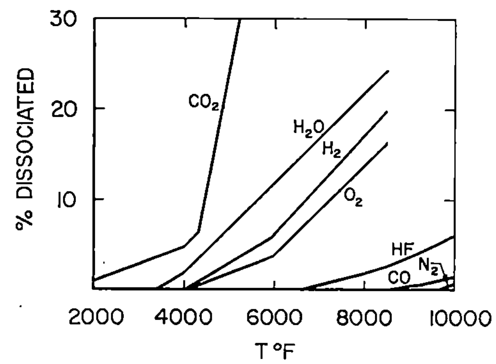
Fig. 4

penalty is paid. Another way to express this point is to state that a gas system containing particles has effectively a high molecular weight.

Thus, it appears for liquid systems that the search is narrowed somewhat to C, B, Li, Be and H compounds. The toxicity of Be and its availability almost preclude it from consideration, *but its unique position on the graph given in Figure 4 will always make it of interest.*[6] Lithium as a metal is so reactive (it is pyrophoric) that the convenient introduction of lithium into liquid or solid systems is difficult. The hydride, of course, could possibly be used in solid propellants. The boron compounds form glassy solids and the accumulation of such solids in nozzles is a serious detriment to the use of boron compounds. In fact, the handling of boron compounds and the slow burning characteristics of the elemental metal make it extremely doubtful that boron

[6] It is of interest to note that, currently, Be is under active investigation.

compounds will ever be used with oxygen systems. Consequently, the screening procedure suggests only CHN compounds, the hydrocarbons, hydrogen, and hydrazine, as the best fuels in oxygen systems. From product molecular weight considerations hydrogen is obviously the best liquid fuel; hydrazine, since it forms no CO_2 or CO, is the next best and the hydrocarbons, the least.



DISSOCIATION AT 500 PSIA

Fig. 5

Not every C, H, O, N compound has been considered as a fuel in Figure 4 and the question naturally arises, for non-cryogenic applications, is it possible to find a fuel much superior to the common hydrocarbons or hydrazine. Essentially, the answer to this question is no, for there is an upper temperature limit for CHON systems. This limit is a result of the onset of several reactions which result in the dissociation or conversion of the primary combustion products into unstable or less energetic species. Some of these reactions along with the energy absorption for the process are given in Table 1. The values listed in this

TABLE 1

HEATS OF DISSOCIATION REACTIONS

	kcal/mole	*kcal/gm*
$H_2 \rightarrow 2H$	103.8	51.9
$H_2O \rightarrow 1/2H + OH$	67.9	3.8
$1/2N_2 + H_2O \rightarrow H_2 + NO$	79.4	1.7
$CO_2 + H_2 \rightarrow CO + H_2O$	9.8	0.2

Typical Heat Releases 1.–1.5 kcal/gm Propellant

table take on greater significance when compared to the typical release values found for the CHON systems of about 1 to 1.5 kcal/gm of propellants. Recall then that even the slightest dissociation of the stable combustion products can have a large effect on the overall energy release.

The amount of dissociation in various systems is examined conveniently in Figure 5. In fact, this figure shows quite readily that carbonaceous fuel systems with oxygen suffer great dissociation and, as the temperature is raised, more and more dissociation takes place. Thus, if a "very energetic" hydrocarbon fuel is found, the temperature will be limited by the great heat-absorbing dissociation reactions. An interesting exception to this CHON case is cyanogen-oxygen in stoichiometric proportions giving CO and N_2, which only dissociate at very high temperatures. Although it is a high temperature system, it is also one of high molecular weight (28) and thus low performance. Most CHON systems have a product molecular weight of about 22 or less.

It becomes evident then that the optimum propellant system with oxygen is hydrogen, which gives a sea level impulse of 390 sec and a vacuum impulse of 455 sec at 1000 psia chamber pressure. It should be recalled that the mixture ratios which give the optimum impulse values quoted above do not correspond to stoichiometric. In all propellant systems, the mixture ratio is adjusted to be hydrogen rich in order to take advantage of the lower molecular weight. The loss in energy release is more than compensated by the decrease in molecular weight. Table 2

TABLE 2

PERFORMANCE OF LIQUID HYDROGEN-LIQUID OXYGEN SYSTEMS AT 300 PSIA

Mixture Ratio (lbs O_2/lb H_2)	9	8	5	3.2	2.5
Flame Temperature (°F)	5750	5800	5300	4500	340
Mean Molecular Weight	17	16	11.5	8.6	7
Specific Impulse (lb secs/lb)	280	295	338	345	340
		STOICH.		MAX. ISP	

gives the comparison between the optimum and stoichiometric values for the H_2-O_2 system. Notice that the impulse optimizes at a weight ratio of 3.5 and a product molecular weight of approximately 9.

Two factors make this system suitable only for space missions: the poor bulk density of hydrogen and the complete cryogenic aspects of the system. Further, although the discussion to this point essentially has been limited to liquid oxygen as the oxidizer, it should not be assumed that this compound is the best oxidizer. In fact, oxidizers can be listed in order of decreasing performance with a given reducing substance containing hydrogen predominately in the following manner

$$O_3,\ F_2,\ F_2O,\ NF_3,\ O_2,\ ClO_3F,\ ClF_3,\ H_2O_2,$$

$$N_2O_4,\ C(NO_3)_4,\ HNO_3,\ BrF_5.$$

Liquid ozone which has a large positive heat of formation still must be proven because of its present sensitivity to explosive decomposition in high concentrations. Its relatively high density is an advantage, but

its performance and bulk density with hydrogen may not make it much superior to fluorine. Fluorine chemistry is worth examining, for there is a possibility that fluorine may be one of the oxidizers used for space travel. It is not likely that it would ever be used for a terrestrial weapon or a first stage, because of the corrosive hydrogen fluoride exhaust. The possibility of having tons per second of hydrogen fluoride pouring over a launching area is not an attractive thought. The hypergolicity properties of F_2 with practically all fuels is an added advantage, particularly under vacuum conditions which exist at high altitudes. Whereas O_2-H_2 has a

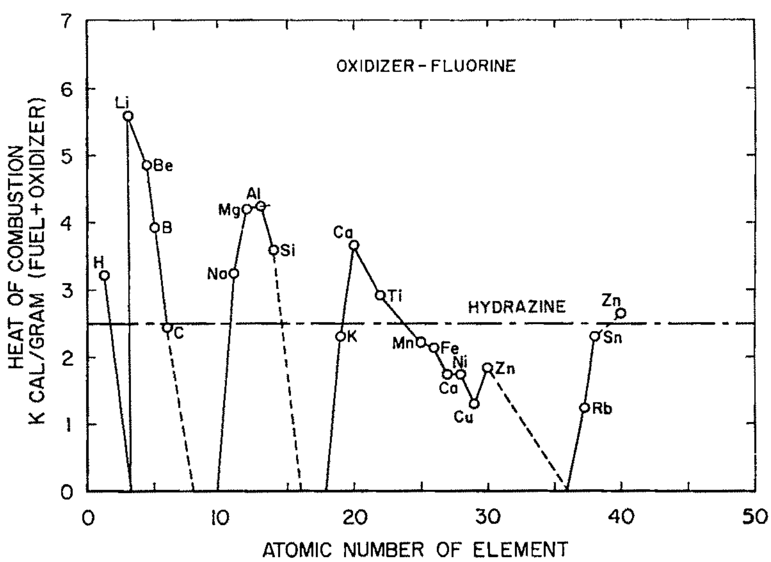


Fig. 6

390 sec impulse, H_2-F_2 has a 410 sec impulse under the same pressure conditions. This difference in performance can be quite significant in range since the range is proportional to the impulse squared.

Figure 6 is a plot of the heat of combustion of the elements with fluorine as the oxidizer. One can carry through the same arguments as those made for the oxygen plot except that it is well to remember that many of the metal fluorides are gaseous under the combustion chamber conditions that prevail. Since BF_3 is a gas, the significance of boron compounds as fuels particularly changes. Comparison of Figures 4 and 6 will show that H_2-O_2 and H_2-F_2 have the same heat release. Yet, as was stated previously, the fluorine impulse was the greater. This comparison emphasizes one of the limitations of the plots, which do not take into account the dissociation at combustion chamber temperatures. Figure 5 reveals why the fluoride system is better then the oxygen system. The product HF is strongly bonded and dissociates only at the

very highest temperatures. If the H_2-F_2 were not run H_2-rich, there would be practically no dissociation in this system at all. As for the O_2 system, the optimum mixture ratio is quite fuel-rich and the product molecular weight is again about 9.

Like the H_2-O_2 system, the H_2-F_2 system is one of low bulk density. To increase the bulk density and to relieve the cryogenic problems associated with hydrogen, hydrazine was proposed as a fuel. Hydrazine has a density and freezing point close to water. It, too, is appropriate for F_2 because it forms stable products; unfortunately, one of them is N_2 which has a high molecular weight and which thus causes a decrease in the performance. Since carbon-containing fuels form the very high molecular weight halocarbon exhaust, they are never considered for pure fluorine oxidizer systems. The question of oxidizers containing more than one oxidizing element will be discussed in detail when solid propellants are taken up. Ammonium perchlorate introduces both Cl and O atoms into the system and serves as the best example for the discussion. In regard to pure fluorine systems again, it is obvious that ammonia would be an appropriate fuel as well.

Not only is hydrazine best with fluorine, it shows better performance than the hydrocarbons with oxygen, peroxide, N_2O_4 or nitric acid. On Figure 4, hydrazine is placed below the gasoline reference line. It is still better than gasoline, of course, because hydrazine contains no carbon and thus no readily dissociated CO_2 can possibly form. Again, it should be recalled that Figure 4 does not take into account dissociation. If corrosive properties and HF exhaust rule out fluorine as an oxidizer, then hydrazine undoubtedly will be used with one of the other oxidizers.

Hydrazine does not meet the important military specification of low freezing point. In order to circumvent this difficulty, unsymmetrical dimethyl hydrazine was suggested as a fuel. It meets the physical property requirements of a military rocket fuel, but, because it contains carbon, it gives a lower performance than hydrazine. Mixtures of the two have been used to obtain suitable physical properties and increased performance over pure UDMH. Both hydrazine and UDMH are hypergolic with N_2O_4 and nitric acid, a decided advantage. Hydrazine is a monopropellant (i.e., decomposes exothermally, in a controlled fashion) and thus possesses certain unique characteristics. The fact that it undergoes controlled exothermic decomposition means it can be stored in self-heating containers to alleviate the freezing point problem. Further, monopropellant gas generators can be operated to drive fuel pumps, supply auxiliary power, etc.

The next oxidizer in the list given previously is F_2O, fluorine monoxide. F_2O, which has a density about that of F_2 and is cryogenic, has been considered because it is not as corrosive as F_2. But the presence of oxygen defeats the purpose of the fluorine oxidizer; water forms, it dissociates.

and this dissociation is reflected in the performance.[7]

Chlorine trifluoride was introduced and considered because of its high boiling point for the halogens (11.8°C); further, it has a high specific gravity (1.7). But it produces high molecular weight exhaust products (HCl) and thus lower performance. In order to alleviate the high molecular product gas, NF_3 was introduced, but it is again cryogenic and of lower density. For volume-limited, storable systems, BrF_5 with a high specific gravity (2.45) and boiling point (40.5°C) is probably the best

TABLE III

OPTIMUM SPECIFIC IMPULSE VALUES

Rocket Oxidizer	*Rocket Fuel*	*Average Bulk Density*	*500 pisa Sea Level*	*500 pisa Vacuum*	*1000 pisa Sea Level*	*1000 pisa Vacuum*	*+°F 500 pisa*	*Molecular Weight*
Hydrogen Peroxide	Gasoline	1.28	248		273		4830	21
Hydrogen Peroxide	Hydrazine	1.24	262	325	288	330	4690	19
Nitric Acid	Gasoline	1.30	240		255		5150	25
Nitric Acid	Aniline	1.39	235		258		5100	21
Nitric Acid	Hydrazine	1.26	255		261		4728	19
Nitrogen Tetroxide	Hydrazine	1.20	263	320		325	4950	19
Oxygen	Alcohol	0.99	259		285		5560	22
Oxygen	Gasoline	0.98	264	335	290	340	5770	22
Oxygen	Hydrazine	1.06	280		308		5370	18
Oxygen	Hydrogen	0.43	364	450	400	455	4500	9
Fluorine	Ammonia	1.16	306	400	337	405	7224	19
Fluorine	Hydrazine	1.30	316		348		7940	19
Fluorine	Hydrogen	0.32	373	465	410	470	5100	9
Current Composite Solid Propellant					250			
Best Projected Solid Propellant					290			

alternative. Due to the formation of HBr and Br_2, the specific impulse with this oxidizer is low.

Perchloryl fluoride (ClO_3F) initially looks most attractive as an oxidizer. First, its performance with most fuels is close to that obtained with liquid oxygen. Second, it can be used with carbon-containing fuels to allow the formation of CO and CO_2 rather than the high molecular weight halocarbons; and lastly, it is much less corrosive than the other interhalogens. However, the critical point of perchloryl fluoride is such that it has a very high coefficient of expansion and tanks require great ullage. Although it can be stored under its own vapor pressure as a liquid, the vapor pressure required is very large.

[7] Systems which contain both carbon and hydrogen show better performance with F_2O than with F_2. The carbon preferentially combines with the oxygen and the hydrogen with the fluorine. See Footnote 9.

The remaining oxidizers in the list are the non-cryogenics, nonhalogen materials. Some of these are what are termed, in the missile industry today, storable propellants—propellants which have high thermal stability and respectable freezing and boiling points. In fact, in order for liquids to compete with solid propellants, a trend towards prepackaged liquid propellant systems has been started. Packaging would eliminate H_2O_2 as a storable because it undergoes slow decomposition. The field then would be narrowed down to N_2O_4 and nitric acid. Tetranitromethane, though dense, has poor thermal stability. The obvious choice is N_2O_4 because of its higher performance. The advantage of prepackaged liquid systems over solid propellants would be thrust control, cut-off capabilities, and regenerative cooling possibilities. It would appear, then, from the above analysis, that the most probable choice for storable propellant systems would be N_2O_4 and a hydrazine fuel. It is interesting to note the recent change of Titan missile power plants to storable propellants; the combination chosen was N_2O_4 with a hydrazine-UDMH mixture.

One can make further conclusions from Table 3 which lists various propellant combinations and their performance results. In this table, the peroxides may be desirable because of their auxiliary uses—power units, supplies of water and oxygen; the acids and N_2O_4 are the storables and meet the terrestrial weapon requirements. The fluorine systems compete with the oxygen for high performance missions; perhaps oxygen will be the first stage and fluorine the oxidizer for the upper stage rockets. As yet there are no systems using fluorine. Comparing the data shown in Table 3 with the requirements given in Figure 1, one readily sees that the fluorine and oxygen combinations readily meet the space travel requirements.

Solid Propellants

The last two entries on Table 3 are solid propellants, so that comparisions can be made.[8] The best projected solid approaches the performance of current operable liquid systems. Current solids have a performance somewhat lower than that of the storable liquids, but they have other advantages that have given them a unique position in the power plant field. Reliability, readiness, ease of handling, weight savings, etc., are the basic merits of the solid propellant systems that led to the phenomenal rise in their usage. These facts emphasize that which was stated earlier, specific impulse cannot be the sole basis for comparision of various systems.

Much of what has been said in regard to liquid propellant philosophy can be carried over to solids; however, certain unique points do arise. Before they are discussed, a brief history of solid propellants will be given. The first solids made in this country were made of asphalt (the

These values are still appropriate.

fuel binder), oil (essentially a plasticizer), and potassium perchlorate (the oxidizer); then came the ballistites, the nitrocellulose-nitroglycerine homogeneous combinations, which gave higher performance. For the composite (heterogeneous) propellants, the solid ammonium salts followed to eliminate the smoke due to KCl in the exhaust of the asphalt propellants. Ammonium perchlorate (NH_4ClO_4), although more expensive than the nitrate, contains more available oxygen, gives faster burning rates, and consequently has been the solid oxidizer most predominantly used. Most other solid oxidizers $NaClO_4$, $Mg(ClO_4)_2$, Li_2ClO_4 are hygroscopic and are thus not suitable for propellant usage.

Early in the development of solid propellant, the asphalt composites were found to have poor physical properties, such as cracking under normal temperature cycling, poor tensile characteristics, etc. They were replaced with the elastomeric polymers which have become the present-day binders. The first of these was Thiokol rubber, a polysulfide rubber, which gives the propellant with the best physical properties. The presence of the sulfur atom in the Thiokol rubber decreases the performance compared to a CHO polymer; thus the most frequently used binders are polyurethane, polybutadiene acrylic acid (PBAA), epoxy resin, etc. The choice of the latter binders is made with regard to physical properties rather than performance.

Most solids are limited in their performance by the amount of solid oxidizer that can be added. The addition of too much solids makes the cured propellant brittle and causes complicated manufacturing difficulties because of the very viscous mix in the uncured state. The blending of the oxidizer in the viscous polymer before curing can be an expensive plant operation.

In order to achieve higher performance, aluminum metal is added to the solid mix. Examination of Figure 4 reveals that this metal is the best choice. Beryllium would be better, but again the toxicity question arises. Li and B are difficult to add as a base metal because of their activity and the reasons outlined in the discussion of liquids. Undoubtedly, the light metal elements offer the greatest promise for increased performance in both solids and liquids. For the solids, the best additive, when NH_4ClO_4 is used as an oxidizer, would be an alloy of Be and Li. This conclusion is drawn from examination of Figure 4 and Figure 7, which is the same type of plot as Figure 4 except the oxidizer is chlorine. From this plot it is seen that Li is the best fuel, whereas Be is the best with oxygen.[9] An alloy in proportion to the Cl and O ratio in NH_4ClO_4 would then be the best. Solids which require high density

[9] The free energies of formation of the metal products are approximately proportional to their heats of formation. The entropy change in two competing systems will be about the same and small compared to the heat of formation even at combustion temperatures. Thus the compounds with the highest heat of formation will form.

impulse (the product of density and the specific impulse) usually add the heavy metals such as Ti or Zr.

Of course, the addition of metal to the propellant imposes a problem with respect to total solids added. In most cases, the amount of oxidizer is reduced. For aluminum addition, one nevertheless obtains an increase in performance because aluminum reduces the water and any CO_2 to aluminum oxide and lower molecular weight products. One then obtains a large energy release and the presence of the solid oxide is compensated by the larger amounts of H_2 present. In order to alleviate the total solids problem, there are great efforts to introduce the energy-

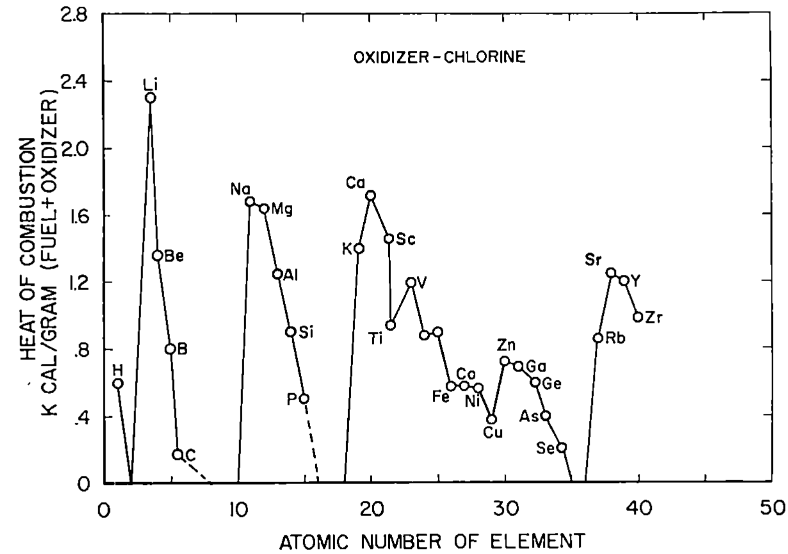


Fig. 7

bearing fuel atom in the polymer or by substituting appropriate oxidizer groups such as nitrates in the pure polymer. Both approaches tend to decrease the amount of solids required.

If the best projected solid becomes a reality and reliability of present day solids are retained, they certainly hold a good chance to replace most liquids for terrestrial weapons, some space vehicle boosters, and satellite applications. One must realize that there will never be a universal propellant combination though. Solids and liquids of various kinds will all have their use in a particular mission.

For example, monopropellants such as ethylene oxide, hydrogen peroxide, etc., will be used for auxilliary power sources to drive pumps, generators, etc. Monopropellants are particularly suited for this type of application because their decomposition temperatures are low and the

exhaust from a gas generator can be passed over turbine blades without difficulty.[10] High energy liquid monopropellants may even compete with the solids for certain missions. First, they can be used as coolants for regenerative cooling and thus can have very long total burning times. Second, the propellants can be shipped independent of the motor case; they have good physical properties, are very dense, can be throttled, programmed, and cut-off on command during an actual firing. When these particular requirements are of prime importance, monopropellants will find a use.

To conclude, then, it is safe to say that, even under the current state of the art, chemical propellants have the performance, reliability and auxiliary advantages that are required to meet the first space age requirements. Even in their infancy the ultra-high-energy power plants necessarily have been designed with a high degree of sophistication never applied to chemical rockets. When Equation 11 is examined, it is evident that performance is a function not only of specific impulse, but also of the number of stages, n, and the mass ratio, α. Only chemical systems have the flexibility for real substantial improvements in n and α. When such sophisticated approaches as hybrids, consummable cases and tanks, etc. are embodied in chemical rocket technology, vastly new potential systems may be realized.

[10] Since the development of a catalyst for the decomposition of liquid hydrazine, hydrazine has become the most widely used monopropellant for auxiliary power, control rockets, etc. It appears to be replacing hydrogen peroxide which is the first monopropellant to have an initiation catalyst.

HERE AND THERE

By the Board of Editors and the Membership of The Sigma Xi-RESA Societies

THE TWOFOLD PRINCIPLE OF FREEDOM AND RESPONSIBILITY*

BY DON K. PRICE

The scientific estate, of course, is merely a subdivision, although perhaps the most influential one, of the broader scholarly estate. Historically, the philosophers and the theologians, being concerned with another variety of pure knowledge, have played an analogous role in society. And the lawyers and the clergy, in taking over their basic ideas and applying them, with modifications, to human purposes, play important roles in the professional estate along with the engineers and the physicians. The difference between the older and the newer wings of each estate is that the Western world has had several centuries more experience in working out a satisfactory constitutional position for the lawyers and the clergy, while the scientists and engineers and physicians have risen so rapidly in influence as a result of the advancement of science that the politicians have not had time to develop a new set of relationships with them.

The Twofold Principle of Freedom and Responsibility

But the United States is clearly working out some crude principles as a basis for these relationships, or adapting some old ones, and these principles may help to establish a new set of checks and balances within the constitutional system. That system will be the subject of the next chapter, but it will be useful here to anticipate a general point. The most important principle seems to be a twofold one: (1) the closer the estate is to the end of the spectrum that is concerned solely with truth, the more it is entitled to freedom and self-government; and (2) the closer it gets to the exercise of power, the less it is permitted to organize itself as a corporate entity, and the more it is required to submit to the test of political responsibility, in the sense of submitting to the ultimate decision of the electorate.

* Reprinted by permission of the Belknap Press of Harvard University Press from *The Scientific Estate* by Don K. Price, published in September, 1965, pp. 136–144, in a chapter entitled, "The Spectrum from Truth to Power."

It is true that in the United States the college or university is not organized in legal form as a self-governing community of scholars; it is either under the legal direction of a private board of lay trustees, or of a board of state officials. But in actuality, the American university, with respect to its academic affairs, is a community to which society concedes the right of self-government; the full members of the academic fellowship —the tenure professors—consider that they are entitled to control the admission of new members, and to protect them in their freedom. Similarly, the government concedes completely the right of scientists and scholars to organize any self-governing societies for scientific purposes that they please.

When it comes to the professions, whose work mixes a commitment of service to the public with its scientific interests, the government is usually content to leave the control of standards to the professional society as a matter of practice, but in theory (and sometimes in practice) it insists on maintaining the right to exercise control.

The administrators, who can rely less on systematic and testable knowledge and are closer to ultimate power, are not permitted to exercise, through their quasi-professional societies, any influence on the terms of their admission to their vocations. They are appointed, on terms regulated by law, by procedures controlled by politicians.

And as for the politicians who exercise ultimate power, but can never be certain that their major decisions are justified by scientific knowledge or provable truth, they are permitted to hold office only by periodic reelection by the people.

This twofold principle about the spectrum between power and truth is not generally accepted in all other parts of the world, even though the practice is reasonably similar in other English-speaking countries and in some countries of Western Europe. The existence of the four estates seems to be common to all countries with advanced science and technology: the respective functions of the estates seem to derive from the way in which any large-scale institution has to be organized in order to translate abstract scientific knowledge into purposeful action. But the mutual independence of the several estates seems to be a quite different matter, that is, one that is not determined by the nature of science and technology (or if you like, by the mode of production) but by the way men think about political power in relation to truth.

For the twofold principle that seems to be accepted in the United States is by no means axiomatic. The idea that an institution concerned only with truth should be permitted to govern itself, and the idea that those who hold political power should be accountable to the people, are plausible only on the basis of certain historic political assumptions. If you think of a nation primarily as an organization that must have a coherent purpose, and think of its inner institutional relationships as a problem in

communications and control, you will come out with a quite different approach. The logic of that approach is quite simple: If we have a reliable method for discovering the truth, namely, science, should we not use it to solve all our most important problems? If we propose to do so, should not those who make our most important decisions be selected for their understanding of the most relevant science? How can such a process of selection be carried on except by the judgment of their peers (as any university department chooses its professors) rather than by a popularity contest among ignorant and apathetic voters? And if they are controlling society on scientific principles, should they not direct scientific institutions to carry forward their research along lines that will be of the greatest service to society?

The logic of these questions forces us to admit that, although the way in which scientific knowledge is related to political purpose seems to require the existence of something like our four estates, it by no means requires the relationship among them that is conventional in the Western constitutional tradition. "The real brains at the bottom," as our science fiction hero put it, is an accurate observation if not a justified complaint. Is it really right that supreme authority should be vested in the Congress and the top political offices in the executive branch, which are filled with men who by any conventional test of abstract and theoretical intelligence are not the match of the scientists and professionals who work several layers down in the hierarchy? Even if the government must make decisions that cannot be determined completely by the rigorous processes of the natural sciences, many new techniques exist for dealing in a scientific way with situations of uncertainty and incomplete knowledge. Since science is producing the dynamic changes in our society, why should not our government be headed by men who will bring to its administrative and political functions the greatest possible ability to use science to the maximum?

The answer to these questions turns not on what you think about the nature of knowledge, but on what you believe about the nature of man and politics.

The Western, and particularly the American, point of view is determined by two traditional fears. The first is the fear that the scientist or the professional will never be guided completely by his desire for scientific truth and his professional ethics; the second is the fear that he will. In logic, these fears are contradictory; politically, they lead to the same end.

As for the first fear, neither the politician not the administrator is prepared to accept any institutional arrangements that depend on the detachment, the unselfishness, and the purely scientific motives of the scientist. His distrust is warranted by what the scientists themselves have told him. For if limits to objective knowledge are set by the nature of things and the imperfections of the instruments that science can use for

observing them, the limits are even more severe when men are serving as those instruments, and making deductions from the observations. For it is not merely that there are finite limits to what man can observe and understand, but that he has a capacity that is nearly infinite for reading the evidence in the light of his own interests and passions.

To say this is not at all to deny that the sciences, within their own fields, provide a remarkably effective training in the virtues of objectivity and honesty. Even more important, they have the world's most effective policing system. Since the main prize in the game is the esteem of one's scientific colleagues for the results of one's research, and that esteem is based on the publication of the results of experiments which can be tested, the scientist has a most powerful incentive for objectivity and honesty. But the system works only when it deals with scientific data and logical patterns—abstractions from reality which can be dealt with uniformly in laboratories and studies all over the world. And to keep the system pure, scientists tend to frown on any colleague who mixes with such data irrelevant considerations like purposes or values that cannot be tested according to uniform standards by everyone else.

When scientists and engineers undertake to apply science to practical problems, however, they find it impossible to stick to quantitative data and rules of logic. The problems include too many other aspects, and in dealing with them they naturally behave like human beings. And so weary cynics like lawyers and political scientists take a certain malicious pleasure in showing that the natural scientists are led by confidence in their special scientific techniques to try to make judgments on political values or policy decisions, without realizing all the other components of a nonscientific nature that go into those decisions. The lesson is a valid and important one, and needs to be driven home to the general public much more than to the scientists, most of whom know it already even if they do not like for nonscientists to remind them of it.

If you feel obliged to document the lesson, the illustrations are plentifully available. Read the hearings published by the Atomic Energy Commission in the case of J. Robert Oppenheimer, and see how two groups of eminent scientists and engineers, differing very little indeed at any given time on specific scientific matters, mistrusted each others' political intentions to such a point that we had the closest thing to a heresy trial that modern American politics has provided. Or read the history of the disputes over nuclear fall-out, or the civil defense shelter program, or the use of insecticides, in each of which both sides used much the same statistics and drew widely different conclusions. Or the story of the Geneva disarmament negotiations with the Soviets, in which the scientists themselves must have found it difficult to tell when they were trying to sort out the technical evidence and when they were making judgments about foreign-policy priorities or estimating the intentions and good faith of the Russians.

You can explain these difficulties in professional and scientific terms, if you like, by saying that the physicists and chemists all ought to realize that issues of this kind need to be worked on by social as well as natural scientists. This is obviously true, although the social scientists are at least as guilty as physicists of mistaking the abstractions of their respective disciplines for the sum total of all wisdom. Or you can add an extra bit of rational explanation by noting that the physical scientist is used to working with data that may be difficult but not malevolent—that is, the data have no will of their own to exercise against the experimenter—whereas in politics the data not only have wills of their own, but sometimes oppose the experimenter not because they dislike what he is doing but because they dislike him. Thus Einstein remarked, as an encouragement to those scientists who seek to find order in the complex universe, that "God is subtle, but He is not malicious." But in an international negotiation on, for example, arms control, no matter how high a scientific content the subject matter may have, the contribution that science can make is a limited one because the Russians may be malicious. In the eyes of the Democrats, so are the Republicans, and vice versa. Maybe in the eyes of the Russians, so are the Americans.

And this type of conflict creates in politics, domestic or international, a problem quite different from those problems on which the scientific observer can take a detached and superior point of view, and ignore the elements of conflict. It sets up a system of relationships on any major political issue in which it is hard to judge the capabilities and intentions of your adversary, and to determine the exact degree to which he means exactly what he says, or is discounting your own reaction in advance, or is deliberately using conflict with you for other irrelevant purposes or out of irrational ill will—and equally hard to judge your own motives.

If we understand that the scientist and the engineer cannot be guided in their political actions entirely by what science teaches them, and that even when they think they are doing so they may be deceiving themselves and others, we are led to a realization that the fruit of the tree of knowledge is not always peace. Maybe this is the reason that, among professional political scientists, we read a great deal more today than we did a generation ago about the dogma—or to moderns, the myth—of original sin. Perhaps the fear that scientists and professionals will not be guided entirely by their scientific knowledge and professional ethics in public affairs, and hence cannot be trusted with political power, is a political attitude derived directly from an old Puritanical prejudice. The Reformers began by observing the immorality of the higher clergy and therefore distrusting hierarchy in an ecclesiastical establishment, and ended by dividing and weakening the role of the church in political affairs. An analogous attack has begun in recent years on the ethics of scientists who use government funds, and on their conflicts of interest.

But the old dogma had a more subtle side to it: it recognized that it was not only the base or material side of man's nature that caused him the greatest trouble, but his spiritual pride. Men are more likely to fight with each other for noble than for base motives. The most powerful personal motive is not the gratification of the senses, or the acquisition of mere wealth or power, but the conviction that one's own skills and knowledge have a special contribution to make to the salvation of humanity, or that they have provided an insight which the rest of the world must be induced to accept. The most tyrannical political systems are those built not on corruption, but on self-righteous fanaticism.

That is why, in regulating the constitutional relationships among the estates of our society, we should be less concerned that the scientists and professionals will yield to material interests or sensual temptations, than that they will be utterly and unselfishly devoted to their respective disciplines or professions. The two types of temptation of course can be related; the more importance society attaches to chemistry, the more income it will let individual chemists earn; the more it is persuaded that doctors can cure its ills, the more likely it is to concede to the American Medical Association the right to prescribe the terms of public insurance for medical care. At any rate, so the chemists and doctors are tempted to believe. But if we tend to think of the problem as one in which the main incentive is material gain, we not only fail to do justice to the motives of the contending parties, but we fail to understand the more difficult part of the problem.

Because of the rapid advance of the sciences, and because of the contribution that they are making continuously to engineering and medicine, there can clearly be no set limit on the contribution they may be asked or permitted to make to our social problems. They do not merely produce things that the public asks for; what they discover determines the range of new possibilities that are open to us. Invention is the mother of necessity, as a British observer recently remarked. Since scientists and mathematicians and engineers are extending their ability to solve problems in which there is a high degree of uncertainty, the administrators and politicians would be stupid not to encourage them to go as far as they can. But the administrators and politicians would be even more stupid if they failed to note the aspects of those problems on which the scientists and professionals are making judgments not required by the nature of their subject matter—judgments that have the effect, and perhaps the purpose, of extending the power of their particular estates.

Kodak reports on:

Charles Bridgman's availability . . . the mating game, audiovisual style. . . . orthographical support for the Harvard team

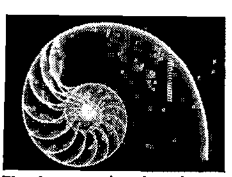
To x-ray rocks and mud

A movement appears under way to get x-ray machines into geological and oceanographic laboratories. We intend to encourage it.

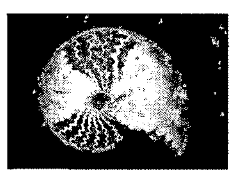
Radiography will earn its way there by opening up the third dimension of specimens to the properly prepared mind's eye. An effective missionary of the movement has been W. K. Hamblin, now of Brigham Young University, Provo, Utah.

Dr. Hamblin first drew attention (*J. Sediment. Petrol. 32*, 201) to how radiographs can unmask sedimentary structure inside rock specimens that present a blankly homogeneous surface aspect. Very quickly thereafter the hint was being taken with informative results for cores of unconsolidated marine sediments (*Sedimentology 1*, 287). Next he pointed out (*American Mineralogist 49*, 17) how a stereoscopic pair of radiographs reinforce conventional microscope technique with an entirely new perspective on ore textures. (X-ray eyes in 3-D make it very easy to know whether fragments are skeletally connected and therefore younger than the matrix, or isolated and therefore older.) Then Dr. Hamblin made the acquaintance of Charles Bridgman.

C. F. Bridgman earns his pay here by enjoying himself. All he has to do is give sound technical advice to those who want to use radiography for other than the routines common in medicine, dentistry, and industry. His working days are filled with wonder, and when he voices complaint it is only to be sociable with those less fortunate. He has been quite helpful to Hamblin. His address is Radiography Markets Division, Eastman Kodak Company, Rochester, N. Y. 14650. Here are two samples of his own work:



The famous chambered nautilus *Nautilus pompilius* from Philippine waters.



Its 80-million-year-old Cretaceous prototype *Placenticeras whitfieldi* from South Dakota.

The chief starts it off crisply

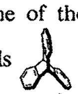
Orders are now being accepted by camera shops and audiovisual dealers for a little device of ours priced at less than $30 that mates any decent stereo tape machine to a KODAK CAROUSEL Projector. It permits a field man to fill more of the happily heavy demand for presentations of his observations without neglect of his obligations at home base.

The chief's voice is heard from the tape. When he makes the recording, every time he pushes the "forward" button of the CAROUSEL Projector a signal is recorded on the unused one of the two stereo audio channels. In playback the beep is never heard but serves merely to change the slide.

Carefully prepared among all the comforts of home, with opportunity for revision, polishing, and editing, his presentation is succinct and finishes in plenty of time to allow a good, stimulating question period. This question period is handled by the junior colleague who brought the tape, the recorder, and the CAROUSEL Slide Tray of 80 slides. (The hosts will probably provide the CAROUSEL Projector. It is becoming sort of standard lecture equipment.) The junior colleague needs the practice of standing up before an audience to defend and promote the line of research. Being junior, he is doubtless more intimately familiar with the details than the chief. With his mentor present only in canned form, he will feel less inhibited.

KODAK CAROUSEL *Sound Synchronizer we designate the little gadget. It works with the* CAROUSEL *700, 800, or AV-900 Projector.*

A hydrocarbon of unnatural shape

The finest one of the freshest crop of organic textbooks calls  "tryptycene." This spelling ignores the suggestion from the Department of Classics at Harvard to the team of Harvard chemists who first prepared the rigid, 3-fold-symmetrical, propeller-like hydrocarbon in 1942. Classical scholars like triptychs better than propellers. We support the Harvard team orthographically and announce *Triptycene* (EASTMAN 9739) at the low, low price of $15.50 for 10 grams, which is about 1/8 what we have seen the compound advertised at a year ago.

The timing of this announcement is interesting. The generation that learned about triptycene in graduate school has now been out long enough to have made a small start toward paying off their mortgages, and their employers are daring to hope they will soon start thinking about practical matters. Here and there chemical thinking in three dimensions is being applied to such matters, whereupon the eminently non-planar triptycene comes to mind.

In medicinal chemistry, for example, studies have been published on the effect of non-planarity in the aromatic blocking group that constitutes one end of many different pharmacological agents. In the absence of evidence that triptycene occurs anywhere in nature, would there be sinister implications if this work should eventually lead to something taken internally to extend human life or comfort? Another school of chemists—those who work on colorants—take an obvious interest in how odd steric configuration might govern the behavior of their traditionally flat molecules toward light and substrates. Whatever excitement they may feel at being propelled in profitable directions by this hydrocarbon propeller, they remain outwardly calm.

Within the short time for this to reach print, we may also be offering derivatives of triptycene with a handle on the bridgehead for fastening to other moieties. Ask the source of all EASTMAN *Organic Chemicals for the laboratory, Distillation Products Industries, Rochester, N. Y. 14603 (Division of Eastman Kodak Company). The catalog of these wares has a new supplement, No. 43-4. We hope all who need it have it.*

Prices subject to change without notice.

This is another advertisement where Eastman Kodak Company probes at random for mutual interests and occasionally a little revenue from those whose work has something to do with science

THE SCIENTISTS' BOOKSHELF

By Hugh Taylor, the Associate Editors, and Guest Reviewers

Sunspots by R. J. Bray & R. E. Loughhead; 303 pages; $13.75; John Wiley & Sons, 1965.

This book provides scientists with an up-to-date account of our knowledge of sunspots. Those portions of the subject which the authors chose to cover thoroughly are excellent. These topics are handled with great care, and the references are in general quite complete. Many readers may be unhappy because the subject of sunspot statistics is treated rather sketchily. Others might prefer a more complete theoretical treatment of the origin and stability of individual sunspots. In general, the authors have treated in greatest detail those areas of the subject in which they have made contributions in the last few years, particularly white-light photography of sunspots, although many other areas have received the same careful treatment. They cover in detail some points which are not directly connected with the subject of sunspots. There is a good treatment of high-resolution observing methods and of white-light studies of photospheric granulation.

The illustrations are carefully chosen, numerous, and excellent. Altogether it is the nicest published collection of sunspot photographs this reviewer has come across.

In summary, this book is to be highly recommended for the professional or the serious amateur as a textbook or a reference work.—*Robert Howard*

Aging & Stabilization of Polymers, edited by M. B. Neiman. Translated from the Russian. 365 pages; $25; Consultants Bureau, 1965.

The values of this book are twofold. Firstly, it reviews the Russian approach to research in this rather specialized field, collecting in one place many of their experimental and theoretical results obtained in the last two decades. Secondly, it forms in itself a useful reference book for those interested in the stabilization of polymers.

The first chapter discusses the mechanism of thermooxidative destruction and stabilization of polymers. This is the weakest point of the book, the size and nature of which probably dictated the sparse coverage. Certainly better reviews of the subject can be found elsewhere. An exception to this criticism is the interesting discussion on the concept of the critical concentration of antioxidants, the theory of which has been developed by the Russian school.

A short chapter on the formation and EPR spectra of stable inhibitor radicals is followed by a chapter on the synthesis of stabilizers for polymer materials. Although little experimental detail is given, the chapter will neuertheless be of great value to those concerned with the field. It gives over 300 references, mostly non-Russian patents.

The book then turns to the aging of specific polymers. It covers fully polyolefins, polyformaldehyde, polyvinyl chloride and copolymers, polyamides, certain condensation polymers, inorganic polymers, and finally raw and cured rubbers. The mechanisms for the thermal, thermooxidative, and light destruction of the above are discussed and, with the exception of the condensation and inorganic polymers, their stabilization.

The layout of the book is good on the whole. However, its usefulness as a reference book is marred by the absence of a subject index.

In the preface, Neiman states that the studies discussed in this book were designed to create a scientific basis for the purposeful selection of effective

stabilizers. This monograph undoubtedly makes a most useful contribution toward reaching this distant goal.—*T. C. P. Lee*

Essays on Crop Plant Evolution, edited by SIR JOSEPH HUTCHINSON; 204 pages; $9.50; Cambridge University Press, 1965.

The time for a comprehensive book on the origin and evolution of crop plants, similar to the 19th century classic of deCandolle and N. I. Vavilov's brilliant monograph published in 1926, is long overdue. The amount of information newly acquired from the fields of archeology, anthropology, and cytogenetics is overwhelming. Many previously mysterious situations have now been cleared up, and methods have been developed by means of which much of our understanding of crop plant evolution can be lifted from the status of plausible speculations to theories based upon sound experimental evidence and clear-cut archeological data.

In the absence of such a volume, the series of eight essays collected by Sir Joseph Hutchinson, and first delivered as lectures at Cambridge University, provides a very fine stop gap for those who wish summary accounts and evaluations of the newer knowledge. Five different crops: maize, sorghum, the temperate cereals, potato, and forage plants are each treated by a specialist on the crop concerned. In addition, the archeological and paleobotanical evidence bearing upon the beginnings of agriculture in northwestern Europe is reviewed by H. Godwin, while Dr. Hutchinson completes the volume with a general discussion of the evolutionary principles involved in the development of crop plants.

As might be expected, the individual essays are somewhat uneven in their degree of comprehensiveness, and in the amount of technical knowledge which the reader needs to understand them. P. C. Mangelsdorf's treatment of maize is a model for essays of this kind.

The discussion of *Sorghum* by H. C. Doggett is largely taxonomic, phytogeographic, and ethnological; most of the interesting information now available on *Sorghum* cytogenetics is omitted. The temperate cereals are given a comprehensive treatment by G. D. H. Bell, after which Ralph Riley presents an excellent review of the cytogenetic evidence bearing on the origin and evolution of wheat. The essay by K. S. Dodds on the potato rightly emphasizes the very great taxonomic problems involved in unravelling the probably complex ancestry of the cultivated clones. It provides good evidence in support of the theory that the European potatoes came from the high Andes, rather than from southern Chile, as some authors have supposed. J. P. Cooper, in discussing the evolution of forage plants, has combined an interesting review of their geographic and climatic distribution with a discussion of adaptational factors involved in the success of the more useful species.

For students of both plant evolution and the basic sciences connected with agriculture and the improvement of crop plants, this little volume is the best that can be obtained on crop plant evolution.—*G. Ledyard Stebbins*

Stratigraphy & Life History by M. KAY & E. H. COLBERT; 736 pages; $9.75; John Wiley & Sons, 1965.

This new book by Kay and Colbert fills a need for an up-to-date and advanced textbook of historical geology.

The nature and increasing quantity of new facts concerning the geologic history of our earth is forcing changes in both teaching and textbooks. This new information is not truly meaningful unless it is accompanied by discussion of the means for obtaining it. Even an attempt to present just the pertinent facts and principles in one semester is becoming increasingly hectic. As a partial solution, many teachers, after a sort of parallel evolution, have arrived at a method in which a unit of factual material is used as a convenient peg from which to hang a discussion of appropriate concepts and techniques. Kay and Colbert, aware of these problems, have used such a method with conspicuous success.

Some principles and theories, not amenable to treatment in association with a single geologic period, are handled in separate chapters such as "The Permanence or Drift of Continents." The special chapter on "The Origin of the Earth" by D. B. McLaughlin is especially helpful. However, most chapters deal with both a principle and a time or rock unit. For example, the chapter on "Some Methods of Correlation by Physical Characters" could equally well have been "The Huronian Rocks of the Southern Canadian Shield." The Silurian Period is dealt with as "Silurian Reefs and Salt Basins," and the Devonian is presented as "The Devonian Period and the Evolving Vertebrates."

Many teachers may decide that it is too potent for the average class in elementary historical geology; but sophisticated beginners, all geology majors, and even professional geologists wishing to update their general geologic knowledge will find this extremely useful book fills their need for an advanced text.—*Walter H. Wheeler*

Light (Second Edition) by R. W. DITCHBURN; Vol. I, 520 pages; Vol. II; 330 pages; $4.95 each, paper, also as one volume (cloth); John Wiley & Sons, Interscience, 1964.

The title "Light" may seem unreasonably broad for a book of this size, but it is justified. Professor Ditchburn has covered thoroughly the whole subject of the nature and propagation of light, with excursions into neighboring areas of mathematics and physics. This is both an encyclopedia and a textbook, with emphasis on understanding ideas and principles. There are ideas here as old as Huygens' principle or the opinions of the Greeks, and as new as information theory or the optical maser; all are presented with extreme clarity and honesty.

Volume I covers optics as the study of light wave disturbances, including diffraction and polarization effects. Volume II deals with the electromagnetic and quantum theories of light, with further discussion of properties which can only be calculated from them. The underlying idea is that of successive approximations to reality. The ray picture of light is sufficient for geometrical optics; we find that the wave theory not only reproduces the rays and allows a description of interference phenomena, but also improves our understanding of geometrical optics itself (for example, Abbé's proof that one can see spurious detail through a microscope). In a similar way, the quantum theory both allows a description of photons and increases our ability to deal with the rest of optics.

The main limitation of this book is the fact that it uses only intermediate-level mathematics. The second volume is not quite up to the standard of the first, and the last three chapters (on quantum theory and the limitations of optical instruments) are only introductory. Several of the figures are incorrect enough to be misleading, and the list of plates appears in the second volume but not in the first. Such annoyances are trivial; this reviewer's advice is to buy Volume I before reading it (one does not want to give it back) and to read Volume II before buying it.—*E. Alan Phillips*

Essays on Physiological Evolution, edited by T. M. TURPAYEV; 364 pages; $12; The Macmillan Co.; Pergamon, 1965; J. W. S. PRINGLE, Honorary Editor.

This volume contains a series of essays originally gathered as a festschrift for Kh. S. Koshtoyants. At his untimely death it was converted to a memorial and published by the USSR Academy of Sciences in 1961. Although each author has had the opportunity to update his contribution, the long delay in producing the English edition limits somewhat the usefulness of the book.

There are 31 essays, several by distinguished Western biologists. The topics range widely through the areas of physiological evolution. Some of the essays are syntheses, and some research reports; there is an unevenness of quality and coverage which is typical of such collections. The book suffers from

a lack of a general introduction or overview of the topic. A short biography of Koshtoyants and a list of his works compiled by Turpayev will prove valuable to those interested in the life and work of one of Russia's leading comparative physiologists.

Comparative physiology in Russia has often taken a different course from that in the West. As a consequence, one finds in this book unfamiliar ideas expressed with erudition and some familiar concepts treated very naively. It is this difference that makes this volume unique and valuable. The bibliographies accompanying each article should provide a key to the Russian literature.

The translation is at times literal and awkwardly phrased in English, but it is relatively free of distracting errors. One finds occasional examples of loss of meaning or information content due to multiple translations, e.g., the swordtail poeciliid (*Xiphophorus helleri*) permuted to swordfish (p. 234). The price of the book is not unreasonable. It should prove interesting to those concerned with the interface of physiology and evolution.—*James Edward Heath*

Immunology (An Outline of Basic Principles, Problems, & Theories Concerning the Immunological Behaviour of Man & Animals) by D. F. GRAY; 154 pages; $2.95 paper; American Elsevier Publishing Company, 1964.

This book emphasizes immunobiology rather than immunochemistry. It covers those aspects of immunology which are of direct interest to students of human and veterinary medicine. The book includes the author's own interesting blend of "Genetic-Instructive" theories of antibody formation. Host-parasite relationships, pathogenesis, prophylaxis and immunotherapy, allergy and hypersensitivity and especially antibody formation, are discussed in detail.

The author fully realizes the limited state of our present knowledge in many areas. He reminds his readers that active research is in progress in many laboratories and foresees clarification of the confused picture he is unfortunately forced to present in many places. Perhaps, even now, he would wish to revise numerous sections. For example, he refers to the evidence available in 1962 on the structure of immunoglobulin molecules, which suggested that the heavy polypeptide chains of antibodies lacked specificity and the light chains determined their specificity. None of the more recent evidence for chain interaction, and specificity of heavy chains is discussed.

Students and general readers will find summaries at the end of each chapter, which are concise and useful. Gray decreased the value of the book to advanced students and research workers by including only a spotty selection of references chosen to suggest further reading or to document controversial points.

Since this book is inexpensive, easy to read, and reasonably accurate, it would seem to serve best as an adjunct to a more comprehensive text or detailed lecture series.—*Rose G. Mage*

Naturalistic Behavior of Nonhuman Primates by C. R. CARPENTER; 454 pages; $9.50; The Pennsylvania State University Press, 1965.

This volume contains a reprinting of seventeen papers by C. R. Carpenter. Included are the classic monographs on howling monkeys and gibbons, which more than any other papers laid the foundation for modern studies of free-ranging primates. There are shorter papers on spider monkey, gorilla, orang-utan, siamang gibbon, and the sexual behavior of rhesus monkeys. There are six papers interpreting primate society and new concluding remarks by the author. Until the appearance of this volume these articles have been out of print, scattered, and difficult to obtain. Now Carpenter's contributions have been brought together and made easily available, and this will increase their impact on the numerous students who are now following the example which Carpenter set in his study of howling monkeys thirty years ago.

The papers are presented in chronological order, but the reader is advised to

start with two of the general statements on the nature of societies of monkeys and apes, pp. 342–364. In these Carpenter states his aims and methods, and they form a useful background to the studies on particular species.

In spite of all the recent field studies, Carpenter's contributions still rank among the most useful, and there is no better statement on the goals, methods, and problems of the field studies than what he wrote in 1942.—*S. L. Washburn*

Fundamental Astrometry, Determination of Stellar Coordination by V. V. PODOBED; English edition edited by A. N. VYSSOTSKY; 236 pages; $7.50; The University of Chicago Press, 1965; Original Moscow edition 1962.

The field of Astrometry, i.e., the determination of coordinates of celestial objects has been overshadowed by the more glamorous branches of astronomy for a long time. Advanced and researched by only a handful of specialists, many of today's astrophysicists and space scientists are woefully misinformed about the importance, aims and methods of astrometry. One might have hoped that the present book would be capable of remedying this situation at least in part. Unfortunately, such hopes appear unfounded.

The book is essentially a collection of notes for a course that the author teaches at Moscow University. Almost two-thirds of the book are devoted to the discussion of technical details of the reduction of measurements made on meridian instruments. Descriptions of instruments and methods for the elimination of instrument errors are given in great detail, while one must look through the pages very carefully (and often in vain) for discussions of the physical significance of the subjects considered or their tie-in with the science of the universe as a whole.

The rest of the book, dealing with the determination proper of stellar coordinates by meridian observations and by photographic means and their combination into star catalogues of various types, is just as uninspired. Here again we miss the pointing out of important highlights.

The author refrains from the use of modern mathematical techniques, even where their application would be natural. Nowhere in the book are matrices and vectors used. Series developments are retained where exact formulas would have given better results and would have permitted deeper insight for the reader. The emergence of electronic computers and the accompanying change in techniques is ignored. (As desk calculators should not be used only to add logarithms, computers should not be used only to program formulas developed in the precomputer area.)

There is quite a number of small inaccuracies. The chapter on photographic methods ignores the developments of the last 30 years and contains several errors. Technical terms are sometimes improperly rendered (i.e., for instance "equalization" is used where "adjustment" is meant, etc.) and names are occasionally improperly re-transcribed into the latin alphabet.

I am afraid the book will do nothing to dispel the prejudices of some astrophysicists against astrometry which they believe to be a dull, stagnant discipline apart from the main stream of the problems on which today's scientists focus their interest. The specialist, however, will have to look elsewhere for more complete, more contemporary and more authoritative information.—*Heinrich K. Eichhorn-von Wurmb*

Histones & Other Nuclear Proteins by H. BUSCH; 266 pages; $9.50; Academic Press, 1965.

Kossel's book "The Protamines and Histones" appeared in 1921. The subject than went book-less for over 40 years. Suddenly in the past two years, two volumes devoted to histone matters have appeared, of which the present book by Professor Harris Busch is one. The sudden interest in the histones is, of course, due to the fact that it is to these proteins that nature entrusts the task of complex-formation with the genetic DNA of higher organisms. Further, DNA fully complexed with an appropriate histone is inaccessible for transcrip-

tion by RNA polymerase. Histones are therefore repressors of genetic activity. Investigation of the interactions of histone and DNA can, we imagine, provide a route to the study of the regulation of gene function, and hence to the molecular basis of development.

Study of the histones and their DNA complexes, the nucleohistones, is, then a current and exciting frontier of biology, although this excitement does not always shine through clearly in Professor Busch's book, which is in addition highly cancer oriented. It is, however, thin enough (266 pages) for the busy biologist to read it in its entirety, but thick enough to contain appreciable pertinent information. An introductory chapter provides an excellent historical summary of the subject which started with Miescher, also the discoverer of DNA over 100 years ago. Chapter I is devoted to the protamines which replace histones in the sperm of fish, birds and some invertebrates. Although the protamines are small (molecular weight 2000 to 5000), neither the number of different species of molecules present in any particular sperm, nor the amino acid sequence of any one species, is as yet known. There is clearly much to do in the protamine field. Chapter II summarizes the amino acid compositions of histone mixtures obtained from different cell types and the histone fractions separated from them, as well as the N-terminal amino acids of such fractions. Generally speaking, current histone fractionation procedures, discussed in Chapter III, divide whole histone into six sub-groups, each characterized by its own N-terminal amino acid, and characteristic amino acid composition. Interestingly, these characteristics, as well as the amino acid compositions of the tryptic peptides obtained from individual histone fractions, are similar, or indeed identical, throughout a wide range of living creatures, higher plants and animals. So far as today's knowledge goes, the histones of different creatures vary from one another no more than do proteins of other specific functions, as for example the hemoglobins, or the cytochromes. Chapter III concerns methodology—methods for isolation of nuclei and nuclear subfractions, as well as methods for the direct isolation of chromatin. Curiously enough, Busch favors for the latter purpose Mirsky's methods, developed nearly 20 years ago, which make use of high salt concentrations, and does not mention either the newer procedure of Zubay and Doty or our own methods which employ low ionic strength and sucrose density gradient centrifugation, and which yield nucleohistone much more closely approximating the native state than does the high ionic strength procedure. For separation of individual histones from one another, Busch favors the fractional extraction and chromatographic methods of Johns and Butler, and pays little attention to the methods of Rasmussen, Murray and Luck, which have proven so highly repeatable and successful in other laboratories. Busch likes starch gel electrophoresis, too, although his results, photographically presented, do not show clearly why he does so. Chapter IV concerns spatial relationships between DNA and histones, a subject which naturally is treated less than wholly satisfactorily since next to nothing is known about it. The following chapter, "Functions of the Histones," concerns principally the fact, alluded to earlier, that native nucleohistone, as well as appropriately reconstituted artificial nucleohistone, is inactive in the support of DNA-dependent RNA synthesis by RNA polymerase.

The primary structure of histone, a subject to which Busch has been a major contributor, is considered in Chapter VI. This subject is now approachable since Busch has been able to prepare two individual histones which yield, respectively, 24 and 14 tryptic peptides. It is to be hoped that the complete amino acid sequence of these two histones will be available in the near future. Chapter VII concerns histone synthesis, the exact geographical site of which cannot as yet be pinpointed, and histone turnover, which takes place at a slow rate, even in cells in which there is no DNA replication. Chapter VIII concerns the non-histone nuclear proteins, about which little is however known,

while Chapter IX discusses, although not in detail, several of the nuclear enzymes, particularly DNA polymerase and RNA polymerase.

Professor Busch is a cautious man, and he presents all, or nearly all sides, of every question, without coming right out and boldly stating which he prefers. This makes difficulties for the reader. Considering the present unsettled state of histone knowledge, equivocation may well, however, be the course of wisdom for the author of a book such as this. Professor Busch's book can be recommended to biologists, particularly those in the cancer field, interested in entering the field of nuclear metabolism; in it they will find not only a summary of what has gone on in the past, but also a wealth of references and a handy guide to Professor Busch's own publications.—*James Bonner*

Concepts in Quantum Mechanics by F. A. KAEMPFFER; 358 pages; $9.75; Academic Press, 1964. Vol. 18 of Pure & Applied Physics Series, edited by H. S. W. MASSEY & K. A. BRUECKNER.

Professor Kaempffer's book is a significant contribution to the enterprise of anchoring the conceptual framework of quantum mechanics firmly in the education of our graduate students. The title describes the purpose of the book accurately, and it is probably realistic to assume that the book will find its greatest use as a supplementary text in the standard quantum mechanics course.

Great stress is laid on the exploitation of symmetries and on the exposition of the structural features essential to an understanding of current views on elementary particles. The choice of material is quite idiosyncratic but, with the possible exception of an unproductive overemphasis on discrete symmetry operations, what is taken up deserves full attention. The guiding principle in the selection seems to have been an insistence on topics which, like spin, isospin, quasiparticles, and time (or, as Kaempffer properly prefers, motion) reversal invariance, have no simple classical correspondence.

The author breaks with tradition wherever possible, and usually with advantage to the treatment. For example, he assumes the fundamental properties of photons and eventually derives Maxwell's equations instead of quantizing them. He is to be commended for his efforts to show how quantum mechanics has, by concentrating on states and excitations, led to a subtle but radical change in our concept of a particle. Although, as Kaempffer points out, there is still room for an even better understanding of the notion of quasiparticles as something more than a useful mathematical device, every attempt to make the new attitude toward the concept of a physical system pedagogically palatable is welcome.

Occasionally, Kaempffer carries his antitraditionalism too far: Planck's constant is never mentioned, and it is tacitly assumed that the reader has grown up with some "natural" units (although, surprisingly, an explanatory statement is made about Boltzmann's constant). In view of this attitude, which reflects the abstract temper of the book, "physicists engaged in experimental research" whom the author includes among his intended readers, may find the distance between Kaempffer's concepts and the objects of their measurements unnecessarily extended.

Kaempffer shows impatience with perturbation theory and its achievements; yet, his brief chapters on quantum electrodynamics and Feynman diagrams are some of the best parts of the book. Here, as elsewhere, he has some perceptive, and often pungent, things to say about the meaning of what has been done, or the prospects for the future. ("One has to go back in the history of physics to Faraday's concept of the field line if one wants to find a mnemonic device which matches Feynman's graph in propagandistic persuasiveness.")

Since this is an interesting book, there is inevitably cause here and there for disagreement with the author's approach. For instance, one may remain unconvinced that the early sections pur-

porting to contain a theory of measurement even approach that lofty goal; nor are the speculations on the ultimate role of superselection rules very illuminating; and the enormous care lavished on the consistent use of vector spaces leads to a profusion of direct products and not much greater clarity. The teacher of quantum mechanics who is considering "Concepts of Quantum Mechanics" as a text should also realize that such "mathematical stunts" as the theory of the hydrogen atom are omitted, although one supposes that they are taken up as assigned problems in the author's introductory two-year course at the University of British Columbia, upon which the present volume is based.

Kaempffer's style is, on the whole, unusually fine, but it is marred by such terms as "conjugality" and by innumerable split infinitives.

On balance, the book succeeds well in its announced attempt to present advanced quantum mechanics from an elementary point of view. It will be read with profit, and enjoyment, by those who manipulate easily in linear vector spaces and who know that "abstract" is not the antonym of "elementary."—*Eugen Merzbacher*

Radiation Damage in Crystals by L. T. Chadderton; 202 pages; $6.75; John Wiley & Sons, 1965. (Methuen Monographs on Physical Subjects.)

There are many books on scientific subjects which are scientifically correct, which have a fairly uniform level of sophistication, but which are very poorly written from the pedagogical point of view. Chadderton's "Radiation Damage in Crystals" is a prime example of a book which is not only scientifically correct but which satisfies also all the requirements of an introductory text. It is really a gem in its field. It does not require any complicated mathematical concepts and it stresses the basic physics of the various processes taking place in the crystal subjected to radiation. Starting with a description of various lattice defects such as point defects, dislocations, crowdions, split interstitials, etc., it goes into thermal spikes, displacement spikes, and correlated collisions. The reviewer was particularly impressed by the treatment of focussing, channelling, sputtering, etc. The computer simulation of irradiation phenomena is treated in detail, stressing the tremendous impact it had on our intuitive picture of what goes on in a crystal. It is also quite properly pointed out that these "mathematical experiments" do not supplant real theoretical calculations in which the role of various parameters can be quantitatively and often even analytically understood. A special compliment should be paid concerning the excellent illustrations of which Fig. 50, showing the regions of applicability of various stopping power formulae, is a prime example. There are many parts of the book which will be of value not only to those who need an introduction to this field but also to many who are already deeply in it. Here, a good example is the chapter on atomic interaction potentials. After reading the book one has a feeling not only of having had a guided, though quick, tour in a new world but also an esthetically satisfactory experience. —*R. Smoluchowski*

Mammals of the World by E. P. Walker *et al.;* 1500 pages; Vols. I & II boxed set $25; Vol. III, Bibliography, $12.50; Johns Hopkins Press, 1964.

More than thirty years ago E. P. Walker set himself the horrendous task of describing and illustrating all the genera of living mammals. Aided by six associates, a massive two-volume work with an additional volume of bibliography was published in 1964. No other book or books dedicated to this purpose have been published, and this reviewer is lost in admiration for anyone who has the courage to undertake, and the tenacity to finish, this enormous labor.

The fundamental purpose of a work

such as this is to establish a single source to which scientist or amateur may turn for information concerning a certain genus of mammals. Thus, the authors are faced with the problem of defining each genus and including enough solid facts about it for the serious student, while making the text moderately readable for the amateur. They have succeeded in part in this endeavor, for the text, which is divided into descriptions of each order, family and genus, is interesting and informative.

When one examines the books for concrete scientific information, however, it is apparent that they suffer from an absence of bibliographic references in the text. There is a bibliography of about 4500 titles in the back of Volume I, and Volume III is totally devoted to bibliography and has over 40,000 references arranged under various subject headings. The text does not refer to either of these bibliographies.

Systematists will undoubtedly argue forever about the various genera of mammals, and this is their prerogative as long as they provide the interested observer with basic references so that he may form his own opinion. The mammalian genera listed in these two volumes differ greatly from those in Simpson's classical work (Simpson, G. G., 1945, *Bull. Am. Mus. Nat. Hist.*, *85*) and other standard references; yet, without a bibliography, the reader cannot even begin to check on the accuracy of the authors' judgment. This criticism applies also to statements concerning the natural history and physiology of the various genera.

The authors must have expended great effort to obtain photographs of each genus of mammals, and some of them are very good. Others, however, are of such poor quality that they lack any illustrative value. Some are of study skins, others are of bodies still wet from preservative; in some the reproduction or the original photography is poor, and in others there is no explanation in the text of the insets in the illustrations. Simple sketches of genera for which there was no good photograph would be much more informative. In these respects the work falls short of being a reliable reference.—*Charles P. Lyman*

Mars by R. S. Richardson; Paintings by C. Bonestell; 151 pages; $8.50; Harcourt, Brace & World, 1964.

This book is written by Robert S. Richardson, a professional astronomer well-known for his popular writings, and includes 11 paintings in color, by the equally well-known astronomical illustrator, Chesley Bonestell. Richardson qualitatively describes, with considerable success, not only the physical environment of Mars and its characteristic and enigmatic features, such as the seasonal variations, the canals, and the blue haze, but also manages to introduce, on a very simple level, a fair amount of celestial mechanics and interplanetary transfer trajectories. There is a good discussion of the relative motions of the Earth and Mars, with an emphasis on naked-eye observations of Mars near the 1971 opposition: the reader will have plenty of time to plan his observations carefully. The description of observations is sometimes given in the first person, and Richardson very successfully conveys the frustrations and exultations of conventional planetary astronomy. The volume is also graced with a charming speculation, which Richardson is quick to disown, on the circumstances surrounding Jonathan Swift's celebrated announcement that Mars is accompanied by two small moons, one hundred and fifty years before their discovery by Asaph Hall.

Unfortunately, the book is flawed by many errors of fact or interpretation. It is stated that the thin Martian atmosphere is unable to support dust; but the yellow clouds have the same polarimetric properties as the granulated material of the Martian bright areas, and theoretical studies by Ryan and by Gifford have shown that the raising of the dust by Martian winds leads to no apparent theoretical difficulties. The book states that, in the absence of air resistance, two falling objects of unequal mass have the same motion, because the gravitational force exerted by the Earth is independent of the mass of the objects. It is, of course, the acceleration, not the force, which is independent of mass. National space agencies in both hemispheres will want to investigate more thoroughly the de-

picted "retroactive" rockets used in planetary landing missions. They are employed, evidently, only after accidental crash landings.

On the biological side, there are a number of implicit or explicit fallacies. The absence of oxygen in the Martian atmosphere is considered to restrict severely the possibility of Martian life. Chlorophyll is considered a molecule which might reasonably be expected in Martian plants. The possible presence of 1 mm-atm of NO_2 in the Martian atmosphere—the actual upper limit is now known to be much less—is said to rule out automatically life on Mars because of the vaunted poisonous properties of nitrogen peroxide. Despite a nod to molecular biology in Chapter I, Richardson later delivers himself of the opinion that the study of Martian organisms would be of little general interest to biology. Many would argue that since all organisms on the Earth have apparently evolved from a single instance of the origin of life, only by investigating extraterrestrial life can the biologist obtain any sense of perspective. The forthcoming publication of the National Academy of Sciences, "Biology and the Exploration of Mars," is devoted in part to an elaboration of this thesis.

The discussion of life on Mars, in fact, underlines one general weakness of the book. In a work which discusses the percentage of women allowable in a future Martian colony, its calendrical conventions, hydroponic systems, and the psychological problems attendant on extraterrestrial colonization, there is a curious lack of justification, scientific or otherwise, for the exploration of Mars. The enormous significance of scientific exploration of Mars, whatever the results, for biology, for geology, for meteorology, for cosmogony, are never mentioned. A similar lack of excitement carries over to some of Bonestell's paintings. In years past, a Bonestell illustration of a spacecraft docking manoeuvre with a familiar geographical feature looming below was exhilarating in its realism. However, reality has now caught up with Bonestell. Photographs of southern California with astronaut rampant have graced the front pages of our newspapers. The place for artistic license would have been in the depiction of the Martian landscape *in situ*. The bright areas are deserts and it is easy to paint deserts, but what do the dark areas look like? Of all the illustrations in the book, not one attempts to depict the dark areas with the verve and imagination that made Bonestell's paintings in "The Conquest of Space" so memorable. Yet, some of the paintings are breathtaking, and the text, taken *cum grano salis*, is lively and informative popular writing.—*Carl Sagan*

Basic Concepts of Ecology, by C. B. Knight; 468 pages; \$8.50; The Macmillan Company, 1965.

The title of this book is misleading in that the author is primarily concerned with describing the environment rather than presenting fundamental concepts. An excessive amount of terminology and factual material, sometimes of dubious relevance, is introduced—an entire chapter, for instance, is devoted to climatology—with little attempt made at integration. On the other hand, many of the most important and interesting ecological principles, problems, controversies, and fields of endeavor go without proper illustration, analysis, and sometimes even mention. As examples, the idea of the ecological niche, population growth and oscillations, population and community energetics, biological competition, predation, and community structure all receive poor and superficial treatment, while biogeochemical cycles are not included.

The book cannot be recommended for introductory college courses in ecology, for which use it was designed. Other introductory texts contain much more comprehensive, up-to-date, balanced, and stimulating coverages. The subject matter of modern ecology is certainly not adequately represented.—*John J. Gilbert*

Advances in Child Development & Behavior, Vol. 2., edited by L. P. LIPSITT & C. C. SPIKER; 256 pp; $12.50; Academic Press, 1965.

This book is the second volume in what should prove to be a very important series to those interested in maintaining close contact with research trends in developmental psychology. The proliferation of new psychological journals as well as the scattering of research reports relevant to child psychology throughout the psychological literature precludes the possibility of most investigators following developments on topics beyond their own areas of specific interest. Consequently, the singular value of the series must lie in the efforts of the contributing authors to draw together information pertinent to their particular areas of competence from the many diverse original sources in addition to presenting their own recent efforts in those areas.

The present volume includes eight papers representing several approaches to the study of development. Experimental analyses of learning processes are discussed in (1) A. Castaneda's methodological and theoretical treatment of the paired-associate learning task as a vehicle for studying the possible motivational characteristics of associative habits, (2) one of the most comprehensive treatments available of the transfer effects of stimulus pretraining on subsequent learning by J. H. Cantor, (3) a presentation of the relatively recent but rapidly developing concern with factors influencing social reinforcement by H. W. Stevenson, and (4) G. Terrel's compilation of findings from widely diversified situations related to the effects of delayed reinforcement. From a considerable body of experimental and descriptive data, S. H. White marshals support for an hypothesis concerning the ontogeny of the learning process. Despite starting from rather tenuous premises, E. S. Gollin presents a lucid argument in favor of the developmental approach traditional to the field of child development. The factors influencing the development of social responses in infancy and early childhood are discussed by R. H. Walters and R. D. Parke, while H. V. Meredith presents a sizable catalogue of correlational analyses of interage relationships among anatomic measures.

The strength of this series will be maintained in the future, as it has been in this volume, by the inclusion of papers representing breadth both across the field and within the contributors' areas of competence. There is obvious justification for all but the price of this series.—*David A. Wicklund*

Fishes of the Western North Atlantic. Part Four. Soft-rayed Bony Fishes, Order Isospondyli (part), Suborder Argentinoidea, Suborder Stomiatoidea, Suborder Esocoidea, Suborder Bathylaconoidea, Order Giganturoidei, edited by Y. OLSEN; 599 pages; $27.50; Sears Foundation for Marine Research, Bingham Oceanographic Laboratory, Yale University, 1964.

Part four of the *Fishes of the Western North Atlantic* continues the tradition of the first three parts. Its appearance only a year after part three, however, sets some sort of a record. Part one appeared in 1948, part two in 1953 and part three in 1963. Like its predecessors, part four is a handsome volume and a magnificent example of the printer's art; this makes its price a little easier to bear.

Fishes of the Western North Atlantic is intended to be a series of monographs bringing together our taxonomic knowledge of all the fishes occurring in the western part of the Atlantic Ocean from Hudson Bay to the mouth of the Amazon. The authors are all competent specialists and each section is a major ichthyological contribution. Following the examples set by Bigelow and Schroeder in the first two numbers, most authors have included extralimital species in their keys for comparative purposes and this greatly enhances the value of the work.

The fishes treated in part four are nearly all deep-sea forms that are not apt to be encountered by the layman. Many are known from only a few specimens. Professional ichthyologists will welcome this volume as a ready aid for identifying these little-known species

and as an introduction to the scattered literature of deep-sea fishes. Unfortunately, titles are not included in the lists of bibliographic references but they would have been welcome in this field where we so often locate pertinent papers by scanning the citations in related and not-so-related papers. To have included the titles would probably not have added more than 5 or 6 pages to the entire volume. It would have been nice to have had the title of the articles included in the running heads for convenience in using the book.

Perhaps the major drawback of such an ambitious series of monographs is the relative inflexibility of format and content. The section on the pikes (Suborder Esocoidei) is weaker than it should have been because this is a freshwater taxon and the author was limited to two species that have been recorded a few times from brackish water. In this case, adhering to the rules has lessened the value of the monograph. Presumably the editorial committee has been reluctant to change, but isn't rigidity a major handicap in a series such as this? This reviewer feels that any conformistic tendency should have been directed toward the illustrations, some of which should have been redrawn for this work.

Certainly the *Fishes of the Western North Atlantic* is a fine series and it is our hope that part five will appear in the near future.—*C. Lavett Smith*

Physics of Thin Films, Vol. 2, edited by G. HASS & R. THUN; 441 pages; $15; New York, Academic Press, 1964.

This volume, the second of four, focuses on recent advances in thin film research and development. Included are three papers on fundamental film properties as well as two on electric film component fabrication and two discussions of optical properties on thin films. The content surveyed in these chapters has been extensively described in the literature and is well referenced by the individual authors.

The first chapter, by C. Neugebauer, investigates the dependency of evaporated film properties on structural disorders (instead of film thickness). Thin films differ from bulk materials by reason of their high defect density. Indeed, sufficiently thin films may exist only as islands, which are formed at the time of their initial deposition. The initial atoms of deposited material are subject to reevaporation; the probability of this phenomenon is decreased by low substrate temperature and high binding energy of the adsorbed atom to the substrate. As migrating adsorbed atoms form aggregates, they tend to become more stable structures, having lower probability of reevaporation since they are bound to each other. As the islands' surface-to-volume ratio decreases with larger island size, the stability similarly improves. For a given volume of evaporated material, aggregates are most likely to represent the final film condition when the evaporation process is accompanied by a high substrate temperature, a low deposition rate, and a choice of substrate which results in low substrate-film binding energies. One manifestation of a film aggregate is the fact that the electrical resistivity is orders of magnitude greater than that of a continuous structure, and that it exhibits a negative temperature coefficient in the case of metallic films. Thermionic emission and tunneling have been suggested as the conduction mechanisms of island films.

Further material deposition results in continuous films, containing several classes of dislocations and other defects. These are extensively discussed by Neugebauer in light of both theory and experiment in this paper.

Two other functional papers are included: "Interaction of Electron Beams with Thin Films" by C. J. Calbick and "Solar Absorptance and Thermal Emittance of Evaporated Coatings" by L. Drummeter and G. Hass. In the first paper, thin films are of particular interest because they provide miniature laboratories in which to study, among other things, scattering of electrons by single atoms and monolayers. In the second paper, thin films provide a vehicle to prepare surfaces of known pure composition to study their solar absorptance and thermal emittance properties which are related to such

different questions as the control of temperature of space vehicles and the efficient conversion of solar energy. The two articles on optical properties of thin films discuss thin film optical reflection and transmission measurements, and the design of antireflection coatings for visible and infrared windows and lenses.

In a chapter indicating the future of thin film active circuit devices, Paul Weimer summarizes the development and present status of the Insulated Gate Thin Film Transistor, first reported in 1961. Finally, Schwartz and Berry provide a comprehensive and informative discussion of Thin Film Components and Circuits, relating gross thin film properties to the fabrication of precision electrical network components.

This publication serves a useful purpose in presenting recent thin film developments in a timely, compact, well written and well referenced book.—*Ralph W. Wyndrum, Jr.*

Wind Waves; Their Generation and Propagation on the Ocean Surface by B. KINSMAN; 675 pages; $23.35; Prentice-Hall, 1965.

During the last ten or fifteen years, there has been something of a revolution in our understanding of the physics of ocean waves and in the art and techniques of practical wave forecasting. The author of this book approached and mastered the subject during the time of change; the book is almost a chronicle of the revolution.

It is very personal work, and indeed much of its charm and value derives from this. It begins with the most basic properties of surface waves presented in an elementary way. These are developed and extended, gaining in sophistication until the frontiers of the subject are approached. From simple sinusoidal waves, we progress by Fourier synthesis to more complicated wave forms, by the introduction of statistical ideas to the specification and properties of a random wave field, to the non-linear aspects of the phenomena, the generation mechanisms and the various approaches to wave forecasting. In some cases, where finality has not been reached the author tries to present the opposing points of view, usually providing enough information to give the reader a basis for a judgment of his own. A great deal of information is presented; as a mere compendium the book has value. But it is much more than that.

The author is clearly very conscious of the difficulties experienced by the student oceanographer threading his way through the contributions made by his predecessors. Troublesome points are not hidden. In a tiresome calculation, the student is encouraged to stand back to see the point of the whole exercise, to resist the tendency to become lost in the trees. The whole book is enlivened by many an aside, a literate reference or a salty note drawn from the author's experience. It is a book that could have been written for specialists much more briefly and (in the mathematical sense) elegantly, but this was not the author's purpose. His book is for students new to this branch of oceanography; he tries to lead them through the various ideas of the subject, to show how life was breathed into it by those who made it. It is an unusual book in this, and is surely stimulating and successful.—*O. M. Phillips*

Protozoan Nutrition by R. P. HALL; 90 pages; $3.50; Ginn & Co., Blaisdell, 1965.

Protozoa are engaging beasts, representing many long-independent lines of animal evolution paralleling our own metazoan line. With recognition that many protozoa permit better glimpses of aspects of metazoan metabolism than permitted by bacteria or fungi, while manipulable by the cheap, handy methods of microbiology, something of a biochemical goldrush is making protozoa widely used research objects. The first step towards biochemical Parnassus is to grow them. The book outlines the recipes for growing protozoa, with neat sketches of the biochemical motives for the aesthetic-biochemical motives underlying the often passionate absorption in refining recipes for culture media. The book is recommended as a charming

survey of the scene—(it is up-to-date) blended with gastronomic lore reminiscent of the great French cookbooks.

The book is therefore valuable as an introduction to biochemistry via the backdoor of natural history—an unpretentious biochemistry without tears.

The illustrations are well chosen. The main topics are phagocytosis and pinocytosis, principal fuels, growth factors, and a timely section on the use of protozoa as pharmacological tools.—*S. H. Hutner*

Guide to the Interpretation of Space Group Symbols by S. K. DICKINSON, JR., 44 pages; copies available on request; Clearing House for Federal Scientific & Technical Information (CFSTI), Sill Building, 5285 Port Royal Road, Springfield, Virginia 22151.

In this report, originating at the Air Force Research Laboratories in Bedford, Massachusetts, a scheme for presenting and interpreting the 230 space groups is developed. The step-wise development from crystal systems to space groups is traced in the report and center-fold chart. This report and the chart, in wall size (22 × 34 inches) and in color, are currently being distributed without charge as a service to those who may find them of use in teaching, studying, or research.

Catalysis & Chemical Kinetics by A. A. BALANDIN, *et al.;* 255 pages; $10.00; Academic Press, 1964.

According to the preface, the purpose of this book is to review some investigations carried out by Polish scientists in the fields of catalysis and kinetics during the post-war period, and thus arouse wider interest in kinetic research. This has been accomplished.

Starting out with a review of Soviet contributions to catalysis and kinetics, the book covers such topics as redox systems in solutions, alcohol dehydrogenation processes, the potential theory of adsorption, magnetic properties of contacts, and interfacial kinetics.

Particularly interesting are detailed discussions of the multiplet theory of catalysis, electrical conductivity measurements on oxide catalysts and Fischer-Tropsch hydrocarbon synthesis.

Catalyst selection, hydrolytic hydrogenation, enzymatic catalysis, thermal dissociation processes, and hydrogen peroxide catalytic reactions round out the fourteen papers presented.

Each chapter, or paper, is self-contained and covers a specific area of interest. These chapters serve as an excellent starting point for further inquiries into the topics covered. One slight drawback is that only about 30% of the references are in English language publications. This, of course, poses no problem to anyone having access to translation facilities or English reprints, but may to one less fortunate.

Anyone interested in catalytic kinetics should find this book very informative. The editors have done a good job in selecting a varied cross section of topics.—*Francis Eastburn*

Principles of Physical Geology by A. HOLMES; 1288 pages; $12.00; Ronald Press Co., 1965.

This book, written by a noted and well qualified British geologist, is massive (5 pounds 10 ounces), wide-ranging, impressive, and well done. It is essentially a new book rather than a revision of Holmes' widely used text which was published in 1945. The 1964 book has 31 chapters (vs. 21 in the 1945 edition), 1288 pages (vs. 532), and 880 figures (vs. 95 plates and 262 figures). Furthermore, a page in the 1964 book includes more words than a similar page in the 1945 publication. It is well indexed, attractively illustrated, and relatively free of printing errors. European, British, and American references are listed at the end of each chapter.

Holmes spent seven years full time in the preparation of the 1964 book. He points out that about 90 percent of all of the geologists who ever lived are still alive and active and that geology has been revolutionized by the research, observations, and new techniques of the last two decades or so. The rate of scientific publication has been doubling every few years and, in geology, this rate has been advancing. Thus, it is increas-

ingly difficult and time consuming to "dig out the essentials of recent progress from the avalanche of publications in which they are recorded."

In addition to topics generally discussed in American physical geology texts, Holmes has ranged very widely. He has also included considerable material concerning controversial and speculative subjects: e.g., the possible expansion of the interior of the earth throughout geologic time, energy sources necessary to keep the earth's internal "machinery" going, granitization, continental drifting and paleomagnetism, gravity gliding as a major factor in the folding of rocks, fluidization, the concept of rheidity, and evidence from a study of Paleozoic corals that the day has gradually lengthened. Holmes considers the evidence available for each topic and states his conclusions.

Principles of Physical Geology should be a useful reference for graduate students preparing for comprehensive exams. However, it may prove too long and expensive for general use in courses in physical geology in American colleges. It seems most suited as a text or key reference in some sort of advanced general geology course for undergraduate geology majors. All majors and teachers of the subject will want a copy for their personal libraries.—*Richard J. Ordway*

Geochemistry of Sediments, Brief Survey, by E. T. Degens; 342 pages; $13.25; Prentice Hall, 1965.

Few geochemists are able or are willing to accept the challenge of producing a book that summarizes the present state of knowledge in both inorganic and organic sedimentary geochemistry. In such a rapidly moving field, a book of this type must of necessity be outdated even before the manuscript reaches the publisher. Degens has not only accepted the challenge but has met it with a book that is of value not only to the specialist but to anyone who wishes to become familiar with the problems and accomplishments in sedimentary geochemistry. The treatment of each subject is necessarily brief; however, the author has included a remarkably complete selection of the spectrum of sedimentary geochemistry. This brevity spawns a certain amount of dogmatism, and a specialist in the geochemistry of clay minerals, carbonates, evaporates, or hydrocarbons, for example, may not be satisfied with the choice of material or the emphasis given to a particular point of view in the chapter devoted to his specialty. The general treatment however is remarkably good, and the book is far more than a recitation of the literature. The review of each subject is critical, and the clear statement of major problems shows refreshing insight in suggesting both problems and approaches for future research. The author has made a deliberate attempt to synthesize ideas and to place conflicting data in perspective. A well-chosen bibliography for each chapter includes 1964 literature and offers the reader an entry into the important ideas and critical problems.

A brief statement of general background and a critical examination of problems, conclusions from previous investigations, and thoughts for future research form the pattern of each subject. Such a book should serve a real purpose as an excellent supplementary text in any course in sedimentation or geochemistry. In addition it should be of value to the reader who wants a critical review of sedimentary geochemistry. However, its uniqueness and its greatest value lie in giving general background, ideas, research leads, and an introductory literature to anyone contemplating research on a problem in sedimentary geochemistry. As such, it cannot but stimulate graduate students and researchers in the subspecialties of sedimentary geochemistry or peripheral fields.

The author offers an apology in his preface for a "Teutonic-flavor." The crisp, easy style of the text makes this warning unnecessary. The book is a pleasure to read. Indeed, more geologic writing should be done with such a "Teutonic-flavor."—*R. C. Murray*

J. J. Thomson & the Cavendish Laboratory in His Day by G. Thomson; 186 pages; $4.95; Doubleday & Co., 1965.

Biographies of the famous sometimes

are written by their relatives. Often these are sentimental surveys of what might-have-been or shallow, misinformed works. Happily, this book is neither. Sir George Thomson has produced a concise and pleasing biography of his father. The author has intended this book for a youthful audience but his easy style and his insight into the physical problems of his father's day should make this interesting also for those with more than a "popular" knowledge of physics.

As an introduction to the half-century of physical researches after 1890, Thomson has included a good chapter on the evolution of concepts of electric current and of field theory in the nineteenth century. J. J. Thomson's researches in physics form the core of this book. The author traces J. J.'s career from his early studies on vortex rings and moving tubes of force, to investigations on the discharge and conductivity of gases, the measurements of the velocities and specific charge of cathode ray particles and the later work with positive rays and non-radioactive isotopes. For a century, the atom had seemed to be the smallest unit of matter; J. J. Thomson's discovery of a smaller component, the electron, posed new problems in the theories of matter and chemical combination. The Cavendish Laboratory investigations of these problems are discussed in some detail.

Though one is informed of J. J.'s personal life and his administrative activities, the author never hobbles the story with overemphasis of these aspects. As with other volumes in the series "British Men of Science," this book can be recommended.—*C. S. Gillmor*

The Future of Fuel Technology, edited by G. N. Critchley; 237 pages; $10; The Macmillan Co., Pergamon, 1964.

This short volume is the proceedings of a conference held in Amsterdam in 1963 by the Institute of Fuel. The title is misleading in that very little about the future is discussed. Most articles deal with the current activities of various government establishments in Western Europe. These activities appear to this reviewer to be concerned with developmental and present day problems. One would have hoped to find more on the use of combustion processes for magnetohydrodynamic generation of electricity, possibility of various fuel processing systems for fuel cells, the use of natural gas in aircraft gas turbines for "topping" electrical and remote power.

Perhaps the engineer working in the industrial energy field will find the book of value in acquainting him with European activities.—*I. Glassman*

Elements of Cloud Physics by H. R. Byers; 191 pages; $7.50; University of Chicago Press, 1965.

Dr. Byers has done a fine job of presenting the classical thermodynamics and physics essential to an understanding of the physics of clouds. This was his stated purpose, and for the most part he resisted the temptation to include a great deal of recent experimental and theoretical work which would be quickly outdated by the rapid development of this field and which can best be covered in current periodical literature. Where he does refer to recent work he has been conservative and cautious in his selection. The student of cloud physics who uses this book will have it made quite clear that there are a great number of variables whose roles in the development of precipitation are as yet not in sharp focus.

It is perhaps unfortunate that the author chose not to integrate the use of radar in cloud physics into his chapter on precipitation growth and cloud dynamics and, except for its possible role in coalescence drops, atmospheric electricity has been omitted. Granted there are books treating these subjects, as the author states but, at the same time, I feel more could have been done to tempt the students to refer to them.

I would like to have seen the author wax philosophical in an additional short chapter and put cloud physics into perspective. I can think of no one whose background better qualifies him to do this than Dr. Byers. The prospective student is caught between a synoptic and dynamic meteorological profession

which ignores cloud physics and some cloud physicists (not the author) who ignore the dynamicists and are too prone to feel the microphysics of clouds is all-controlling.

The student who masters the material in this book is ready to move into the literature to fend for himself. I would strongly recommend it as required reading for any upper-class undergraduate or graduate student in physical meteorology or cloud physics.—*Charles L. Hosler*

Closed Systems & Open Minds: The Limits of Naivety in Social Anthropology, edited by M. GLUCKMAN; $7.95; Aldine Publishing Company, 1965.

This is a collection of essays directed to British social anthropologists, by eight social scientists associated at one time with the University of Manchester. Dealing with such diverse matters as intercaste conflict in Indian Villages (Bailey), mobility and class in a Scottish mining area (Watson), aspects of puberty rites in Northern Rhodesia (Turner), social structure of new towns in the Rhodesian copper belt (Epstein), and behavior within British clothing and engineering factories (Lupton and Cunnison), the essays and the long interpretative conclusion by Max Gluckman and Ely Devons attempt to define the field of social anthropological inquiry and to suggest the procedures by which anthropologists may make use of the data and approaches of other disciplines. They say that in incorporating material from such tangential sciences as psychology, economics, political science, sociology, and "cultural" anthropology, the social anthropologist may be, indeed ought to be, intentionally naive, particularly where the tangential findings are irrelevant to his inquiry. For example, in the anthropological study of circumcision rites, it makes little difference whether the Freudian explanation that these rites relate to castration or that of Bettleheim that they represent womb envy (or both or neither) is in fact correct. Since his interest is in the social field, the anthropologist can afford to

assume a naive conception of personality and its processes.

The essays themselves are interesting and Turner's analysis of ritual symbolism among the Ndembu is brilliant. The argument of the conclusion, however, is overstated, unconvincing where it is not truistic ("...a social or human scientist may profit by studying disciplines other than his own. It is dangerous to practice them without training and appropriate skills" p. 261), and occasionally inconsistent. Few would argue that the researcher ought to venture beyond the limits of his competence. But neither must we decide that the traditional disciplinary boundaries forever delimit inquiry, and that he who violates them does so in necessary peril.—*Michael M. Horowitz*

Human Motivation: A Symposium, edited by M. R. Jones; 87 pages; $4.25; University of Nebraska Press, 1965.

This little book contains the revised papers presented at a symposium on human motivation during the XVIIth International Congress of Psychology in 1963. Brief critical comments by two discussants accompany each of the four papers. The reviewer found the book to have little value, either to the general reader or to the interested specialist: its presentations are too concise and technical to serve as a useful introductory survey, and there is little for the specialist in the way of new ideas, new data, or clarification of the rather confused state of much thinking about motivation.

John W. Atkinson (University of Michigan) reviews his well-known work on achievement motivation which involves an integration of the motive itself (conceived as a stable trait) with expectations concerning the effects of action and the incentive values of the effects. He has some interesting things to say about the law of effect and anxiety, and I found this the most rewarding paper in the collection, despite agreement with Ingmar Dureman (Uppsala University) that there are psychometric difficulties in the measurement of achievement motivation. Abraham H. Maslow (Brandeis University) attempts to show that love and curiosity are basic (instinctoid) needs, like needs for vitamins, whose satisfaction is essential to psychological well-being. Unfortunately, Maslow presents little evidence. K. B. Madsen (Royal Danish College of Education, Copenhagen) surveys theories of motivation and attempts a synthesis. Neither discussant (Dureman and Marshall R. Jones, of the University of Miami, Florida) is pleased with Madsen's synthesis, and I share their disenchantment. Janusz Reykowski (University of Warsaw) presents a complex experiment, the results of which he believes show that motivational concepts are insufficient, alone, to account for behavior.

The discussants are critical of these papers and their criticisms are well-taken.—*Charles N. Cofer*

Geohydrology by R. J. M. De Wiest; 366 pages; $10.50; John Wiley & Sons, 1965.

This text gives the student an outline of nearly all the basic concepts of the movement of water underground, developed as rigorously as possible in an introductory text. Because of the broad scope the author has assigned himself, most of the topics are considered only briefly; the student will probably want to avail himself of a very good list of references to clarify entirely some of the concepts.

The present text is to be followed by another called "Hydrogeology." Confusion is bound to arise as no two references are likely to agree on the distinction between the words. The present volume deals with the more deductive phase of ground-water study; it treats of the mathematical models developed on the assumption of the homogeneous aquifer and, in brief manner, of the physical principles involved. The second volume will treat the geological and chemical aspects of ground-water study and methods of exploration.

Nearly one-third of the present text is given to a discussion of surface-water features; unit hydrographs, flood rout-

ing, maximum floods, and other statistical phases of surface flow are briefly treated. The section on ground water develops the classical mathematical theory of ground-water movement through the theory of the leaky aquifer, and also includes brief treatments of the movement of immiscible fluids, as in oil fields, and of miscible fluids, as involved in problems of salt-water encroachment. Phenomena of dispersion and of osmotic and electrical forces as causes of ground-water movement are briefly considered.

Some typographical errors and a few places where Professor De Wiest's linguistic abilities have led to expressions not quite in the English idiom, are found. All in all the text is probably about the most satisfying introductory treatment in English of the fundamental quantitative concepts of ground-water movement.—*Charles V. Theis*

Theory and Practice in Experimental Bacteriology by G. G. Meynell & E. Meynell; 288 pages; $9.50; Cambridge University Press, 1965.

One occasionally hears or reads of a complaint that the present day college graduate with some course work in bacteriology has no concept of how to make media, measure bacteria, effect proper sterilization, etc. Although these graduates may have a good grasp on the latest information of the mode of action of antimicrobial agents, carbohydrate metabolism, the structure of DNA and protein synthesis, they are at a loss for the rudiments of the "bread and butter," day-to-day, work of the laboratory.

For all who have raised such a complaint, this book should be placed within arms' reach, to be commended as necessary reading to the new employee before he is even shown the bench where he will be doing bacteriological work.

One might best describe this publication as "The Methods of Basic General Bacteriology, Brought Up-To-Date." It is clearly the old classical treatment of laboratory fundamentals, but with the refinements and apparatus of today's laboratory. Also, considerable space is given to the basic mathematics of bacterial growth and the statistical

determination of M.P.N. and ED50. This will make a fine reference source for a course in general bacteriology or a text for a course concerned with a technical training program. The information found in this volume is primarily that which one would seek in any of the "Standard Methods" volumes, but written in a text book style.

This volume has been organized by the authors into seven chapters: 1. Measurement of Bacterial Mass and Number, 2. Bacteriological Culture Media, 3. Oxygen, Carbon Dioxide and Anaerobiosis, 4. Sterilization, 5. Examination of Bacteria by Microscopy, 6. Quantitative Aspects of Microbiological Experiments, and 7. Miscellaneous Techniques.

The first six chapters represent fairly thorough presentations of the basic methods of interest while the last chapter does not have this quality and the few techniques presented here would have been better presented elsewhere in the volume.—*Bernard W. Koft*

The Elements of Stochastic Processes with Applications to the Natural Sciences by N. T. J. BAILEY; 249 pages; $7.95; John Wiley & Sons, 1964.

Like probability theory, the theory of stochastic processes has the feature that many of its most elementary theorems are based on rather deep mathematics while many of its most advanced theorems are known and understood by research workers who do not have the mathematical background to understand the proofs. Bailey's clearly and carefully written book is an introduction to stochastic processes for scientists (mainly biologists since there are few applications to physics or chemistry) wanting a brief and readable survey of what can be done with stochastic processes. It may not be useful as a text in a mathematics course which aims to prove most of the theorems stated.

The book presupposes a basic knowledge of probability, complex variables, matrices, and differential equations. The topics covered are best indicated by the chapter headings: Generating Functions, Recurrent Events, Random Walk Models, Markov Chains, Discrete Branching Processes, Markov Processes in Continuous Time, Homogeneous Birth and Death Processes, Some Non-homogeneous Processes, Multidimensional Processes, Queueing Processes, Epidemic Processes, Competition and Predation, Diffusion Processes, Approximations to Stochastic Processes, Some Non-Markovian Processes.

The style of the book is heuristic and discursive rather than rigorous and systematic. Researchers seeking a model they might fit to their own data will find the book very incomplete. Readers beginning to learn about stochastic processes will find the book a wise investment.—*Emanuel Parzen*

Metalorganic Polymers by K. A. ANDRIANOV; 371 pages; $12.75; John Wiley & Sons, Interscience, 1965.

In their introduction to this translation the editors point out that much of Professor Andrianov's work has been discussed recently in other books but express the hope "that polymer chemists will want to have available this comprehensive and authoritative monograph by Professor Andrianov himself." Such a purpose is laudable, and we are here offered a discussion which presents the background for many of the studies reported from Professor Andrianov's laboratories. It seems to the present reviewer, however, that the title and dust jacket are somewhat misleading insofar as the actual content of the book is concerned. Over two-thirds of it is devoted to a thorough discussion of the siloxanes, polymers about which Professor Andrianov is well qualified to write but hardly "metalorganic polymers" in the usual sense. The metal-containing siloxanes developed by Professor Andrianov which have been so widely publicized are not treated in as great detail. It is interesting to note that approximately one-third of the references are to patents. Nevertheless, much emphasis is placed on paths of reactions and characterization of products.

There are occasional misprints and awkward spots in the translation, but none are misleading if one takes care in

his reading. Several typographical errors were noted in the bibliography. The Russian version appeared in 1962, and fewer than ten per cent of the references are from 1960 or later. It is unfortunate that the translation and publication of this volume took almost three years, particularly since a less polished translation has been available from OTS for over two years.

If one is not overly concerned with timeliness and reads carefully, this book is worthwhile. A subject index would have added much to its usefulness and made it more attractive to the individual interested in specific reactions.—*B. P. Block*

Markov Processes by E. B. DYNKIN, translated by J. Fabius, *et al.*, 2 vols.; 639 pages; $12 each or $24 the set; Academic Press, 1965.

This is a most welcome translation of the Russian original which appeared in 1962. The translation was prepared by a group of mathematicians at the University of California at Berkeley, and has been fully revised by the author. Its smooth reading, allied to the excellence of the printing, contributes to the success of this edition.

The book is devoted to a systematic exposition of the modern theory of Markov processes, which can be thought, intuitively speaking, as phenomenons changing with time according to a certain stochastic rule and admitting the possibility of a complete stop. Also are presented various theories which bear intimate relations with Markov processes and, in most cases, have been developed to provide tools and techniques for the study of these processes. Various connections between Markov processes and classical analysis have been discovered recently, making the study of these processes fruitful to analysis, besides its importance in the theory of probability. Markov processes, known since a long time to have intimate connections with differential equations, have an appreciable record of influence in the development of the theory of elliptic and parabolic differential equations and potential theory. All

these aspects are treated in this book, which does not presuppose previous knowledge of the subject. Acquaintance with the author's monograph "Foundations of the Theory of Markov processes" (Moscow, 1959; German translation by Springer-Verlag, 1961, in this same series, Die Grundlehren.... Band 108; English translation as *Theory of Markov Processes*, Prentice-Hall, 1961) is helpful, but not required. For the convenience of the reader, an Appendix is provided, summarizing results of measure theory, probability theory, and the theory of differential equations, with references to literature mostly in English.

As the author says in the Preface, the seventeen chapters of the book can be grouped in five parts: (i) general theory of homogeneous Markov processes, where Markov processes are defined and its infinitesimal and characteristic operators are studied. (ii) additive functionals and transformations of Markov processes; in this part the theory of stochastic integrals and stochastic integral equations are studied. (iii) where harmonic and superharmonic functions related to a Markov process are studied, as well as certain applications to the theory of differential equations and probabilistic solutions of certain equations are given. (iv) the n-dimensional Wiener process and its transformations. (v) one-dimensional continuous strong Markov processes.

An Introduction, based on the lecture delivered by the author at the International Congress of Mathematicians, Stockholm, 1962, provides a clear and broad picture of the subject. Also are given an Historical-Bibliographical Note, with references to the literature for the results treated in each chapter and for related questions, a final Additional Remarks, referring to areas of the theory of Markov processes not treated in the text, and a Bibliography of 185 titles. All these features make this book, besides being an excellent text, a valuable help for the investigator.—*U. D'Ambrosio.*

Introduction to Paleoecology by R. F. HECKER, translated and edited by M. K. ELIAS & R. C. MOORE; 166 pages; $7.50; American Elsevier Publishing Co., 1965.

In much American geologic literature the term "paleoecology" has encompassed a plethora of sins. Recognition of subdivisions in the environmental history of a fossil organism, clear definition of the subphases, and use of distinct terms to point out historical differences should help crystallize thought on this complex subject. The Russian geologist Hecker, through Professors Elias and Moore, has given all English-speaking geologists a start along this necessary path. For the first time in an English language text or reference book, the terms paleoecology, biostratinomy, and taphonomy are consistently and logically used throughout this work; Professors Elias and Moore have clarified certain definitions in editorial footnotes. The following definitions emerge from the work of Hecker and the writings of A. H. Müller: study of the environmental history of a fossil organism involves three disciplines—paleoecology, biostratinomy, and diagenetic studies ("Fossildiagenese"). *Paleoecology* focuses upon interrelationships which occurred, in the geologic past, between living organisms and biotic and physicochemical elements in their extrinsic environment. *Biostratinomy* explores pre- and synburial interrelations between dead organisms and components of their external environment. *Diagenetic studies* unravel the post-entombment history of organic remains. Biostratinomic and diagenetic studies are combined under the term *taphonomy*. These fields supply data to, and derive information from, the broader spectrum of environmental geology.

The book contains five thought-units divided among seven chapters and various subchapters. A short history of paleoecology emphasizes early contributions of Russian workers. Four subchapters relate paleoecology to other paleobiologic endeavors (taphonomy,

paleobiogeography, and evolutionary studies) and provide the necessary yet burdensome terminology. A subsequent part relates paleoecology to more general geologic problems while pointing out that paleoecology is sterile if not combined with other geologic studies. Two lengthy units describe field methods and the visual presentation of data.

The book sorely needs an editorial preface and certain editorial refinements. In my opinion, all footnotes are attributable to the editors, but this fact is not tangibly evident in the work. Editorial comment within the text (see p. 85) detracts from the verity of the translation.

Dismissing these minor points, this book should occupy an important place in the history of paleoecologic thought. Paleoecologists today are faced with many interesting and pregnant questions: What is paleoecology? What exactly does a paleoecologist do? How are paleoecologic reconstructions made? Are there any natural subdivisions of paleoecologic work? What steps are necessary for the dynamic growth of paleoecologic work? What steps are necessary for the dynamic growth of paleoecology? Hecker's work begins to unravel the working methods of paleoecologists and provides sound terminologic bases for further studies. Professors Elias and Moore, the American Geological Institute, and the National Science Foundation deserve hearty thanks for making this book available in the English language.—*David R. Lawrence*

The Biosynthesis of Steroids, Terpenes & Acetogenins by J. H. RICHARDS & J. B. HENDRICKSON; 416 pages; $18.50 W. A. Benjamin, Inc., 1964.

Certainly one of the most important concepts ever developed during the studies on natural products is the recognition of the isoprene unit by Ruzicka. Subsequent theoretical consideration on the biogenesis of the cyclic triterpenes and sterols by the Zurich group clarified the mysterious mode of formation of these structurally related compounds, and similar theoretical treatment was

successfully applied by one of the present authors to account for the biogenesis of the various simpler cyclic terpenes. In the meantime, Birch revived the earlier suggestion of Collie and formulated the acetate hypothesis to rationalize the biogenesis of another large group of compounds, particularly aromatic ones, whose structures bear little apparent relationship to acetate or its well known derivative, the fatty acids. Both the isoprene rule and the acetate hypothesis have been verified by more recent biochemical and chemical investigations. This treatise consists of a full discussion of the recent development and refinement of these two fruitful concepts, including the key roles of acetyl-coenzyme A, propionyl-coenzyme A, malonyl-coenzyme A, mevalonic acid, isopentenylpyrophosphate and dimethylallylpyrophosphate. It correlates the vast body of knowledge on the structure of natural products, the considerable amount of information in the labelling pattern of these compounds with the recent rapid elucidation of the enzymatic reactions involved. It is indeed a most timely, comprehensive, and authoritative review.—*T. T. Tchen*

Principles of General Thermodynamics by G. N. Hatsopoulos & J. H. Keenan; 814 pages; $15.00; John Wiley & Sons, 1965.

This weighty (2 lb. 12 oz.) tome is as complete an engineering thermodynamics text and reference as one could hope for. All of the usual topics concerning heat engine cycles, gas laws and chemical solutions are thoroughly covered. The *special* features of this book are collected in a second section covering the use of a single postulate (rather than three or four) to deduce classical thermodynamics, the interpretation of "negative temperature," thermodynamics of plasmas, an introduction to statistical mechanics, and a fine explanation of the authors' special area of research in direct thermoelectric energy conversion.

Hatsopoulos and Keenan carefully discuss the work of Szilard, Mandelbrot, *et al.*, to unify statistical mechanics with classical thermodynamics. The authors also present their candidate for the axiomatic development of classical thermodynamics: a single "Law of Stable Equilibrium," i.e., *A system having specified allowed states and an upper bound in volume can reach, from any given state, one, and only one, stable state and leave no net effect on its environment.* The need for such a reformation may be argued pro and con, but the efforts of the authors in this area are most ingenious and will be very stimulating to the teacher and the superior student. The treatment of statistical mechanics of ensembles complements the axiomatic treatment in completelness and clarity.

The careful scholarship, penetrating discussions, numerous problems (with solutions), attention to clarity in symbology and terminology, and an excellent index make this text one which will have wide use for many years.—*Richard C. Levine*

Quantum Physics of Electronics by S. N. Levine; 301 pages; $8.95; The Macmillan Company, 1965.

Professor Levine (not related to the reviewer) has written a lucid and compact survey of Quantum Mechanics and its applications in electronic devices, such as thermionic and field emission, thermoelectric energy conversion, masers, and lasers. Directed, as it is, toward the applied physicist and electrical engineer, it will certainly find wide use as a senior honors course or graduate level textbook. The reader must possess a good background in Quantum Mechanics, since the first two chapters of the book are very concise and serve mainly as a review. Dirac bracket notation is introduced, but it is used very sparingly, or with alternate notations, in the remainder of the text. Students familiar with the usual Schrödinger notation can use most of the book without difficulty.

A concise derivation of thermal energy distribution functions by the Lagrange multiplier method leads to electron theory of metals and thermionic and field emission. The one dimensional Kronig-Penny periodic square-well potential problem is solved to exhibit the energy bands in a semiconductor. Doped

semiconductors, thermoelectric effects and Hall effect devices are also treated. Spontaneous and induced energy level transitions and the maser and laser are covered in fair depth in the final chapter.

An amazing number of topics are treated quite clearly in the relatively few pages. This can be partially explained by the author's laudable preference for the short, easily grasped example over the tedious belaboring of a topic in all its minute detail; e.g., the brief treatments of perturbation theory, energy bonds in solids, and spontaneous transitions. However, in using some of the sections, notably thermionic emission, crystal structure, irreversible thermodynamics, and the problems provided after each chapter, the teacher may feel the need to supplement this text with outside reading.

One personal reservation of the reviewer is very minor. He would like to eliminate the solid *polar* plots of probability density which are so often used to represent atomic orbitals (in this book, p. 53). These pictures are usually confused in the students' minds with the true *spatial* density, as shown by, e.g., H. E. White, *Introduction to Atomic Spectra*, pp. 71 and 146.—*Richard C. Levine*

The Solar System & the Constellations by C. N. Anderson; 312 pages; $8.50; Vantage Press, 1964.

This large volume is a book for the amateur astronomer containing much interesting information about the constellations and a miscellaneous collection of other astronomical facts. A half page or so describes each constellation, giving its mythological origin, the meanings of the names of the brightest stars, and interesting facts about the variable stars, clusters, and nebulae that can be found there. Many fine photographs illustrate several of these objects, and perhaps the book is worth buying for the pictures alone. However, the amateur should not be misled into thinking that his telescope will show the same magnificent views. The book will not be much help in finding the constellations in the sky, for no information is given on how to use the star charts, and in fact there is

not even a key giving the magnitudes associated with the various size star symbols. The book is thus not very valuable as an atlas.

There are also sections describing the objects in the solar system; a moon map with an index of 502 surface features giving their location, a brief description, and the origin of the name; a somewhat incomplete biographical list of astronomers up to the time of Barnard; and a few pages on stellar spectra and variable stars. It seems strange that only these two topics should be included out of the galactic astronomy. Some of the text sections require a fair amount of previous knowledge, but the constellation descriptions are perfectly readable by anyone.—*John E. Gaustad*

Conodonts by M. LINDSTRÖM; 196 pages; $10; Elsevier Publishing Company, 1964.

The paradox of this book is that it has been long overdue; yet publication of a book on conodonts a few years ago would have been presumptuous. For no other group of microfossils has taxed the imagination of micropaleontologists more than these tooth-like calcium phosphatic forms, yet few have yielded their secrets so unwillingly. According to the author, up to 1960 there were about 1100 papers published dealing with conodonts. This is the first book that presents the results of research on this stratigraphically important group of fossils in a systematic manner.

The information is presented in 11 well-organized chapters in which the author—a Swede—displays a remarkable facility with the English language. The first six chapters are primarily descriptive and deal with the history of research, morphology, and structure, faunal succession, occurrence, and natural assemblages. The remaining five chapters are largely analytical.

In chapter 10 Lindström deals with the nature of these controversial fossils. He reviews the fish and annelid hypotheses and rejects them as unsatisfactory. He also discusses and rejects the radula, copulating spicule, arthropod, algal and *Archaeognathus* hypotheses as inadequate. He agrees that conodonts were embedded in tissues of some sort; they were internal and not external in position. The following points are made: 1) conodonts were free-swimming, not benthonic; 2) the animal was bilaterally symmetrical for the most part; some forms may have been asymmetric, however. The question of complete asymmetry in some instances raises the question as to whether all conodont bearers swam about actively or whether some might not have floated passively in colonies; 3) passive protection was probably not their main function; 4) the morphology of conodonts was probably directly related to that of the supported organs. Lindström attempts to reconstruct a conodont as a unit serving to keep the food-gathering mechanism extended. The organism is conceived of as a plankton feeder, tentacled, ciliated, and somewhat similar to a lophophore. The greater part of the animal was soft organic tissue; in the frontal part an opening (leading inward to a mouth which in turn opened into an alimentary canal) to the interior allowed water to circulate to and from the feeding apparatus.

This book presents an authoritative and highly readable account of the enigmatic conodonts whose zoological affinities are still the subject of considerable controversy. For those who try to present a balanced course in micropaleontology and those engaged in research or applied studies on conodonts this book will fulfill a long felt need.—*W. A. Berggren*

Biological Rhythm Research by A. SOLLBERGER; 461 pages; $25; American Elsevier Publishing Co., 1965.

This book is not a critical essay on biological rhythm research, but is rather the first encyclopedia of the field. There can be little room for critical evaluation when mention is made of practically every aspect of the biological and non-biological sciences where the words "oscillation," "periodic" and "rhythmic" are used. In this connection, the bibliography lists 3200 references, and

comprises 25% of the book. Up to 240 references per page are given, and the average is about 30.

The logic behind this bibliographic approach is suggested by Dr. Sollberger's "distress" at the "...number of theoretical models which have been advanced to explain the behavior of biological rhythms". He continues, "Actually, we know yet much too little about the biological rhythm for excessive model making. What is needed is more experiments and more methods of analysis of experimental data". Many biologists would take issue with this view of experimental science. For it is surely through models, not numbers and letters, that the experimentalist should organize what is known, and choose from the infinite number of possible experiments those which test his conception of how a biological mechanism works.

In spite of the diversity of subjects mentioned, the organization of the text is clear, each chapter being subdivided into numbered and titled paragraphs which are listed in the table of contents. One can immediately locate some area of interest, and find a more or less topical account with references. This use alone may justify the price.—*William F. Zimmerman*

Regional Geomorphology of the United States by W. D. Thornbury; 609 pages; $14.75; John Wiley & Sons, 1965.

In the quarter of a century since the last book on physiography of the United States appeared, our understanding of the country has increased notably. This book covers in one volume the area that Fenneman covered in two, and adds Alaska, Hawaii, and the Continental Shelf. There is no attempt to change regional divisions nor basic philosophy. The author is concerned mainly in bringing the teaching of regional geomorphology up-to-date.

Thornbury follows Fenneman in his orderly approach from east coast to west coast, but the style of writing is completely different. Thornbury gives general physiographic coverage to each

area in non-technical style, before presenting the geomorphology. Where various investigators have come to different conclusions, Thornbury is likely to review all of the more important opinions, even to the extent of tabulating conflicting findings. He is scrupulous in trying to present opposing views without prejudice. The result is an interesting text, covering what is known, and leading the student toward problems still to be solved.

The illustrations are excellent. There are abundant sketches, physiographic diagrams, detail maps, and photographs. Especially good are the airplane photos.

Thornbury sticks to his subject with laudable single-mindedness. Neither climate nor vegetation nor the works of man are allowed to divert attention from the land itself.

This book should replace all previous texts in physiography or geomorphology of the United States.—*Winnifred V. Fischer*

Function & Structure in Micro-organism, edited by M. R. Pollock & M. H. Richmond; 405 pages; $13.50. Cambridge University Press, 1965.

This volume contains the papers of the Fifteenth Symposium of the Society for General Microbiology held at the Middlesex Hospital, London, in April 1965. Like its predecessors, it not only updates the work in a specified area but offers interpretations of the results by leaders in each field. This symposium is concerned with function and structure in micro-organisms and attempts to answer questions about the correlation of their intricate structures with the functions they perform, the extent to which these relations can be interpreted in physical and chemical terms, and the mechanism by which the component parts are co-ordinated to form the whole. In his introduction, the late Donald Woods aptly compares the work of Antony van Leeuwenhoek with that of Sir Christopher Wren and the work of the biochemical microbiologist with that of the architect.

The book is divided into four parts: The Supply of Energy and Metabolites, with papers by Kornberg, Lascelles, Britten, Holter, and Lampen; the Coordinated Biosynthesis of Macromolecules, with papers by McQuillen, Stacey, and Rogers; Processes and Organs of Locomotion, with papers by Newton and Kerridge, Burge and Holwill, and Wolpert; and Differentiation in Development, Heredity and Survival, with papers by Hayes, Sager, Halvorson, Fitz-James, and Mazia. These names, familiar to microbiologists, are the assurance of the quality of each chapter which is amply documented with references through 1964. Even though the reader may not agree with all the conclusions (and of what use would be the book if he did?) the discussions are logical and thought-provoking.

This book is well-written, interesting, and informative. The plates are well-reproduced but, following the format of the *Journal of General Microbiology,* they are inserted at the chapter ends and the explanations are grouped together

on a separate page. This arrangement is inconvenient and requires considerable page-turning and back-tracking.

As a discipline, microbiology is in a sort of twilight zone. It impinges on other fields and is not sharply defined. Nevertheless, substantial progress has been made and the next decade promises even greater advances. These symposia are making vital contributions and this volume is a worthy companion to its predecessors. It deserves a prominent spot on the bookshelf of the thinking microbiologist.—*David B. Sabine*

Friedel-Crafts & Related Reactions, Vol. III, Parts I & II: *Acylation & Related Reactions*, edited by G. A. OLAH; 1606 pages; $60, the set of 2 parts; John Wiley & Sons, Interscience, 1964.

This volume of 1600 pages provides sixteen chapters by nineteen authors. It contains a vast amount of information, much of it conveniently presented in tables which, with their literature references, provide ready access to what has been done; an invaluable contribution. Unfortunately, the volume gives insufficient warning that, since many of the reactions were effected in heterogeneous systems, reproducibility is uncertain. Most of the preparative work was not directed in the light of the present incomplete understanding of the role of reaction conditions in determining products and yields: conditions more appropriate to required results could now be applied to many of the reactions reported in the literature. All too frequently, carbon bisulphide, carbon tetrachloride, chloroform, hexane and suchlike compounds are referred to as solvents when, for most reaction mixtures in Friedel-Crafts acylation, they are not. Thus acylation *in* boiling carbon bisulphide usually means reaction was effected in a heterogeneous sludge at the temperature of the heating bath and *under* boiling carbon bisulphide. The importance of such considerations is illustrated by the caution given on line 10 of p. 1335: it can be withdrawn, for Holt and Pagdin used a homogeneous reaction mixture at room temperature and in the absence of an excess of

aluminum chloride, whereas Truce and co-workers used an excess of this reagent and the boiling carbon bisulphide technique which, not surprisingly, gave a different result.

The chapters have not been written with equal authority and critical appreciation. Compare, for example, the comment on p. 1053 and lack of comment on p. 1289 concerning the claim that interaction of phosgene and propene gives β-chloroisobutyryl chloride. Different authors sometimes offer different interpretations of the same reaction leaving the reader to choose wisely. Fortunately, the right choice is usually easy to make; for example, the contents of p. 1175 will not be preferred to those of p. 1086 and the formulated mechanism in the middle of p. 401 will be preferred to that, by the same author, in the middle of p. 393. The volume contains numerous minor errors which can be corrected by the careful reader; for example, he will probably reorient the bent arrows of formula (LIII) on p. 1097. On the other hand, errors of fact, for example, "Symmetrical tetrachloroethane forms a soluble complex with aluminum chloride" (p. 547, 1. 16) may be more difficult to recognize. Most of the volume is sufficiently well written and free from error to be readily understood; there are passages, however, which are difficult to comprehend: in illustration, the paragraph occupying the middle of p. 1568 provides an amusing problem in interpretation.

This volume, *Acylation and Related Reactions*, includes aldehyde syntheses, sulphonylation, sulphonation, nitration, amination, perchlorylation and halogenation. More surprisingly, it is concerned also with the interaction of carbon monoxide and alkylating agent (see Chap. 39) and of hydrogen halide and olefin (see Chap. 46). The index at the end of the volume is useful.—*G. Baddeley*

The Rational Use of Dyes in Biology & General Staining Methods by E. Gurr; 422 pages; $14.00; Williams & Wilkins Co., 1965.

Much of the first 130 pages is devoted to theoretical considerations leading to the presentation of a new scheme for the classification of the biologically important dyes now known. Heretofore, classification has been based on the configuration of the chromophoric group. It has, however, been universally recognized that dyes are either non-ionic (neutral), cationic (basic) or anionic (acidic). Dr. Gurr proposes that, as far as biological staining is concerned, one chromophore is essentially equivalent to another but that the ionic charge of the whole chromophore, the character of its various reactive side chains, and its molecular weight are the important factors which determine its staining properties. Knowing these facts about dyes should make it possible to predict with reasonable accuracy their staining reactions and interactions and thus to devise new staining methods on a rational rather than an empirical basis. In each of the 14 classes of dyes which he presents the dyes are listed in order of increasing molecular weight, but the basis of classification is the character of the side chains. Thus, in the non-ionic group there are three classes; those with acidic, those with basic, and those devoid of both acidic and basic side chains. In the cationic group there are two classes; those with only basic side chains and those with acidic side chains, as well. The anionic group, which comprises by far the greatest number of dyes, is divided into three sub-groups. The wholly acidic sub-group contains five classes on the basis of the presence of carboxyl and/or sulphate groups in combination with hydroxyl groups. The two amphoteric sub-groups have two classes each depending on the presence or absence of hydroxyl groups. The reviewer found this portion of the book informative and stimulating. It is believed that Gurr's classification will prove to be most useful to biologists in devising new staining procedures.

Unfortunately, the practical portion of the book probably will not prove as useful as the theoretical. The present volume is called a companion to a previous work by the same author (*Staining Animal Tissues, Practical &*

Theoretical). The earlier work presented practical staining methods for the animal histologist, whereas the present one provides an impressive number of up-to-date staining methods applicable to general cytology, hematology, microbiology, and botany. Each of these books is purported to be complete in itself. The present one is not entirely successful in this respect inasmuch as there are at least a dozen entries among the various methods that are so incomplete as to concentration and/or solvent that one must guess or refer to some other source. The eight pages devoted to specimen syntheses of dyes would have been better occupied by completing the recipes—I believe that almost all biologists will leave the synthesis of dyes to dye chemists. The addition of page cross references in the staining methods would make some of them much easier to use. In the preface, Dr. Gurr states that, "...apart from the Publishers, no one but myself has seen the whole of this book prior to publication." Perhaps if someone knowledgeable about staining procedures had seen it before publication the practical part would be as useful as I believe the theoretical part will prove to be.—*Roy R. Peterson*

Adaptive Growth by R. J. Goss; 360 pages; $12.00; Academic Press (Logos), 1964.

In this book, Professor Goss concentrates on one of the most important problems in animal growth—the mechanism which regulates organ-size. "Adaptive" is to be interpreted in the physiological, proximate sense, in fact, rather than in the evolutionary, ultimate sense. The author's thesis is that after early embryogenesis, size-regulation is closely bound up with physiological adjustments,—the classical "functional demand" theory.

Without completely rejecting alternative theories Goss marshals a wealth of research-evidence, including much from his own laboratory. He has done a valuable service in collating work from diverse morphogenetic approaches, usually all too much segregated. The results are mainly from work on mammals and some other vertebrates, however, for the invertebrates have been relatively little studied; but the general zoologist, particularly, will welcome the very extensive bibliography on the higher vertebrates. The material is assessed with the analytical acuity and attack-thinking which characterizes the author's own experiments. It is presented with a scrupulous clarity of illustration and text which makes the book eminently readable, even through the heavier narrative sections and the complexities of organ-interactions.

It could be contended, constructively, that the focal, theoretical Chapter III should have come after, and not before, the body of facts on which it is based and which may seem tedious once the reader has been primed to expect that the limelight thenceforward is on the author's thesis. For the purpose of stage-setting, the preface and introduction would seem already adequate. Allometry would then not be isolated from the other lines of approach. The unity of theme and development would probably also have gained from less weight on physiological mechanisms and other peripheral, if essential, material.

Professor Goss has made a very good, freshened, and strengthened case that functional deficit/excess is a major factor promoting morphological hypertrophy/atrophy. The nature and complexity of the pathway of intermediation is now the immediate problem. The possibility of other factors and mechanisms seems still in court.—*A. E. Needham*

Fundamentals of Engineering Mechanics by L. Levinson; translated from the Russian; 334 pages; $10.50; Gordon & Breach, 1965.

Profound concern regarding the quality of American Engineering Education has resulted in considerable activity, including the production of numerous outstanding undergraduate texts. Unfortunately, this book does not fall into this category, and gives rise to immediate speculation as to why it was selected for translation. It surely is not representative of the outstanding con-

tributions of many Russians in the area of solid mechanics.

The author has attempted to cover an extremely broad area including statics, kinematics, kinetics, the theory of machines, and strength of materials. This is done without calculus so the text would not be suitable for courses in Engineering Mechanics as they are known in most American universities. Possibilities of using the text in an introductory Physics course (occasionally done without calculus) would most likely be ruled out, because of the generally awkward presentation, lack of good quality figures and illustrations, and the omission of an index.

A more serious criticism is the use of the book as a propaganda vehicle. After an extremely brief mention of Archimedes, Leonardo, Stevinus, Galilei and Newton, the reader is definitely led to believe that most advances in analytical mechanics and "machine engineering" are of Russian origin. In fact, this argument is supported by a direct quotation from Lenin. The latter is so patently out of place as to raise serious questions regarding the responsibility of the publisher.—*Robert W. Gerstner*

Clinical Phonocardiography by D. C. Deuchar; 144 pages; $3.75; D. Van Nostrand Co., 1965.

This is an admirable little book. The author set himself the task of providing a short text on clinical phonocardiography for the student and general practitioner, which he has achieved. It is clear, concise, and comprehensive. Of great value too is the author's adherence to his promise that matters not the province of such a slim volume shall not be included The whole subject is dealt with in an orderly fashion: instruments and techniques, physical basis of acoustic phenomena, both normal and abnormal, then a consideration of each important pattern in turn. Each pattern is interpreted in terms of the hemodynamic circumstances responsible for its origin. Such an approach, in which laboratory and research data are transported to the bedside, is in keeping with the best traditions of British Clinical Cardiology. Where the interpretation is uncertain or unknown the author says so, and, in the case of multiple interpretations, he presents the alternatives fairly. The line drawings, which are happily profuse, meet the standard of the text. For the individual planning the first brave plunge or for him reentering for a refresher, it would not be bad advice to let Dr. Deuchar initiate him into "the mysteries of phonocardiography."—*David H. Lewis*

Introduction to Semiconductor Devices by M. J. Morant; 126 pages; $2.95; Addison-Wesley Publishing Company (A-W Series in Electrical Engineering); 1965.

This monograph is offered as "an introduction to the physics of semiconductor devices . . . at the advanced undergraduate level." Its aim is an understanding of the operation of these devices "in a moderate amount of detail in order to take full advantage of their characteristics and to be aware of their limitations." The author has successfully accomplished his purpose.

The first two chapters provide an introduction to the physics of electrons and holes in semiconducting solids. The opening sections may prove too concise for "engineers who may have had little or no exposure to the fundamentals of semiconductor devices," but, in general, these chapters will contribute to a clear understanding of the subject. The topic of *p-n* junctions is discussed in detail in chapter 3, which leads immediately to the consideration of the junction transistor in chapter 4, and of its use as a circuit element in chapter 5. A too concise review of "miscellaneous semiconductor devices" constitutes the final chapter. The value of this chapter, in particular, would have been enhanced by a wider selection of specific references for further reading.

The author's style is basically descriptive, and equations are often presented in "approximate" form. Nevertheless, the physics of semiconductors is reliably and adequately communicated. The coverage of both the theoretical and practical aspects of semiconducting devices is good. The discussions of the correlation and/or discrepancy be-

tween theory and application, and the illustrations of the relative importance of different parameters contribute much to the general excellence. Significant numerical data, important manufacture and application aspects, current terminology and recent developments are reviewed and briefly discussed. The inclusion of thirty-six problems, with solutions, throughout the text contributes to the potential value of this monograph as a readable and worthwhile introduction to semiconductor devices.—*Robert F. O'Brien*

Handbook of Paleontological Techniques, edited by B. KUMMEL & D. RAUP; 852 pages; $18.00; W. H. Freeman & Co., 1965.

A basic fact of paleontology, so taken for granted by practitioners that scientists in other fields may be unaware of it, is that most fossils require extensive technical manipulation before they can be studied. Differences in the nature of various fossil organisms are vast, as are differences in their occurrence and mode of preservation; the questions posed by today's investigators are far more subtle and penetrating than those of the old-time describers of species; and the ingenuity of technicians in finding ways to answer these questions seems to be limitless. As a result, investigative techniques have been developed in almost infinite diversity—but these are commonly buried in the "Techniques" sections of specialized papers where they reach only a limited audience.

Compiling an orderly and comprehensive cookbook of these multifarious methods and procedures must have rivaled the labors of Hercules, but Kummel and Raup have done it. Their book consists of a series of concise descriptive articles written by specialists, plus an extensive bibliography of techniques arranged by subject, plus a classified index of the bibliographies used by paleontologists and stratigraphers. The latter two sections alone are worth the price.

Just to list the 86 descriptive essays would treble the length of this review. Section by section they cover general practices in the study of each of the major fossil groups, collecting techniques, mechanical and chemical preparation, radiation-based methods, casting and molding, and the preparation of illustrations. A separate chapter treats techniques in palynology.

To call this book a goldmine of information would be unfair, for a goldmine involves laborious digging and the tedious extraction of values from dross. Instead, Kummel and Raup offer a treasury of fresh-minted data to paleontologists of all persuasions, as well as many things of value to neontologists, stratigraphers, archeologists, and others. We are much in their debt.—*Donald Baird*

Human Diversity (The Nature & Significance of Differences among Men) by K. MATHER; 126 pages; $5.95; The Macmillan Co., Free Press & Oliver & Boyd, 1965.

In this book which carries the subtitle "The Nature and Significance of Differences among Men" the British geneticist Kenneth Mather addresses himself to a wider public—"to all who are seeking a better understanding of human differences and their importance for our populations and societies."

The first five chapters which make up about 60 per cent of the slender volume deal with some facts about diversity, its environmental and genetic causation, with mutation and selection and with the wide-spread existence of polymorphisms in Man with particular reference to blood characters. Contrary to a first impression according to which natural selection will usually lead to the establishment of a single, fittest genotype for any one trait, it is now clear that a variety of alternate traits coexist in any population. How this is brought about is one of the great problems of present research. Mather's treatment is clear and fair even if it is difficult to be original in these widely discussed areas.

With its sixth chapter "Continuous variation," the book rises to an impressive level. Mather is an authority on polygenic systems, those in which the existence of many different genes, all

contributing to a quantitatively measurable trait, account for continuous variation. Without using the tools of the mathematical geneticist, he describes lucidly the basic facts with special reference to the very large amount of potential genetic variation which remains concealed in a population by means of the balancing action of non-allelic genes. Thus two genotypes, AAbb and aaBB, may be alike in expression but their second generation offspring may contain five observably different types, from AABB to aabb. "So," he concludes, "great as may seem the continuous variation that we observe in stature or intelligence. . .it is no more than a token of the genetic variation that we actually carry in the population. . ." and he adds that "our experience with other species suggests that . . . intense selective breeding would take no more than a dozen or so generations to double or halve either the average stature or the average level of I.Q . . . !" Notwithstanding such an unequivocal statement, the treatment of the genetics of intelligence in populations is open-minded and stresses the need for much more knowledge. It also warns of complacency: "The adjustment of the genetical constitution of the population by deliberate intervention . . . is generally regarded as something that we can choose either to undertake or to leave alone, but in truth we must already be in the business of altering our genetical structure. What we are doing to it we cannot as yet see clearly."

The two final chapters go beyond the biological factors common to all organisms and deal with man's unique attributes, "exosomatic evolution," social transmission, social evolution, and the interplay of genetical differences and social development. Here the sober and restrained tone of the volume yields at times to an eloquence which is impressively compatible with the author's objective attitudes.—*Curt Stern*

Exploration of the Universe by G. Abell; 646 pages; $9.50; Holt, Rinehart & Winston, 1964.

This book has sat on my desk for an entire year while I have been teaching an introductory course in astronomy, and my constant reference to it for lecture ideas and illustrations has convinced me that it is the finest university textbook on elementary astronomy yet written. Dr. Abell writes in a remarkably readable style. His explanations on every topic are clear and convincing, and he has chosen well the diagrams and photographs which appear on almost every page. The thirty-two chapters contain probably more material than can be taught in a year to an average group of students, but the more advanced topics are set off in smaller type, and can be omitted without loss of continuity. Their presence, however, enhances the value of the book as a reference, and makes it a challenge for even the best student. The author says that the book was written for the university student who is not majoring in a physical science, which explains the absence (except in footnotes) of logarithms and any mathematics beyond elementary algebra, but the general level of discussion seems well above that of the typical "science for the non-scientist" book.

It is pleasing that the most modern topics are given their due amount of space. The two chapters on stellar evolution and stellar structure (36 pages) are particularly good. The latter describes the basic physical considerations that determine the structure of a star in an extremely clear way, without actually writing down the differential equations involved.

The book contains 16 useful appendices, including the entire Messier Catalogue, a good glossary, the complete list of constellations, and a table of physical and astronomical constants. The star maps, printed on small cards inserted in a back cover pocket, are designed for use at mid-northern latitudes, and hence are unfortunately useless at this reviewer's equatorial university. The many questions and exercises at the end of each chapter are well designed to make the student (and occasionally the instructor) think about what he has read instead of just memorizing it.

There is very little that I find to criticize, but I do feel that the historical

method of introduction is a wrong approach, particularly for students who know nothing about the subject previously. Several of my students were initially confused as to whether the stars were really set on crystal spheres or not! The only major technical error I noticed was the explanation of the presence of forbidden lines in gaseous nebulae, which are erroneously stated to become stronger with decreasing density.

The book, of course, will be dated as belonging to the prequasar era, but such is the exciting ever-changing state of astronomy today.—*John E. Gaustad*

Astronomy, edited by S. RAPPORT & H. WRIGHT; 354 pages; $4.95; New York University Press, 1964.

This is one volume of the New York University Library of Science, a series, according to the jacket, "designed for the general reader, spanning the whole range of the natural and physical sciences." This is a tall order, and the twenty-three selections in this anthology do not by any means span the whole range of the subject of astronomy. Yet there are samplings from many areas, historical and modern, none of which are written at a level higher than the typical *Scientific American* article. Each article is preceded by a page or so of well-written background information, designed to put the subject in context.

There are some real gems in this book, such as the chapter from Eddington's *Space, Time and Gravitation*, describing his eclipse expedition of 1919, in which the bending of light passing near the sun was first observed. His own group in West Africa had poor weather and obtained only two measurable photographs, but each of these showed a displacement of the stars in agreement with Einstein's prediction. Imagine his dismay when the first set of plates returned by the expedition to Brazil showed half the displacement, in agreement with the Newtonian predictions. But the images in this set were not good, and there were certain possibilities of systematic error. Fortunately, another set of observations was obtained in Brazil with another telescope of a larger scale and these plates had ideal images. Their measurement confirmed the Einstein value, setting the theoretician's mind at ease.

Other interesting chapters include Galileo's account of the first observations with his telescope, the article by the Schwarzschilds on balloon astronomy, a history of the circumstances surrounding the discovery of Neptune by Sir Harold Spencer Jones, and a selection entitled "The Birth and Death of a Star" by Allan Sandage. I would have thought that a more up-to-date description of the solar system could have been found than the thirty-year-old chapter by Moulton included here, but this author does write in an engaging manner.

This is not by any means a text or reference work. It is recommended for the layman who wants to dabble in some interesting aspects of the science of astronomy.—*John E. Gaustad*

Exploration of the Universe by H. C. KING; 335 pages; $0.75; The New American Library (Signet Science Library original), 1964.

Although this little paperback bears the same title as the recent text by G. Abell, it is not an exposition of modern astronomy for students, but a very readable history of the development of man's ideas about the universe for the general reader. Dr. King guides us on the fascinating journey from the beginnings of recorded history in Egypt and Babylonia to the modern events of man's first faltering steps away from his planet into space. The narrative is not strictly chronological, but is informative and thorough without being pedantic.

I was pleased to see the number of pages devoted to the Muslim astronomers, whom we can thank for keeping astronomy alive during the Dark Ages, and I enjoyed very much the account of the seventeenth and eighteenth century speculations about life elsewhere in the universe, a subject which has experienced quite a revival recently, but on more scientific grounds.

Fifty pages cannot really do justice to the vast expansion of knowledge in the present century, but the last chapter touches on most of the important points, from the discovery of the energy source of the stars to the realization of the vastness of the universe and the expansion of the galaxies. Here however, a few technical errors creep in, such as the confusion of the stellar interferometer in Australia with radio instruments which operate on an entirely different principle.

Twenty pages at the back of the book contain a reasonable glossary, but the index contains chiefly proper names. Sixteen standard photographs bound in the middle of the volume have little connection with the text. The flyleaf states that a hard cover edition is published in Britain by Martin Secker & Warburg.—*John E. Gaustad*

Meteors, Comets & Meteorites by G. S. HAWKINS; 134 pages; $2.50 paper; McGraw-Hill Book Company, 1964.

This small book is the first of a new soft cover series in astronomy intended for the junior or senior student in physical science. Such a series forms a welcome addition to the literature, for there is relatively little published at the intermediate level between the elementary survey and the graduate textbook.

Six chapters in this book are devoted to meteors, including methods of observation, the physics of the meteor interaction with the atmosphere, the phenomenon of swarms and streams and their relation to comet orbits, sporadic meteors, and the supposed effects of meteor dust on the weather. Only 16 pages are devoted to meteorites and 23 to comets, which shows, I think, where the author's main interest lies, not where the most interesting and extensive material is to be found. A few review questions and problems are given at the back of the book, and there is a small index. No references are included.

Although there is a great need for a series of this type, I do not think that this first number adequately fulfills the needs of the intermediate student at which it is aimed. The main criticism is that the author does not maintain an even level throughout. Many equations are given with little or no derivation, apparently on the assumption that the student can derive them himself, yet in other places oversimplified explanations are used. For example, the resolving power of a telescope is given in terms of the separation of two objects at 100 km distance. The author's writing style jumps from topic to topic rather abruptly, and the inclusion of too many irrelevant details and personal references is rather annoying.

Apparently there is still confusion of terminology in meteor astronomy, for according to the author, a micrometeorite is defined as an object that completely melts upon entering the atmosphere but does not vaporize, yet he shows photographs of micrometeorites which obviously have never melted. Again, if a meteorite is an object which has survived to reach the surface of the earth, as it is defined here, then one can not talk of a meteorite which is consumed in the atmosphere, yet this is how those meteors showing nickel-iron spectra are described. Such inconsistencies also detract from the readability of the book.

In spite of these drawbacks, this book will be useful to some students, for there is little else yet published on this subject. However, it is to be hoped that future contributions to this series can be of higher quality.—*John E. Gaustad*

Surface Phenomena in Metallurgical Processes, edited by A. L. BELYAEV; 228 pages; $27.50; Consultants Bureau, 1965.

The 29 papers in this volume were presented at a conference in Moscow in 1961 and published in the original version (Russian) in 1963. Perhaps 4 of them may be classified as frankly review articles, about 10 are mainly reports on experiments; there are also purely theoretical papers and some which are partly theoretical, partly experimental, and partly reviews.

No direct reference to metallurgical processes could be found, e.g., in the

article on the method of the maximum bubble pressure by Pugachevich (not Pugachev as stated on p. 152) or in those by Shcherbakov on the capillary effects of the second kind. However, the major part of the text really discusses simple steps in some metallurgical operations from the point of view of the physical chemistry of interfaces. Thus, the entrapment of aluminum droplets in cryolite melts during the electrodeposition of aluminum is correlated with the tension of the interface aluminum—cryolite (both liquid); wetting of graphite by molten chlorides and fluorides with cathodic and anodic polarization is studied in detail (29 pages) because graphite cathodes are used in the above electrodeposition, and so on.

It would be unfair to expect a very critical attitude to scientific theories from people dealing with applications of science; and, indeed, some authors are more credulous than the reviewer prefers to be. The translation is not everywhere smooth; Volmer appears again as Folmer, etc. On the whole, however, the volume should be useful to both budding and mature metallurgists, especially of the more practical type. Unfortunately, the price of the book is excessive.—*J. J. Bikerman*

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Cosmic Rays (B. Rossi); Sept., 380A.
Cotton, F. A., ed.; Sept., 368A.
Craigie's Neuroanatomy of the Rat (W. Zeman & J. R. M. Innes, revisers); March, 87A.
Crandall, L. S.; March, 92A.
Crisis in Medical Education, The (L. J. Evans); June, 259A.
Critchley, G. N., ed.; Dec., 492A.
Cronquist, A.; Sept., 372A (with H. A. Gleason).
Cummins, K. W. *et al.*; June, 249A.
Current Interpretation of Wave Mechanics, A Critical Study, The (L. De Broglie); Sept., 384A.
Cytoplasm in Heredity, The (D. Wilkie); March, 126A.

Damask, A. C.; March, 76A (with G. J. Dienes).
Danielli, J. F. *et al.*, eds.; June, 228A.
Darlington, C. D.; June, 200A.
Dasmann, R. F.; Sept., 389A.
David Rittenhouse (B. Hindle); June, 206A.
Davis, D. E.; March, 98A (with F. B. Golley).
Davis, P. H.; June, 212A (with V. H. Heywood).
DeBenedetti, S.; Sept., 380A.
De Broglie, L.; Sept., 384A.

Deep and the Past, The (D. B. Ericson & G. Wollin); March, 131A.
Degens, E. T.; Dec., 490A.
Delcroix, J. L.; March, 78A (with J. F. Denisse).
Denisse, J. F.; March, 78A (with J. L. Delcroix).
DeRienzo, P.; Sept., 398A.
Design of the Universe (in *Astronomy Highlights*) (S. I. Gale); Sept., 330A.
Desmonde, W. H.; March, 137A.
Deuchar, D. C.; Dec., 508A.
Development of Weak Interaction Theory, The (P. K. Kabir, ed.); Sept., 339A.
Development: Selected Topics (L. J. Barth); March, 94A.
DeVirville, A. D., ed.; March, 120A (with J. Feldmann).
De Vore, I.; Sept., 376A (with S. Eimerl and the Editors of LIFE).
De Wiest, R. J. M.; Dec., 494A.
Dickinson, S. K., Jr.; Dec., 488A.
Dienes, G. J.; March, 76A (with A. C. Damask).
Differentiation & Development (N. Y. Heart Association); June, 242A.
Discrete Maximum Principle, The; A Study of Multistage Systems Optimization (L.-T. Fan & C.-S. Wang); March, 130A.
Dislocations (J. Friedel); June, 253A.
Ditchburn, R. W., ed.; Dec., 466A.
Dondero, N. C., ed.; Sept., 356A (with H. Heukelekian).
Drell, S. D.; March, 106A (with J. D. Bjorken).
Dwyer, F. P., ed.; June, 210A (with D. P. Mellor).
Dynkin, E. B.; Dec., 497A (trans. by J. Fabius *et al.*)

Eardley, A. J.; Sept., 328A.
Early Growth of Logic in the Child, The (B. Inhelder & J. Piaget); March, 128A.
Eaves, G.; June, 251A.
Editors of LIFE; Sept., 376A (with S. Eimerl & I. De Vore).
Education of Vision (*Vision and Value series* Vol. I) (G. Kepes, ed.); Sept., 392A.
Eiduson, S. *et al.*; March, 134A.
Eimerl, S.; Sept., 376A (with I. De Vore and the Editors of LIFE).
Electromagnetics in Space (K. R. Spangenberg, ed.); Sept., 397A.
Electron Paramagnetic Resonance (S. A. Al'tshuler & B. M. Kozyrev); June, 221A.
Electronic Analog & Hybrid Computers (G. A. Korn & T. M. Korn); June, 253A.
Electronic Spectra & Quantum Chemistry (C. Sandorfy); June, 223A.
Elementary Coordination Chemistry (M. M. Jones); Sept., 383A.
Elements of Cloud Physics (H. R. Byers); Dec., 492A.
Elements of Cytology (N. S. Cohn); Sept., 355A.
Elements of Stochastic Processes with Applications to the Natural Sciences, The (N. T. J. Bailey); Dec., 496A.
Elving, P. J., ed.; March, 130A (with I. M. Kolthoff).
Embryology of the ovary & testis Homo sapiens & Macaca mulatta (G. van Wagenen & M. E. Simpson); Sept., 399A.
Emeleus, H. J., ed.; March, 113A (with A. G. Sharpe).
Environment & Archeology: An Introduction to Pleistocene Geography (K. W. Butzer); Sept., 394A.
Ericson, D. B.; March, 131A (with G. Wollin).
Essays 1958/1962 on Atomic Physics and Human Knowledge (N. Bohr); June, 208A.
Essays on Crop Plant Evolution (J. Hutchinson, ed.); Dec., 464A.
Essays on Physiological Evolution—honoring KH. S. Koshtoyants (J. W. S. Pringle & T. M. Turpayev, eds.); Dec., 466A.
Evans, L. J.; June, 259A.
Evolution of Biology, The (M. J. Sirks & C. Zirkle); March, 109A.
Experimental Endocrinology (M. X. Zarrow *et al.*); June, 224A.
Experimental Entomology (K. W. Cummins *et al.*); June, 249A.
Experimental Methods in Gas Reactions (H. Melville & B. G. Gowenlock); Sept., 339A.
Exploration of the Universe (G. Abell); Dec., 510A.
Exploration of the Universe (H. C. King); Dec., 511A.
Extrachromosomal Inheritance (J. L. Jinks); March, 125A.

Fabric & Mineral Analysis of Soils (R. Brewer); June, 244A.
Fan, L.-T.; March, 130A (with C.-S. Wang).
Federalist, The; March, 140A.
Feldmann, J., ed.; March, 120A (with A. D. DeVirville).
Fenn, W. O., ed.; March, 139A (with H. Rahan).
Fettes, E. M.; June, 249A.
Fiedler, G.; June, 252A (with G. A. Chisnall).
Fishes of the Western North Atlantic (Y. Olsen, ed.); Dec., 482A.
Flame Spectroscopy (R. Mavrodineanu & H. Boiteux); Sept., 378A. Parts I–III.
Florkin, M., ed.; March, 129A (with E. H. Stotz).
Fluvial Processes in Geomorphology (L. B. Leopold *et al.*); Sept., 385A.
Fong, P.; Sept., 375A.
Foundations of Electrical Engineering: Vol. 3, *Fields-Networks-Waves* (K. Simonyi; D. Jones & A. Sekey, trans.); March, 68A.
Foundations of Experimental Embryology (B. H. Willier & J. M. Oppenheimer, eds.); March, 98A.
Foundations of Thermodynamics (P. Fong); Sept., 375A.
Fouraker, L. E.; June, 260A.
Fracture Processes in Polymeric Solids: Phenomena & Theory (B. Rosen, ed.); June, 247A.
Frameworks for Dating Fossil Man (K. Oakley); Sept., 365A.
Francombe, M. H., ed.; June, 239A (with H. Sato).
Franklin, K. L.; Sept., 330A.
Friedel-Crafts & Related Reactions, Vol. 3, Parts I & II: *Acylation & Related Reactions* (G. A. Olah, ed.); Dec., 505A.
Friedel, J.; June, 253A.
Frisch, O. R., ed.; Sept., 348A.
Frocht, M. M., ed.; March, 86A.
Fuchs, B. A.; March, 129A (with B. V. Shabat).
Function and Structure in Micro-organism (M. R. Pollock & M. H. Richmond, eds.): Dec., 504A.
Functions of a Complex Variable & Some of Their Applications Vol. I (B. A. Fuchs & B. V. Shabat); March, 129A.
Fundamental Astrometry . . . (V. V. Podobed; A. N. Vyssotsky, ed.); Dec., 470A.
Fundamentals of Engineering Mechanics (L. Levinson; S. Klein, trans.); Dec., 507A.
Future of Fuel Technology, The (G. N. Critchley, ed.); Dec., 492A.
Future of Man, The (T. de Chardin); June, 216A.

Gale, S. I.; Sept., 330A.
Gas Phase Oxidation of Hydrocarbons, The (V. Ya. Shtern); March, 112A.
Geiringer, H., ed., Sept., 390A.
Gene Action (P. E. Hartman & S. R. Suskind); Sept., 371A.
Genetics & Man (C. D. Darlington); June, 200A.
Genetics of the Silkworm, The (Y. Tazima); June, 218A.
General College Geology (A. J. Eardley); Sept., 328A.
Geochemistry of Sediments, A Brief Survey (E. T. Degens); Dec., 490A.
Geography of Japan (R. Isida); March, 84A.
Geohydrology (R. J. M. De Wiest); Dec., 494A.
Geology of Japan (F. Takai *et al.*, eds.); March, 84A.
George, W.; March, 82A.
Girifalco, L. A.; Sept., 346A.
Give & Take: The Development of Tissue Transplantation (F. D. Moore); March, 108A.
Gleason, H. A.; Sept., 372A (with A. Cronquist).
Gluckman, M., ed.; Dec., 493A.
Godula, E. A., ed.; March, 135A (with E. J. Lawson).
Goldberger, M. L.; March, 126A (with K. M. Watson).
Goldstein, A.; June, 252A.
Golley, F. B.; March, 98A (with D. E. Davis).
Golomb, S. W.; Sept., 356A.
Goss, R. J.; Dec., 507A.
Gould, R. F., ed.; March, 135A.
Gowenlock, B. G.; Sept., 339A (with H. Melville).
Graphics in Space Flight (F. A. Heacock); March, 133A.
Gravitation & Relativity (H.-Y. Chiu & W. F. Hoffmann, eds.) June, 214A.
Gray, D. F.; Dec., 468A.
Green, H. S.; Sept., 362A (with C. A. Hurst).
Groups of Order 2^n ($n \leq 6$), The (M. Hall, Jr., & J. K. Senior); June, 230A.
Group Theory & Quantum Mechanics (M. Tinkham); March, 119A.
Grunbaum, A.; March, 76A.
Guide to the Interpretation of Space Group Symbols (S. K. Dickinson, Jr.); Dec., 488A.
Gurland, J., ed.; June, 254A.
Gurr, E.; Dec., 506A.

Haggis, G. H., *et al.*; March, 105A.
Hahon, N., ed.; March, 110A.
Hais, I. M., ed.; June, 258A (with K. Macek).
Hall, M. Jr.; June, 230A (with J. K. Senior).
Hall, R. P.; Dec., 486A.
Handbook of Applied Hydrology: A Compendium of Water-Resources Technology (V. T. Chow, ed.); Sept., 369A.
Handbook of Cell & Organ Culture (D. J. Merchant *et al.*. eds.); March, 123A.
Handbook of Paleontological Techniques (B. Kummel & D. Raup, eds.); Dec., 509A.
Handbook of Physiology, Section 3: *Respiration*, Vol. I (W. O. Fenn & H. Rahn, sections eds.); March, 139A.
Handbook of Practical Amateur Astronomy, A (P. Moore, ed.); June, 251A.
Hanson, R. P., ed.; June, 235A.
Harker, J. E.; Sept., 342A.
Harriott, P.; March, 137A.
Harris, V. A.; Sept., 376A.
Hartman, P. E.; Sept., 371A (with S. R. Suskind).
Harvey, D. G.; Sept., 398A (with W. R. Corliss).
Hass, G., ed.; Dec., 484A (with R. E. Thun).
Hatsopoulos, G. N.; Dec., 500A (with J. H. Keenan).
Hawkins, G. S.; Dec., 512A.
Heacock, F. A.; March, 133A.
Heat & Life: The Development of the Theory of Animal Heat (E. Mendelsohn); March, 107A.
Hecker, R. F.; Dec., 498A.
Heizer, R. H.; Sept., 338A (with F. Hole).
Hendrickson, J. B.; Dec., 499A (with J. H. Richards).
Hermann, H., Sept., 377A (with R. Mornex).
Herschel, William, & the Construction of the Heavens (M. A. Hoskins); June, 246A.
Hess, F. C.; Sept., 330A.
Heukelekian, H., ed.; Sept., 357A (with N. C. Dondero).
Heywood, V. H.; June, 212A (with P. H. Davis).
Hille, E.; Sept., 362A.
Hindle, B.; June, 206A.
Histones & Other Nuclear Proteins (H. Busch); Dec., 470A.
Hodge, P.; June, 220A (with J. C. Brandt).
Hoffman, J. F., ed.; Sept., 389A.
Hoffmann, W. F., ed.; June, 214A (with H.-Y. Chiu).
Hofstadter, R., ed.; March, 104A (with L. I. Schiff).
Hoiberg, A. J., ed.; June, 250A.
Hole, F.; Sept., 338A (with R. H. Heizer).
Holmes, A.; Dec., 488A.
Homer, W. I.; Sept., 330A.
Hoskin, M. A.; June, 246A.
Hoyle, F.; June, 201A.
Human Diversity: The Nature & Significance of Differences among Men (K. Mather); Dec., 509A.
Human Engineering Guide for Equipment Designers (W. E. Woodson & D. W. Conover); June, 228A.
Human Genetics & Its Foundations (M. Whittinghill); Sept., 331A.
Human Motivation: A Symposium (M. R. Jones, ed.); Dec., 494A.
Human Tumors Secreting Catecholamines (H. Hermann & R. Mornex); Sept., 377A.
Hurst, C. A.; Sept., 362A (with H. S. Green).
Hutchinson, J., ed.; Dec., 464A.
Huxley, A.; March, 78A.

Iconography of the Characae (R. D. Wood & K. Imahori); Sept., 392.
Identification of Materials, Via Physical Properties, Chemical Tests, & Microscopy (A. A. Benedetti-Pichler); March, 90A.
Imahori, K. M.; Sept., 392A (with R. D. Wood).
Imbrie, J., ed.; Sept., 352A (with N. D. Newell).
Immunology (D. F. Gray); Dec., 468A.
Inference & Disputed Authorship: "The Federalist" (F. Mosteller & D. L. Wallace); March, 104A.
Ingram, V. M.; Sept., 395A.
Inhelder, B.; March, 128A (with J. Piaget).
Innes, J. R. M., ed.; March, 87A (with W. Zeman).
Internal Factors in Evolution (L. L. Whyte); June, 198A.
Intersexuality in Vertebrates including Man (C. N. Armstrong & A. J. Marshall, eds.); June, 257A.
Introduction to Cell Differentiation (N. T. Spratt, Jr.); March 94A.

Introduction to General Relativity (R. Adler, *et al.*); Sept., 334A.
Introduction to Genetics, A Program for Self-Instruction (E. J. Kormondy); March, 115A.
Introduction to Infrared & Raman Spectroscopy (N. B. Colthup *et al.*): June, 252A.
Introduction to Molecular Biology (G. H. Haggis *et al.*); March, 105A.
Introduction to Paleoecology (R. F. Hecker); Dec., 498A.
Introduction to Prehistoric Archaeology, An (F. Hole & R. H. Heizer); Sept., 338A.
Introduction to Properties of Materials (D. Rosenthal); June, 245A.
Introduction to Radiation Chemistry, An (J. W. T. Spinks & R. J. Woods); June, 246A.
Introduction to Semiconductor Devices (M. J. Morant); Dec., 508A.
Introduction to Semiconductor Physics (R. B. Adler *et al.*); June, 260A.
Irving, E.; Sept., 352A.
Isida, R.; March, 84A.
Isotopes in Biology (G. Wolf); June, 250A.

Jagersten, G.; March, 86A (with L. Nilsson).
Japanese People, The: Origins of the People & the Language (I. Komatsu); March, 82A.
Jinks, J. L.; March, 125A.
John, B.; June, 238A (with K. R. Lewis).
Johnson, R.; June, 259A (with F. Basolo).
Jones, M. M.; Sept., 383A.
Jones, M. R., ed.; Dec., 494A.

Kabir, P. K., ed.; Sept., 339A.
Kaempffer, F. A.; Dec., 474A.
Kamen, M. D.; March, 71A.
Kaplan, A.; June, 246A.
Kaplan, B.; March, 141A (with H. Werner).
Kaplan, M.; Sept., 370A.
Karling, J. S.; March, 102A.
Kay, M.; Dec., 464A.
Keenan, J. H.; Dec., 500A (with G. Hatsopoulos).
Kepes, G., ed.; Sept., 392A.
King, H. C.; Dec., 511A.
Kinsman, B.; Dec., 486A.
Kirmse, W., *et al.*; Sept., 350A.
Knight, C. B.; Dec., 480A.
Kolthoff, I. M., ed.; March, 130A (with P. J. Elving).
Komatsu, I.; March, 82A.
Kormondy, E. J.; March, 115A.
Korn, G. A.; June, 253A.
Korn, T. M.; June, 253A.
Kozyrev, B. M.; June, 221A (with S. A. Al'tshuler).
Kronig, R.; June, 226A (with V. F. Weisskopf).
Kuiper, G. P., ed.; Sept., 387A (3).
Kummel, B., ed.; Dec., 509A (with D. Raup).

Lam, S. H.; Sept., 360A (with W. H. Li).
Lane, A. M.; March, 71A.
Lawson, E. J., ed.; March, 135A (with E. A. Godula).
Leakey, L. S. B. *et al.;* Sept. 334A.
Lectures on Magnetoionic Theory (K. G. Budden); March, 143A.
LeGalley, D. P., ed.; June, 238A (with A. Rosen); 248A (with J. W. McKee).
Lehninger, A. L.; June, 224A.
Leisure in America: A Social Inquiry (M. Kaplan); Sept., 370A.
Life, eds; et al., Sept., 376A
Life in the Sea (G. Jägersten); March, 86A.
Leone, C. A., ed.; Sept., 350A.
Leopold. A. C.; Sept., 366A.
Leopold, L. B. *et al.;* Sept., 385A.
Lepoutre, G., ed.; Sept., 356A (with M. J. Sienko).
Leprince-Ringuet, L., ed.; Sept., 374A.
Levine, S. N.; Dec., 500A.
Levinson, L.; Dec., 507A (trans. by S. Klein).
Lewis, K. R.; June, 238A (with B. John).
Li, W. H.; Sept., 360A (with S. H. Lam).
Life of the Cell, The: Its Nature, Origin, & Development (J. A. V. Butler); June, 226A.
Life in the Sea (G. Jagersten & L. Nilsson); March, 86A.
Life of the Rainbow Lizard, The (V. A. Harris); Sept., 376A.
Light (R. W. Ditchburn, ed.); Dec., 466A.
Lindstrom, M.; Dec., 502A.
Lipsitt, L. P., ed.; June, 244A (with C. C. Spiker).
Lipsitt, L. P., ed.; Dec., 482A (with C. C. Spiker).
Literature & Science (A. Huxley); March, 78A.
Longo, V. G., ed.; Sept., 384A (with M. Ya. Mikhel'son).
Loughhead, R. E.; Dec., 462A.
Lowdin, P.-O., ed.; March, 116A (with B. Pullman).
Lowenthal, H., ed., Sept., 364A.
Luce, R. D. *et al.*, eds.; March, 132A.
Lumley, J. L.; Sept., 344A (with H. A. Panofsky).
Lunar Charts, G. P. Kuiper *et al.*, Sept., 386A.

Macek, K., ed.; June, 258A (with I. M. Hais).
Macromolecular Structure of Ribonucleic Acids (A. S. Spirin; J. A. Stekol, trans. ed.); Sept., 367A.
Madelung, O.; March, 121A.
Madorsky, S. L., June, 237A.
Mammalian Protein Metabolism, Vol. I (M. Munro & J. B. Allison, eds.); June, 255A.
Mammals of the World, 2 vols. (E. P. Walker *et al.*); Dec., 476A.
Management of Wild Mammals in Captivity, The (L. S. Crandall); March, 92A.
Man & Nature Or, Physical Geography as Modified by Human Action (G. P. Marsh; D. Lowenthal, ed.); Sept., 364A.
Man in Space (F. C. Hess); Sept., 330A.
Mann, T.; March, 113A.
Manual of Experimental Electrophysiology (I. C. Whitfield); March, 92A.
Margenau, H.; March, 87A (with G. M. Murphy).
Marine Bio-Acoustics (W. N. Tavolga, ed.); March, 124A.
Marine Geology of the Gulf of California (T. H. van Andel & G. G. Shor, Jr., eds.); March, 122A.
Markov Processes, Vols. I & II (E. B. Dynkin; J. Fabius *et al.*, trans.); Dec., 497A.
Markstein, G. H., ed.; March, 90A.
Mars (R. S. Richardson with C. Bonestell); Dec., 478A.
Marsh, G. P.; Sept., 364A.
Marshall, A. J., ed.; June, 257A (with C. N. Armstrong).
Marshall, C. E.; June, 242A.
Mason, W. P., ed.; June, 230A.
Mathematical Theory of Probability & Statistics (R. von Mises; H. Geiringer, ed.); Sept., 390A.
Mathematics: Its Content, Methods, & Meaning (A. D. Aleksandrov, *et al.*, eds.); Sept. 374A.

Mather, K.; Dec., 509A.
Mathematics & Psychology (G. A. Miller); March, 132A.
Mathematics of Physics & Chemistry, The, Vol. 2 (H. Margenau & G. M. Murphy); March, 87A.
Matter of Mendelian Heredity, The (K. R. Lewis & B. John); June, 238A.
Mavrodineanu, R.; Sept., 378A.
McDaniel, E. W.; March, 68A.
McHenry, E. W., ed.; Sept., 386A (with G. H. Beaton).
McKee, J. W., ed.; June, 248A (with D. P. LeGalley).
Mechanics of Inheritance, The (F. W. Stahl); March, 118A.
Mellor, D. P., ed.; June, 210A (with F. P. Dwyer).
Melville, H.; Sept., 339A (with B. G. Gowenlock).
Mendelsohn, E.; March, 107A.
Merchant, D. J., *et al.*; March, 123A.
Messel, H.; June, 204A (with S. T. Butler); 3.
Metal-Ammonia Solutions, Physicochemical Properties (G. Lepoutre & M. J. Sienko, eds.); Sept., 356A.
Metalorganic Polymers (K. A. Andrianov); Dec., 496A.
Meteors, Comets, & Meteorites (G. S. Hawkins); Dec., 512A.
Methods in Carbohydrate Chemistry, Vol. 4 (R. L. Whistler, ed.); June, 240A.
Meynell, G. G.; Dec., 495A. (with E. Meynell).
Meynell, E.; Dec., 495A. (with G. G. Meynell).
Mikhel'son, M. Ya., ed.; Sept., 384A (with V. G. Longo).
Miller, G. A.; March, 132A.
Mitochondrion, The; Molecular Basis of Structure & Function (A. L. Lehninger); June, 224A.
Molecular Orbitals in Chemistry, Physics, & Biology, A Tribute to R. S. Mulliken (P.-O. Löwdin & B. Pullman, eds.); March, 116A.
Moore, F. D.; March, 108A.
Moore, P.; June, 251A.
Morant, M. J.; Dec., 508A.
Mornex, R.; Sept., 377A (with H. Hermann).
Morowitz, H. J., ed.; Sept., 338A (with T. H. Waterman).
Mortimer, C. T.; June, 208A.
Mosteller, F.; March, 140A (with D. L. Wallace).
Motivation: Theory & Research (C. N. Cofer & M. H. Appley); June, 236A.
Mulliken, R. S., March 116A.
Multivariate Statistical Analysis for Biologists (H. Seal); Sept., 372A.
Munro, H., ed.; June, 255A (with J. B. Allison).
Murphy, G. M.; March, 87A (with H. Margenau).

National Academy of Sciences; Sept., 328A.
Natural Geography of Plants, The (H. A. Gleason & A. Cronquist); Sept., 372A.
Naturalistic Behavior of Nonhuman Primates (C. R. Carpenter); Dec., 468A.
Nature & Art of Motion, The (*Vision and Value* series) (G. Kepes, ed.); Sept., 392A.
Neiman, M. B., ed.; Dec., 462A.
Neustadt, L. W., ed.; June, 262A (with A. V. Balakrishnan).
Newcastle Disease Virus, An Evolving Pathogen (R. P. Hanson, ed.); June, 235A.
Newell, N. D., ed.; Sept., 352A (with J. Imbrie).
N. Y. Heart Association; June, 242A.
Nicholson, T. D.; Sept., 330A.
Nilsson, L.; March, 86A (with G. Jagersten).
Non-steady Flame Propagation, Agardograph 75 (G. H. Markstein, ed.); March, 90A.
Noyes, W. A. *et al.*, eds.; June, 232A.
Nuclear Interactions (S. DeBenedetti); Sept., 380A.
Nuclear Theory (A. M. Lane); March, 71A.
Nuclei & Radioactivity (G. Choppin); March, 131A.
Nucleon Structure (R. Hofstadter & L. I. Schiff, eds.); March, 104A.
Nutrition, A Comprehensive Treatise, Vol. II: *Vitamins, Nutrient Requirements, & Food Selection* (G. H. Beaton & E. W. McHenry, eds.); Sept., 386A.
Nutritional Factors in Virus Formation (K. Yamafuji); June, 240A.

Oakley, K.; Sept., 365A.
Of Men & Galaxies (F. Hoyle): June, 201A.
Of Stars & Men (*The Human Response to an Expanding Universe*) (H. Shapley); June, 288.
Okamoto, Y.; Sept., 354A (with W. Brenner).
Olah, G. A., ed.; Dec., 505A.
Olduvai Gorge 1951-1961, Vol. I: *Fauna & Background* (L. S. B. Leakey *et al.*); Sept., 334A.
Olivier, J. P.; March, 104A (with S. Ross).
Olsen, Y., ed.; Dec., 482A.
On Physical Adsorption (S. Ross & J. P. Olivier); March, 104A.
Oppenheimer, J. M.; March, 98A (with B. H. Willier).
Optoelectronic Devices & Circuits (S. Weber); June, 234A.
Orchids of the Western Great Lakes Region (F. W. Case, Jr.); Sept., 361A.
Order-Disorder Phenomena, Vol. 5: *Monographs in Statistical Physics* (H. S. Green & C. A. Hurst); Sept., 362A.
Organic Semiconductors (Y. Okamoto & W. Brenner); Sept., 354A.
Organic Syntheses, Vol. 44 (W. E. Parham, ed.); June, 225A.
Origin & Evolution of Atmospheres & Oceans, The (P. J. Brancazio & A. G. W. Cameron, eds.); Sept., 391A.
Orthographic Atlas of the Moon; supplements to *Photographic Lunar Atlas* (G. P. Kuiper, ed.; compiled by D. W. G. Arthur & E. A. Whitaker); Sept., 387A.
Oxidation Mechanisms: Applications to Organic Chemistry (R. Stewart); March, 116A.

Paleomagnetism . . . (E. Irving); Sept., 352A.
Palladin, A. V., ed.; March, 112A.
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Paper Chromatography, A Comprehensive Treatise, 3 Vols. (I. M. Hais & K. Macek, *et al.*, eds.); June, 258A.
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